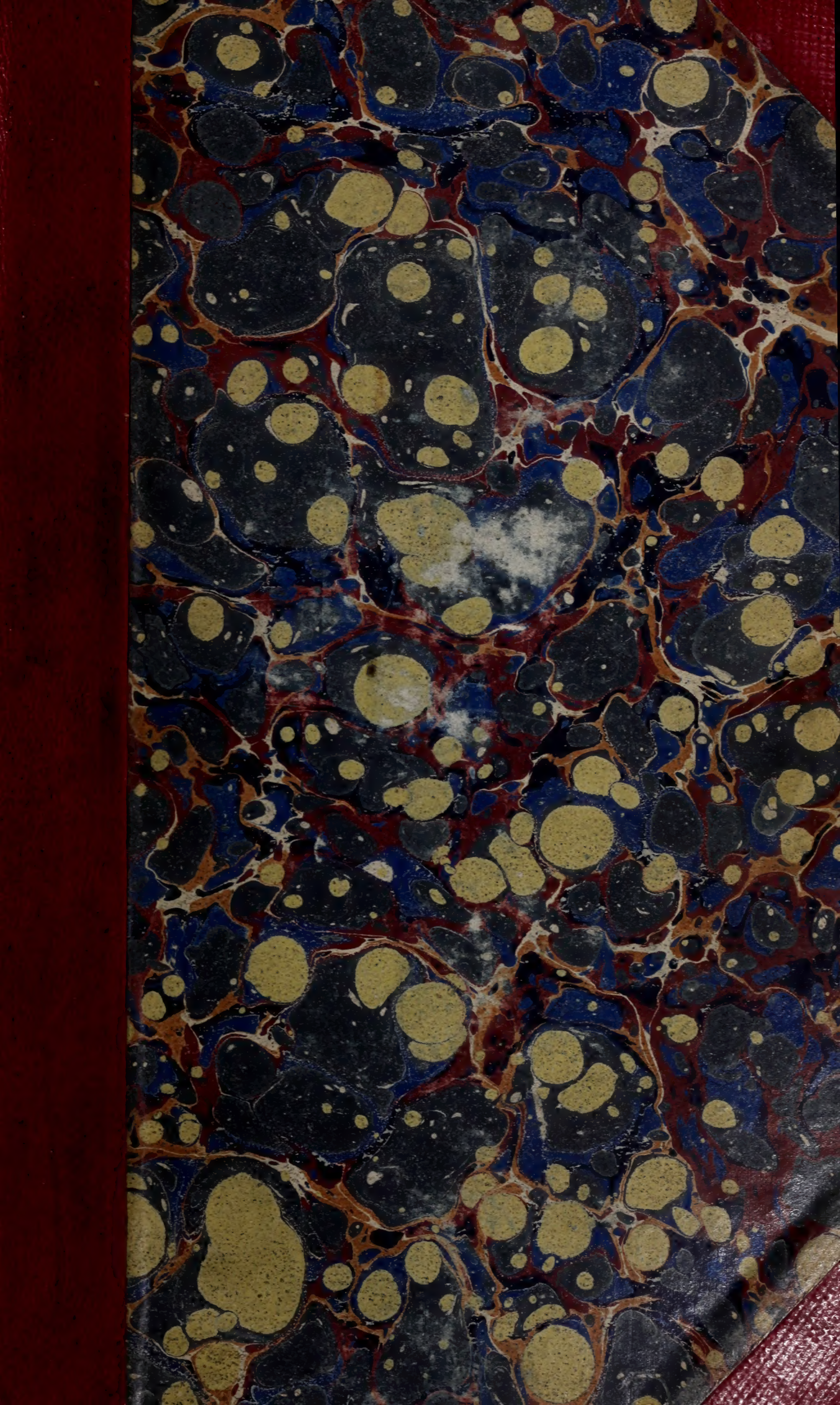

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NEW ORLEANS

Medical News and Hospital Gazette:

A Monthly Journal,

EDITED BY

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NEW ORLEANS

MEDICAL NEWS AND HOSPITAL GAZETTE.

VOL. V. MARCH, 1858. NO. I.

RESPIRATION.

Synopsis of a Lecture delivered at the New Orleans School of Medicine, by ANTHONY PENISTON, M. D., Professor of Physiology.

The application of modern discoveries in chemistry, to the investigation of physiological phenomena, has contributed in a great measure to that accurate and positive method which is now characteristic of the science. Therefore it is that the names of Lavoisier, Dumas, Liebig and numerous other great authorities in chemical science, are quite as illustrious by their invaluable contributions to the science of life as to the field of pure chemistry, even comprising its innumerable applications to the arts and manufactures. In the words of the philosopher of Giessen: "the last and most elevated object of chemistry is the investigation of the causes of natural phenomena, and their associations, and of those factors which are common to different series of phenomena. The chemist ascertains the laws which regulate natural phenomena, and by combining together all that is observable and has been observed by the senses, he at last attains to a general intellectual expression for the phenomena, in other words, to a *theory*. The physician who has learned medicine, not as a science, but as an empirical art, acknowledges no principles, but only *rules* derived from experience. The object of his inquiries is only whether a remedy, in any given case, had a good or a bad effect. This is all the empiric cares about. He never asks *why?* He never inquires into the *causes* of what he observes." How tedious the study of the medical profession when prosecuted in this manner; how unsatisfactory its practice! But if we seek in chemistry an explanation of the phenomena which take place in the animal economy, let us beware how we liken all those processes to purely chemical reactions! The manifestations of life are made up of two kinds of laws; some which are purely material, like digestive absorption, or excretion; others which include vital powers, of a more elevated character, whose essence is not as yet susceptible of explanation by any of the known properties of inert matter: such are the process of interstitial nutrition of tissues, the powers of nerve cells, the functions of reproduction, etc.

We cannot follow to their ultimate destination all the elements which enter the body in the shape of food; but we do know most of the metamorphoses which are successively undergone by its various constituents, prior to those of its ultimate purposes. The products of digestion, after they have been prepared for absorption, are introduced into the general circulation, whence they carry to every organ and every tissue, the materials necessary for their repair and maintenance.

Two conditions are therefore necessary to life: a due supply of food, and the requisite amount of oxygen taken from the atmospheric air by the process of respiration. "Life," says an eloquent Professor,* "far from being a condition of immobility is a condition of ceaseless change. An organism, of no matter what grade it may be, is only a temporary form, which myriads of particles passing through a determinate career give rise to. It is like the flame of a lamp, which presents for a long time the same aspect, being ceaselessly fed, as it ceaselessly wastes away. What then is man?—a mechanism, the parts of which are unceasingly taken asunder and as unceasingly replaced. The appearance of corporeal identity he presents year after year is only an illusion. He begins to die the moment he begins to breathe. Interstitial death is the condition of interstitial removal; being fed with more than a ton of material in a year, he, in the same time wastes more than a ton away." The processes of absorption and assimilation are the means by which the elements of food are prepared for nutrition; and as, in the healthy condition, the weight of an adult scarcely varies from day to day, it follows that certain processes are required to remove from the system an amount of material equivalent to that which is taken into the system. The ordinary amount of fæces passed daily by men in health is about five or six ounces, and if we assume thirty-five ounces as about the average quantity of food taken in twenty-four hours, it may be inferred that at least thirty ounces are appropriated to the various purposes of the economy. It is also evident that this amount being consumed without any increase of weight on the part of the consumer, it must be accompanied by a removal of a corresponding amount from the system; the means by which the condition of equilibrium is maintained, are the lungs, the skin and the kidneys, including the functions of respiration, perspiration and urinary secretion. Some very accurate experiments made by Mr. Barral upon himself, during summer and winter, and arranged in tabular form, will show the proportion of one of the elements of food thus eliminated in the course of twenty-four hours.

---

* Dr. Draper, of New York.

	Weight of Body.	Carbon in Food.	Carbon execreted.		
			In Fœces.	In Urine.	Exhaled.
	lbs.	grs.	grs.	grs.	grs.
In Summer.....	104.5	5654.1	236.2	234.6	5183.3
In winter......	"	4090.0	137.6	211.5	3741.1

It thus appears that a large proportion of one of the most essential elements of food finds its way out of the system through the skin, or a modification of it, viz., the lungs. Respiration is that function by which an interchange of gases takes place between the interior of an organized being and the external medium; the result of which is that oxygen is taken in, and carbonic acid given off. The organs of respiration are the lungs; a most admirable contrivance by which the greatest amount of blood is brought into contact with the necessary quantity of atmospheric air, through the intermedium of exceedingly thin membranes. The air vesicles, which constitute the ultimate ramifications of the bronchi, are composed of minute polygonal cells, having from 1–1600th to 1–2205th of an inch in diameter, and about 1–3500th of an inch in thickness. It has been calculated that as many as 17,790 air cells are grouped around each terminal bronchus; and that their total number amounts to six hundred millions. The capillary net-work disposed between the continuous layers of two adjacent air cells, is so extremely minute in diameter, that it is said the blood dics can only move in single files, and thus each corpuscle as it passes along, in rapid flight, receives nevertheless its proportion of oxygen, and relinquishes its carbonic acid. According to Valentin, the blood is thus in contact with the air during a second and a half at each circuit. As a consequence of the circulation through the lungs, dark venous blood is changed into arterial, by means of the diffusion of gases, and thus the noxious effects of the retention of carbonic acid in the system are at once obviated. In order that fresh supplies of atmospheric air should be continually renewed in the air passages, the lungs are inclosed in an osseous frame work endowed with the power of alternate dilatation and contraction: the mechanism by which this is accomplished is so exceedingly simple, perfect and beautiful, that it should deserve our attention on that score alone, independently of its connection with physiology and pathology.

The action of the lungs in inspiration and expiration, may be compared to that of a bellows; the active force of dilation and compression is derived from the different muscles, which from various points of attachment, act as levers to increase or diminish the capacity of the thorax. The chest, composed of the ribs, vertebral column and sternum, is a movable framework; susceptible of increasing its diameters, vertically, antero-posteriorly, and laterally, for the ribs are connected with the vertebral column by a ginglymoid articulation, at the same time that their cartilaginous adhesion to the sternum, admits considerable motion in every direction. In the act of inspiration the ribs are projected forwards and rotated upon their own axis in such a way that the lateral diameters are materially increased; on the other hand, the contraction of the diaphragm tends to increase the vertical diameter of the chest. "It may be concluded," say Messrs. Todd and Bowman, "that a person whose full capacity is 280 cubic inches, contains in his lungs, after an ordinary expiration, about 110 cubic inches; and after an ordinary inspiration, about 140 cubic inches: a quantity which he is enabled to double by a *full* inspiration. Thus rather more than one-fifth of the ordinary contents of the lungs are expelled at each expiration, and again renewed by inspiration.

The great inspiratory muscle, in fact about the sole agent in tranquil respiration, is the diaphragm. The form of this peculiar muscle, with its great convexity turned towards the thorax, and which by contracting it tends to convert into a horizontal plane, at once explains the mode in which it increases the vertical diameter of the chest. The principal other respiratory muscles are the scaleni and external intercostals, assisted by the serrati and numerous other muscles in forced inspiration. On the other hand, the thoracic expiratory muscles are the internal intercostals, and a portion of the triangularis sterni. The expiratory movement is also assisted by the abdominal muscles which antagonise the diaphragm, by pressing upwards the contents of the abdomen, thereby diminishing the capacity of the thorax. In cases of extreme dyspnœa many other muscles are called into action; therefore it is that patients instinctively grasp some fixed object;

for the scapula being thus rendered immovable, becomes a fixed point from which the pectoralis and serratus magnus can aid in elevating the ribs. According to Dr. Hutchinson, the expiratory power exceeds the inspiratory by one-third, although the contrary opinion is generally credited. It appears, however, that the mode of life exercises a considerable influence on the development of the expiratory muscles, and that wrestlers, boxers, and other men of that class, who are accustomed to make violent efforts, by compressing the thorax in order to render it a fixed point of attachment for different muscles, attain a most unusual power of expiration, which may exceed as much as four times the natural inspiratory force.

We now come to the physico-chemical phenomena of respiration. As a result of the mechanical arrangements above described, a certain quantity of atmospheric air penetrates into the lungs at each inspiration, and a certain amount is given out at the corresponding movement of expiration. But between the air coming in, and that given out, there are certain differences in their constituent elements which are characteristic of the process, and which result from some important changes in the composition of the blood. In order, better to understand the phenomena of respiration, we must remember that atmospheric air consists of a mixture of oxygen and nitrogen in proportions which scarcely vary on the surface of the globe; being very nearly the same in valley and plain, in city and country. It contains also very small quantities of carbonic acid gas, and variable proportions of watery vapor, together with other gaseous and volatile substances, in scarcely appreciable quantities, having no immediate relation to the respiratory function. The respective proportions of these gases in 100 parts by weight of atmospheric air are 76.9, of nitrogen; 23.1 of oxygen.

The proportions in volume are somewhat different, but are easily found by calculating the known densities of the respective gases; the result of which is, that 100 parts, by measure of atmospheric air, contains 20.9 of oxygen and 79.1 of nitrogen. As to the quantity of carbonic acid in the atmosphere, it exists in the small proportion of from 4 to 6 parts in 10,000 parts of atmospheric air. If, from these, the normal proportions of the air we breathe, we pass to an analysis of air expired from the lungs, it will be found that the quantity of moisture has nearly reached the point of saturation, the proportion of oxygen has diminished, and that of carbonic acid has increased, while that of nitrogen has slightly increased, or has undergone little change. On the authority of Valentin and Brunner, it may be stated that 100 parts of expired air contain 4.30 parts of carbonic acid. Now, taking 30 cubic inches as the volume of each expiration, the actual quantity of carbonic acid in each will be 1.29 cubic inches; equal to about 23 cubic inches per minute, or about 1393 cubic inches per hour, or 16.1 cubic feet per day. In this last quantity of carbonic acid there are about seven and a half ounces by weight of carbon. On the other hand, the quantity of oxygen introduced into the system at the same time is slightly greater than the carbonic acid given out; so that there is not an exact compensation between these gaseous exchanges. Whence comes this excess in the absorption of oxygen? We know that a great source of animal heat is the combination of the carbon of the tissues with the oxygen of the blood. On the other hand, chemistry teaches us that a given volume of oxygen employed to burn a piece of charcoal, gives rise to an equal volume of carbonic acid; it therefore appears that a part of the oxygen absorbed by the lungs, is not directly transformed into carbonic acid, but is employed in oxydizing other substances, such as sulphur, phosphorus and hydrogen, which are eliminated by the urine in the shape of sulphates, phosphates and water. The formation of carbonic acid, and the elimination from the system, vary according to the states of the system, the periods of digestion, of exercise, and the quality of the food. Messrs. Dulong and Despretz have shown that animals fed on flesh absorb more oxygen in proportion than those fed on vegetable diet. To account for these diversities, we must bear in mind the different composition of sugar, starch and the various compounds of animal flesh, the last generally containing a large proportion of oily matter. In sugar and starch the hydrogen is already provided with its equivalent of oxygen, and the carbonic acid generated by their combustion, will contain the whole bulk of the oxygen consumed. Not so when the material is fat, for then the oxygen contained in the carbonic acid that is generated will be only about 72 per cent. of the whole amount which disappears, the remainder being appropriated by the surplus hydrogen to form water.

The temperature of the surrounding medium also exercises an important influence on the exhalation of carbonic acid. In summer, the air is warmer, less dense, and contains less oxygen in a given volume than in a colder season. It appears, that certain substances introduced into the circulation, exercise a considerable influence on the development of this gas; thus, alcohol, strong tea or coffee, and the anasthetic agents diminish very greatly the quantity of carbonic acid excreted in a given time.

The accumulation of carbonic acid in the former case is explained by the fact that alcohol soon after its introduction into the blood is transformed into aldehyde, which can be recognized by its peculiar aromatic odor. Aldehyde is an exceedingly combustible substance, remarkable for its affinity for oxygen. It differs from alcohol in having two equivalents less of hydrogen. This substance is therefore much more combustible than any of the elements of the blood, and absorbs a large proportion of the oxygen introduced into the blood. The products of its combustion are likewise carbonic acid and water, but the proportion of water resulting from its oxydation is much greater than from any of the other hydro-carbons, such as sugar and fat. During the time necessary for the combustion and elimination of this aldehyde, it is evident that the other materials of the blood, which are intended to supply the formation of carbonic acid, are not called into requisition, but are deposited in the tissues in the shape of fat. This explains the fatty degeneration of the liver, and the accumulation of fat, which is well known to be one of the most frequent results of long continued use of alcoholic drinks.

From the fact that about four or five hundred cubic feet of air pass through the lungs daily, it becomes evident how important a consideration is a due attention to proper ventilation. From the repeated passage of the same air through the lungs, though it may have been originally pure and wholesome, it will soon become impregnated with carbonic acid, and be rendered unfit for the respiratory process. There are many cases on record in which death has supervened, from a number of persons being confined in apartments where a sufficient quantity of pure air was not constantly renewed.

We have now to consider the changes in the blood resulting from respiration. The most important modification is that which characterizes the change from venous to arterial; the absorption of oxygen and elimination of carbonic acid. Much difference of opinion still exists as to the exact mechanism by which this change is brought about. In the first place, the best authorities are not entirely agreed on the physical constitution of the red corpuscles of the blood, which are admitted to be the carriers of oxygen. According to Robin, the blood corpuscles are cells without any cavity distinct from the cell wall; in other words, it is a homogeneous mass, made up of globuline, an albuminous substance, united atom to atom, molecule to molecule, with hematine or the coloring matter of the blood. On the other hand, Lehmann speaks of the capsule of the blood corpuscle, of a thickening or attenuation of the investing membrane, and of a pigment or coloring matter shining through this envelope. The coloring matter of the .blood or hematine, is an organic substance, containing about seven per cent. of iron. Liebig adopted the highly ingenious theory that the red corpuscles are carriers of ogygen, and that they attract and retain that principle by means of their coloring matter. According to him, the property of attracting oxygen is due to the iron contained in the hematine; that in venous blood, this iron is in the state of the carbonate of the protoxide of iron, but that on being exposed to the air, it absorbs oxygen and is converted into the hydrated per-oxyde of iron.

According to Henle, Nasse, and other distinguished hœmatologists, the cause of the difference in color is rather to be found in the *form* of the corpuscles, affecting their power of transmitting and reflecting light, than in any chemical changes in the *contents* of the blood eells. If we dilute blood with water it assumes a dark red color, and then, under the microscope, the blood corpuscles are evidently distended, they loose their discoid form and become spherical; the blood, collectively, must therefore appear darker, since each individual corpuscle has become converted into a spherical mirror, from which the red rays are scattered and reflected. We observe the reverse on treating the blood with such substances as render the intercellular fluid relatively denser, there is then established an outward current from the interior of the cell, in consequence of which it collapses, the central depression becomes more considerable, and the individual cells resemble concave mirrors, from which the red rays are reflected with greater intensity. Scherer indicates another physical cause, viz.: the attenuation or thickening of the investing membrane. When the capsule becomes thinner by the expansion of the blood corpuscles, the pigment shines through more readily, producing a dark red appearance to the whole mass; when, on the contrary, the corpuscles are diminished, their capsules are thrown into folds, concealing the true color of the hœmatine, and this imparts the brighter color to the blood.

But Lehmann, who has given, at great length, all the opinions on this subject, says that it would be erroneous to attribute the different coloring of the blood solely to mechanical circumstances, and that chemical influences effect changes in the hue of the blood pigment itself, especially oxygen and carbonic acid.

It is therefore, perhaps, the soundest course to adopt the opinion of Robin, Beclard and others, who maintain the alternate chemical combinations, feeble though they be, of oxygen and carbonic acid, with the hœmatine or coloring matter of the blood corpuscles, as the cause of the change in coloration. In favor of this theory, we may cite an experiment by which Bernard proved that carbonic oxide gas, a highly poisonous substance, when introduced into the lungs, enters into permanent chemical combination with the blood, imparts to it a bright red color, and that afterwards the change from arterial to venous can no longer take place. It would seem as if the blood corpuscles under the influence of a strong chemical affinity, could no longer take on those other evanescent gaseous combinations which are necessary to life. Nor can the change in color, by means of carbonic oxyde, be imputed to mechanical alterations of form in the blood corpuscles, for under the microscope they have exactly the same appearance as in normal blood. The poisonous effects of carbonic oxyde differ from those of carbonic acid, in this: that the former prevents that elimination of carbonic acid which is continually produced in the system; the latter causes asphyxia by preventing the absorption of oxygen. It is something of a mooted question, the state in which the gases absorbed by the respiratory process are mingled with the blood: it appears however now settled that these gases are contained in the blood, and especially in the blood corpuscles in a chemical combination, although a loose one, in such a manner that one gas can easily displace another. Such loose combinations are not without analogy in other departments of chemistry.

We must therefore understand that the whole of the respiratory process consists in a simple exchange of gases in the blood. This discovery was made soon after the composition of atmospheric air became known. In 1777, Lavoisier propounded his theory, in which he compared the process of respiration to a species of combustion; in fact he based upon it his explanation of the production of animal heat in the body. Under the authority of his great name, this explanation long passed current among physiologists; but it is now well known that there is no such thing as a local combustion, but that the slow oxydation of the carbon and hydrogen of the tissues is continually taking place in the whole extent of the capillary circulation. These phenomena are not at first very evident to the mind, nor so striking as the old idea of the local production of heat; but they may be compared to the slow oxydation of phosphorus in atmospheric air, where the temperature may be scarcely raised above that of the surrounding medium, though it may continue a long time; on the other hand, the violent combustion which ensues when it is ignited in pure oxygen gas, is attended with a sudden and enormous production of heat, though exceedingly transient; yet it can be calculated that the total amount of heat given out by the long process of slow oxydation is quite equivalent to the sudden elevation of temperature by the rapid combustion in oxygen. The theory of Lavoisier, though long since exploded in its fundamental doctrines, has left behind it some erroneous ideas, which are still found in some of our standard treaties on physiology. Such is the proposition that blood in the left side of the heart, is warmer than in the right, whereas the contrary is the truth. This statement would concur very well with the theory which placed the source of animal heat in the lungs, but as it is, the temperature of arterial blood is lower than venous blood, since in the lungs, it comes in contact with a colder medium, generally, than the temperature of the body. Both reasoning and experiment have now completely settled this question beyond a doubt. In considering the functions of the lungs, we have so far confined our attention to the phenomena of respiration, but we must not forget that they also play an important part in the faculty of speech, and its modification, viz., production of musical sounds. The air accumulated in the lungs is there kept under the influence, partly of the will, and partly under reflex action, so that its exit through the vocal apparatus of the tongue and larynx, gives rise to articulate sounds or musical intonations.

As to the nerves which preside over the functions of respiration, they may be divided into two kinds, the motor and sensitive; the former are derived from various branches of the brachial and cervical plexus. The pneumogastric, on the other hand, seems to preside over the chemical functions of the organ itself—besides being the medium of a certain connection between the functions of the liver, relative to its glucogenic products, and the elimination of that secretion through the lungs. The theory of animal heat, is closely connected with the respiratory function, and will therefore form the subject of a future lecture.

A CASE OF PELVIC ABSCESS.

By D. Warren Brickell, M. D., Professor of Obstetrics, New Orleans School of Medicine.

April 8th, 1857, I was invited by my friend, Dr. Fenner, to see Mrs. ——, with him. Patient is a mulattress, aged about forty years, native of Tennessee, has lived in New Orleans twenty-six years, has been generally very healthy, had "some trouble about her womb" about eight months ago, but it was "remedied" by an old midwife. Since that time has been quite well, until, about four weeks ago, she felt rather feeble and would become more than usually fatigued by washing and general housework. She did not take her bed, however, until 22nd March ult. Dr. F. has been in attendance on her, and tells me that he was called to her on the 24th of March; he found her laboring under an attack of cholera morbus, and considered her quite sick; has treated her for the sequelæ of this disease up to this morning, when, on paying his usual visit, he found her suffering a good deal in the pelvic region, and, on examination, he found all the evidences of retained urine. He was not prepared to draw the urine at the moment, and, meeting her a short time afterwards, he told me his suspicions, and asked that I would assist him in the case, and at the same time try and ascertain the cause of the retention.

We carried with us a flexible catheter, introduced it quite readily, and withdrew at least a quart of urine. I now examined her by the "touch," and found the uterus completely retroverted, the os pressing up against the neck of the bladder, and the fundus thrown down into the bottom of Douglass' sac. The pear-shaped uterus could be felt distinctly per vaginam and per anum. There was no pain on pressure, and the withdrawal of the urine gave entire relief. I attempted to replace the organ by means of the finger introduced into the rectum, but failed to get the fundus beyond the promontory, notwithstanding she was placed on her hands and knees, and the firmest pressure made. Her uterus seemed to come in contact with some resisting body other than the promontory, but I could not imagine its nature. Pressure all over the pelvic region externally revealed no perceptible tumor, but there was in the left iliac region a certain degree of soreness on pressure, which we attributed to irritation of the larger intestine in the region of the sigmoid flexure, consequent on the purgation under which she had been so recently laboring. We left the catheter in the bladder.

On the following morning we saw her again. The catheter had remained in the bladder until about eight or nine o'clock, P. M., the urine flowing readily through it; but since that time she had been troubled by desire to urinate, and was now suffering somewhat from retention again, though the bladder was by no means distended. She had been enabled to pass urine partially several times during the night. I passed the catheter again, and relieved her readily. Tried again to restore the uterus, but failed; the same resistance was offered and still unaccounted for.

I did not see her again until the 12th of April, when Dr. F. told me that there was something like a tumor appearing in the left iliac region, and desired me to examine it with him. She had gradually become accustomed to the pressure of the os uteri against the neck of the bladder, and could now pass the urine voluntarily, though she was still annoyed by rather frequent calls to urinate. The bowels had been regularly evacuated and the discharges were natural. Examination externally readily detected a tumor of pretty firm nature in the left iliac region, apparently growing from below upward, and seemingly fastened to the sides of the pelvis. It was painful on pressure. Examination per vaginam and per anum failed to detect any connection between it and the uterus; the shape of this organ was well defined, and the tumor could not be felt at all from this direction. The patient was now complaining of having had, as she thought, two paroxysms of "chill and fever" in the past two days.

On the 16th I saw her again. The tumor was evidently increasing quite rapidly, the retroversio uteri still existed, though she had ceased to suffer any inconvenience whatever from it. The closest questioning could not trace the commencement of the tumor further back than I have mentioned. The patient was now feverish at times, with chills towards each afternoon, and she was suffering pain in the *right* iliac region, with a sense of pressure, as if the tumor on the left was "pushing everything over there." The uterus could still be distinctly defined, and I could trace no connection between it and the tumor: firm pressure on the tumor externally, had but a limited physical effect on the uterus with the finger applied to it.

Dr. F. now insisted on giving up the case to me, as he had no taste for such things. The patient expressed her entire willingness, but I only consented on condition that she would allow the tumor to be explored if it should be deemed necessary. I was unwilling to assume the responsibility of the case, and then sit quietly by and be guided by her own ideas of treatment. She asked time to make up her mind about things.

On the 29th she sent for me again, and agreed that I might do whatever I thought necessary, as she was now satisfied that little palliative remedies would be of no avail. The tumor had been increasing gradually, and was now distinctly prominent over a diameter of at least four inches in the left iliac region. I asked that Dr. Choppin might now be called in consultation, and it was readily agreed to. During several days we visited her together, each time examining her carefully, and endeavoring to work out a diagnosis. We finally determined that it was one of two things, either an encysted tumor of the ovary, which had lain dormant for a long time, and was suddenly growing with great rapidity, or an abscess, the precise locality of which we could not determine. From the train of symptoms, ill-defined as they had been, we thought abscess most likely. Direct exploration was the only means now of determining with any degree of certainty. She had ceased to suffer from the retroversio uteri, although it existed as completely as ever, and could not be remedied; but she was now suffering from light fevers every afternoon. Appetite was good, however, and digestion perfect.

On the morning of the 6th of May, we passed an exploring needle, hidden in a silver canula, deeply into the tumor at its most prominent point, and as nearly directed towards its centre as possible. Dr. C. passed it in to the depth of about three inches, and, shaking it from side to side, observed that the tumor appeared to be solid. The needle was withdrawn, however, leaving the canula behind, and out gushed *pure, laudable pus*—thus proving that our diagnosis had been a pretty fair one. Pressure was made in every direction, and through this small canula we succeeded, in the course of half an hour, in withdrawing about three-quarters of a pint of pure pus. Of course the parts were left in a state of comparative relaxation, and the patient expressed herself as considerably relieved.

It was now deemed expedient to keep up the flow of pus externally; of course our present opening would not be at all sufficient, and a larger opening, by means of trocar or bistoury, might arouse peritonitis by escape of pus into the abdominal cavity; therefore the application of the caustic potash promised the best result. It was applied over the seat of the recent operation, being guarded by a piece of adhesive plaster in the usual manner, and allowed to remain several hours. After its removal, poultices were applied, and at the end of four days a slough, about the size of a quarter dollar, was taken out. The excavation was not very deep, but firm adhesion between the peritoneal surfaces had taken place.

The tumor was now apparently filling again, so we determined to push a large trocar into it and evacuate the pus. This time fully half a pint of pure pus flowed, and with considerable relief to the patient. The opening was enlarged with a bistoury now, and a tent placed deeply in the wound. Next morning I visited her and found that the tent had dropped out a few hours after we left her, and the opening in the tumor had closed entirely. She expressed herself as perfectly comfortable, however, and was quite cheerful. In a day or two the tumor began to enlarge again, and at the end of six days we concluded again to introduce the trocar. It was passed in the same manner as before, but, to our astonishment, not one drop of pus flowed. We tried faithfully to reach pus, but failed to draw even a drop. It was thus, on the 16th of May, that we found her, with the tumor gradually enlarging again, but apparently no secretion of pus. She was put on general remedies, with application of poultices, etc., as usual, and I attended her from day to day—Dr. Choppin calling occasionally—until the 20th, when the tumor was so much larger than ever before, and was giving so much trouble and distress by pressing over toward the right side, as well as impeding defecation by pressing on the rectum, that it was again determined to open it freely, as everything indicated the presence of pus. Moreover, the patient was laboring under a sort of irritative fever at times, and would occasionally suffer from accumulation of gas in the intestines. Her appetite had not failed her, however, and she had maintained her general health far better than could have been anticipated. She had also passed regularly through a menstrual period. On the morning of the 21st, then, we, for the fourth time, plunged the trocar deeply into the tumor, it being now larger, more prominent and more painful than ever before. This time pus, thick and pure, spouted through the canula boldly, and in less than half an hour *nearly two pints* were withdrawn, with great relief to the patient and entire subsidence of the tumor. The opening was now enlarged with the bistoury and a tent pushed deeply in. Poultices, good diet, anodynes, etc., were ordered, and the patient required to lie as much as possible on the left side, in order to favor the escape of pus.

June 5th. Since the last operation the patient has not been doing well. Although the wound has been kept carefully open, little if any pure pus has flowed, the tumor has enlarged very slightly indeed, and when pressed on considerable pain is felt and there exudes a reddish water only. The wound itself has a perfectly healthy appearance. The chief trouble, however, has been constipation, with great suffering from accumulation of gas in the bowels. At times this is so severe that she cries out with pain, and she says that she feels as if the action of the bowels is reversed, causing almost constant nausea and sometimes distressing emesis. The consequence is she eats very sparingly and has emaciated considerably. Every evening she has an exacerbation of fever, with pain as above, and in the mornings she is exhausted. I have been giving her stimulating enemata and purgatives in variety, but as yet can produce no free evacuation from the bowels; and this, she says, is the great cause of all her trouble.

6th. Was called to her last evening and found her suffering greatly—abdomen distended with gas, nausea, "cramps in her stomach," and high fever. She begged us to give her something to quiet her. This time I determined to try the virtue of opiates on constipation, at the same time having a view principally to giving her at least temporary rest. I ordered half a grain of morphia and five grains of calomel every hour until she slept, anodyne poultices, foot bath, etc.

This morning I find her looking quite bright. After taking the fourth pill—in all, two grains of morphia and twenty grains of calomel—she went to sleep and slept until seven this morning, having been awake only once to have a very free evacuation from the bowels—the first for many days. She still has considerable fever, but is vastly better than when I left her last night. The tumor has grown but little since we last opened it, and the tent is renewed daily, but nothing but a little reddish water flows from it, and this only when pressure is resorted to.

8th. She has been pretty quiet since my last note, until last night, when, about eight P. M., she sent for me, to say that she was suffering "colicky pains" again. I found some accumulation of gas again, and ordered half a grain of morphia and two grains of extract of hyosciamus, every hour until she slept. This morning I find her looking bright again. She took two pills last night and slept until four this morning. Her bowels have been again freely open, and she feels better in every respect. For the first time since the last operation the wound is pouring out pus pretty freely, and in reapplying the tent I find that the probe does not penetrate anything like as deeply as usual, leading me to hope that the cavity is filling up. There is evidently no increase in the size of the tumor.

15th. Since my last note patient has been very low, indeed. Four or five days ago the wound nearly closed again, and the accumulation of pus soon caused great suffering, as accumulation of gas in intestines, fever, etc. Again I passed the trocar, but in enlarging the wound with the bistoury, and without wandering more than the eighth of an inch farther than usual, I cut the superficial circumflex artery. A compress speedily stopped the hemorrhage, however, and next day the wound was suppurating again. I squeezed out a quantity of pus, mixed with a dirty water of disagreeable odor, and up to the present time this mixture continues to flow, under the use of poultices. Unfortunately she has been slightly salivated by the calomel given to overcome constipation, and this has weakened her. However, she is to-day doing about as well as she was at last note. I am giving her nourishing diet, with ale and porter, and opiates when she cannot sleep without them.

18th. Twenty-four hours after my last note my patient began to sink, and she died at two o'clock this morning. Ten hours after death I examined the body, assisted by Drs. Choppin and Fenner. The only description I can give of the condition of the pelvic region is, that all the pelvic viscera—rectum, uterus, ovaries, bladder and all—were involved in a common mass by inflammatory action: they were literally grown to each other, so that it would have required the nicest sort of dissection to separate them. The whole pelvic contents were represented by a common mass, which, if cut out and placed on the table, would not have been readily recognized by the anatomist. The abscess had evidently commenced under the pelvic fascia of the left side, and pus had gradually dissected its way around, until every portion of the pelvis (under the fascia) was occupied by it. The accumulation had lighted up peritonitis, and hence the agglutination of the organs.

This disease attacked the patient most insidiously, and until the tumor appeared in the left iliac region I am sure no one would have suspected its true nature. Under the treatment adopted the patient succumbed, but I am sure the result is in a great measure to be attributed to procrastination on her part. That the mode of evacuating the contents of the tumor is the proper one, I feel satisfied. The mistake in the case was, that the caustic was not applied early, and free evacution made and maintained. In the next number of the Journal I will report a similar case, where prompt and energetic treatment of the same kind has been followed by the happiest results.

SPECIAL SELECTIONS.

[We herewith present our readers a "Special Selection" of much value, inasmuch as it must force the mind to reflection on one of the most important points in Obstetric practice. Some of these days we hope to give our own views in relation to the graver obstetric operations, their indications, etc. Three times in the past ten years have we heard of women at full term, and who died in labor, being buried with their children in utero —no *sufficient* effort having been made to deliver them either before or after death. Surely this is not conservative obstetric practice, whether considered in a scientific or a common-sense point of view.—EDS.]

REPORT OF A CASE OF EXTRACTION OF A LIVING CHILD, BY TURNING, AFTER THE DEATH OF THE MOTHER; WITH REMARKS.

By G. W. THORNTON, of Newport, Kentucky.

[*Read before the Newport and Covington Medical Society.*]

On the fourth of September, 1857, I received an urgent call to visit Mrs. S., who, the messenger said, was either dead or dying in child-bed. In about fifteen minutes I was by the bed-side, and from a careful and deliberate examination, concluded it was too true—the accouchee was not only apparently, but actually, dead.

There was no respiration, no pulsation of the radial or carotid arteries, and not the faintest murmur of the heart; added to which there was a fixed dilatation of the pupils, a bloated and dusky appearance of the face, with cadaveric lividity of the neck, breast and shoulders.

This examination,—together with the time occupied in receiving a short account of what had occurred immediately preceding my arrival, as given by the experienced midwife in charge of the case, and the monthly nurse, who had also been summoned and was in attendance before me, must have consumed twenty minutes, to which add five minutes, for the time it took the messenger to reach my house, and we have forty minutes, as the least possible time, by computation, since the accouchee apparently expired, for the midwife assured me she had not breathed since shortly before she dispatched the messenger. This, then, was the time, forty minutes, after the last expiration of the mother, at which I began to attend to the rights of the undelivered child.

An examination showed that, the membranes had been ruptured, and that the head was in the cavity of the pelvis, the vertex presenting at the inferior strait, toward the arch of the pubis. A messenger was dispatched for my short forceps; but I did not wait a moment for them; the fœtal head was raised and the flaccid womb entered without the least resistance, and the child was turned and extracted as far as the head without delay; at this point it momentarily hung until assisted by my fingers in its mouth. It is supposed the complete extraction was accomplished in five minutes, making in all forty-five minutes since the last apparent expiration of the mother to the completion of extraction.

The child did not breathe, and was of a bluish color. I thought there was a feeble pulsation of the cord at this time, though the midwife, who tied it, assures me there was none whatever. The doors and windows were all open, and the child was laid on a blanket on a table immediately under a window. On applying the ear and listening attentively a slight ticking pulsation of the heart was audible. The fauces having been cleared, the Marshall Hall Ready Method was diligently practised for, it is believed, not less than half an hour, perhaps longer, before the first attempt at a convulsive inspiration declared itself, but these measures being perseveringly continued, assisted as they were by aspersions and frictions with a brush, respiration was at length established, and the child assumed a good pink color, though for some time it groaned with almost every breath, and had frequent slight spasms for about twenty-four hours; these, however, at length subsided, and the child, a boy of average size, sucked vigorously at its bottle and at the breast of a delicate woman for three weeks and two days, when it died.

The nurse gave suck to her own emaciated child which had a sore mouth and which died more than a week after she first took the posthumous one to nurse. If we except the first thirty-six hours, this child seemed to thrive and do remarkably well in every respect for ten days, when I ceased to visit or inquire after it. But on the twenty-fifth of September, when it was just three weeks old, I was sent for to see it, and was astonished to find it in such a state of marasmus as to give it the appearance of a very old shriveled man. It had been seized with stomatitis since I last saw it, and had, as an effect of this, a much enlarged and inflamed submaxillary gland, which seemed on the point of suppuration.

The child died in two days after this, on the twenty-seventh of September, having lived just three weeks and two days. From its general appearance, from its nursing and crying so vigorously, from its breathing so well except for a short time after its birth, and from the function of calorification having been performed so perfectly, all for ten days, I was utterly surprised at the death of this child, in which I felt so much interest; nor can I now divest myself of the opinion, that could it have had better advantages it might have fared far otherwise.

Mrs. S. was about twenty-six years old; for the last five years of her life she had had phthisis pulmonalis. This was her fourth child, and during the latter months of its gestation, she suffered much from dyspnœa, palpitation of the heart and œdema of the inferior extremities. It is here proper further to add, that she had been engaged, during the forenoon of a very hot day, in caning tomatoes, up to the time of her being seized, at about one o'clock, with active labor; that then she and the midwife, who was with her, went up stairs to the room in which she was to be confined; that for about two hours she progressed very rapidly, and as was supposed, favorably; so that it was expected the unfortunate case would soon be gotten through with, when it is said the accouchee suddenly threw her hands high over her stomach, and exclaimed "Oh! that pain is not right!" and at this moment she clenched her hands and ceased to breathe.

The midwife now used up half of a large bottle of camphorated spirit, rubbing and dashing it on, and also tried putting water into her mouth, but she never after, swallowed or breathed. After extracting and resuscitating the child, and again examining the deceased, we sent for another midwife who was a relation of the family, so that there were two present at the shrouding of the corpse, and as there was a smart lochial discharge, the first midwife tried to deliver the placenta by the cord, which had been cut off close to the mother, but failed; the second midwife now introduced her hand into the womb, and, as she told me afterward, succeeded with difficulty in scooping it away. In answer to the question,—"Was the womb at this time drawn up on itself?" she said, "No, it was not, it was open just like an empty bag." After this the wasting made it necessary to use cotton-batting, as a tampon, in the vagina, in order to prevent soiling in shrouding the corpse. This was after the cadaveric rigidity had commenced, and about two hours and a half after death. I would still further state that, after the body was laid out, I again visited it, anxious, though not expecting, to discover some sign of lingering animation, but in this I was not surprised, every vestige of life was extinct. The child cried in the lap of the stranger, but its voice awakened not the ear of death, nor kindled into expression, features which were fast stiffening into marble-like rigidity. The face, before bloated and dusky was now shrunken and palid,—the cadaveric lividity had somewhat faded, by gravitation, from the front part of the bust, but on the back, one of the midwives told me, the blood had settled so as to look like a child that had died of croup, this I did not see myself. A *post mortem* examination was desired, but not obtained.

The above case is highly suggestive of many points of great interest to the physiologist, the obstetrician, and the medical jurist, some of which it may be useful practically to consider. They are as follows:

1. What was the condition of the woman at the time the messenger was dispatched, and up to the time of my arrival? which interval has been computed at twenty minutes.

2. Was she in a state of asphyxia? if so, was it sudden and complete? And what occasioned?

3. How long after complete asphyxia before the heart ceases to propel the blood through the systemic blood vessels?

4. How long after complete arrestation of the heart's action, if at all, may the capillaries of a woman at full term, having been in active labor for some hours, continue to interchange with those of the placenta?

5. How long after the cessation of such interchange, may an unborn child, without breathing, maintain its vitality?

6. Should a child, at full term, die in-utero, from deprivation of oxygen and non-decarbonization of its blood, what should be the pathological manner of, and reason for, its death? Are they the same as when death occurs from constriction of the cord as the primary cause?

7. How long, and under what circumstances, may a child at full term, survive its mother and continue in the womb or partly in it and the vagina?

8. How long after the birth of a child, which is in a state of suspended animation at the time, may we hope to resuscitate it? and is the time for resuscitation shorter in the adult, under suspended animation from asphyxia?

9. Is the utero gestation of a living child, at full term, a circumstance that would be likely to modify, in any degree, the period of the woman's survivorship after complete asphyxia?

10. How are we to distinguish actual from apparent death?

11. What are the legal questions that might grow out of such a case as the one above reported?

12. What are the duties, and what should be the conduct of the obstetrician in such an emergency as the one reported?

It is obvious that a full and proper discussion of any *one* of the above questions would form in itself a lengthy scientific paper, and as the principles they involve have all been ably treated of in works on physiology, obstetrics, and forensic medicine, it is not my intention to dwell upon them in this manner: indeed, had I the ability to do so, it would not consist with the character and intention of a paper like this; they have only been suggested, therefore, with the view of presenting to the minds of my hearers, in a concise and somewhat methodical mode, some of the chief points of interest connected with the above narrative of posthumous birth; and further, in a very general manner to remark upon them. And I should, moreover, here observe that, my remarks on this occasion will, from necessity, on the score of time and deference to the patience of my auditors, be confined to the first five of the questions. In the first place, then, if it be remembered that the mother had been for five years, subject to phthisis pulmonalis, and that during gestation she suffered greatly from palpitation of the heart, dyspnœa, and œdema; if, likewise, it be further borne in mind that, for some hours, immediately preceding the confinement, she was personally engaged on a hot day in a suffocative employment, which was only interrupted by the supervention of active labor pains, and that then she ascended a flight of stairs to be confined in very rapid labor, before the completion of which she suddenly died, the inference will strike the mind of the physiological reasoner, with the force of irresistable conviction, that, the condition in which she was said to be at the time medical aid was sought, was that of suffocation or commencing asphyxia, indeed, with such antecedents, the occurrence of death by any of its other modes, would fill his mind with utter astonishment and surprise. The cadaveric lividity too, which was found at my arrival, on the neck, breast, and shoulders, most unequivocally declares that the left ventricle continued to propel its undecarbonized blood, with which it continued to inject the systemic capillaries, after respiration had ceased, and beyond successful controversy stamps the death as one by asphyxia in contradistinction to that by syncope in either of its forms of anemia or asthenia. For so sudden a death to have occurred from anemia, we must conjecture a rapid concealed hemorrhage from the rupture of perhaps, the womb, the heart, the aorta, or the pulmonary artery, in which event we should have had a blanched, not a livid appearance of the cadaver.

If, then, the death was not caused by this form of syncope, let us examine for a moment the probability of its having been occasioned by the other, namely—asthenia.

Now, for death to have taken place so suddenly, by this mode, in one not worn down by protracted labor, we must suppose a violent and sudden shock to the nervous centers commensurate to the effect—such causes are sometimes of the emotional kind—such as terror, violent anger, great joy, sudden surprise, etc., but it is not in evidence that the deceased was subjected to any of these. Was it then physical pain that paralyzed so suddenly and completely the action of the heart? What could have produced such violent and instantaneous pain as this implies, in the fourth accouchment of a woman, after the os uteri was so far dilated as that the greater part of the circumference of the fœtal head had passed its circle, and presented at the inferior strait? Surely then it was not the pain of dilatation.

Could it have been the perineal pain, which is often so sharp, in the act of expulsion, as to extort a shriek from the most resolute and courageous woman? The head was not far enough advanced for this, nor was the woman a primipara, of rigid fibre, with an unusually large fœtal head: on the contrary, this was a fourth labor, in a woman of relaxed fibre, whose child was not above the average size. Could the violent shock then have been occasioned by rupture of the womb at the time the woman threw her hands high up on the abdomen, (as the midwife states,) and exclaimed, "O! that pain is not right." Short as is this sentence, it may strike the minds of some as being five words too long to express a pain capable of being the immediate cause of sudden death; the simple exclamation, O! would have been quite sufficient: for the pain that paralyzed the heart had first to paralyze the brain and nervous centers. Again, that the child nor the afterbirth did not retreat and escape into the cavity of the abdomen, is not conclusive evidence, under the circumstances, that the womb was not ruptured, but certainly bears strongly on the point that death was not superinduced by asthenia consequent upon the rupture; for it is not probable, as we have seen from the criticism on the last exclamation of the deceased, that the shock of laceration was sufficient to have produced instantaneous death, unless followed up and aided by a subsequent and superadded cause; such might possibly have been the effect of the escape of the uterine contents into the peritoneal cavity; but this did not occur in the case of the child nor the afterbirth, neither could it have been the case with the liquor amnii, which had been drained off, it may be presumed, after the rupture of the membranes. We may, therefore, fairly conclude that there was no moral nor physical shock capable of producing death by asthenia. Further, the same serious objection lies equally against death in this case by asthenia as anemia: there should have been palor after the former as after the latter, not lividity. If it were deemed necessary, and had not the appearance of arguing in a circle, the long survivorship of the child, before extraction, and after the death of the mother, might, it is believed, be successfully urged in favor of death by asphyxia, as against syncope or any other mode of sudden death. Could a post mortem examination have been had, our knowledge in reference to rupture of the uterus would, of course, be more satisfactory.

Apoplexy, resulting in sudden and great mischief to the medulla oblongata, is sometimes the cause of instant death, by producing apnœa or syncope. But to be convinced that the organ of animal life had no share in the primary causation of death, in this instance, it would seem quite sufficient merely to reapply the argument already made use of against the death of this woman by asthenia, namely, that immediately before she expired, she threw her hands high up over her abdomen and made the exclamation above quoted, which voluntary actions of the arms and vocal organs, together with the manifestation of the reflective faculties obviously preclude the idea of the cause of death having been primary in the brain: but it has been proved that it was not primary at the heart. Then it must have been, as was before concluded, by asphyxia; and this, too, must, if the above course of reasoning be correct, and have been primary in its origin in the lungs themselves, and most unquestionably had for its *predisposing* cause the obstruction produced by phthisis. For its *exciting* cause the hurried state of the circulation and respiration, together with the holding of the breath in order to bear down violently, all occasioned by labor, together with the previous employment and exertions of the woman; and for its *proximate* or *immediate* cause, pulmonary congestion—perhaps even pulmonary extravasation, or pulmonary apoplexy.

Whether this lesion commenced simultaneous in both lungs cannot be determined, though it is highly probable that, in its very origin, it was attended with great embarrassment, causing distress and pain, which might have been occasioned by a spasmodic action of the diaphragm and intercostal muscles, which soon caused the woman to cry out, on the relaxation of the spasm, and to throw her hands up over the region of the diaphragm and lower part of the chest, and that then, the mischief still progressing, and that more and more rapidly, complete asphyxia and coma were very soon induced; all of this might have transpired in two minutes; then from the moment of complete apnœa it would not be safe, with the lights we have, furnished from asphyxia from drowning, to conclude that the heart could continue to pulsate longer than five minutes; from which supposition it would appear that the somatic life of the woman was extinct in about seven minutes after she first threw her hands up and made the exclamation quoted, and for a period of about thirteen minutes before my arrival, and thirty-eight minutes before the extraction of the child.

Having now arrived at the conclusion that the mother died from asphyxia, and that her heart probably ceased to beat and propel its blood over the system for about thirteen minutes before my arrival, and for about thirty-eight minutes before the extraction of the child. The question is asked, "How long after complete arrestation of the heart's action, if at all, may the capillaries of a woman at full term, having been in active labor for some time, continue to interchange with those of the placenta?"

In attempting to arrive at some conclusions in answer to this question, it is needless to remind my hearers that we possess after birth a *dual* mode of existence—an animal and an organic life—and that, after the dissolution of the animal, the cessation of the organic occurs at different times in the organs and systems of which the body is composed, and that the time of death and its order of sequence may be much modified by the causes which have induced it in the organism, and by other controling circumstances. Thus, in death from any of its proximate modes, the general capillary system of bloodvessels seems to survive, for a short time, the arterial and venous, but where the *proximate* cause of death has been asphyxia, the general capillary system seems to survive and to continue the performance of some of its functions for a much longer time than after any other mode of death.

The doctrine of "ubi irritatio, ibi fluxus" may be modified by "ubi irritatio vel excitatio, ibi fluxus" without in the least diminishing its truth, and if so, during the highly energised and physiologically excited state in which the uterine organ is, during not only gestation, but, in a much more especial and increased degree, during labor, we are bound to admit that a larger portion than usual of blood is distributed to this organ during labor than to other organs relatively, or than to itself, at other times. Now, in the death of a parturient woman from asphyxia, there would occur in the capillaries a remora, or, perhaps, stasis of this increased quantity of blood, so sent to the womb by reason of their inability to empty themselves into a venous system impeded in its circulation by an already engorged cava and surcharged right ventricle. So, then, at the time of the death of a woman in labor, from asphyxia, we have the capillaries full of blood, and, though dark, (for I will not say venous,) it still remains fluid; both of which circumstances—the quantity and fluidity—are amply sustained by the fact of the free postmortem lochial discharge, which in the above report is said to have occurred. This being the case, if the uterine capillaries ordinarily empty themselves into the umbilical vessels of the child through the placenta, (their vitality remaining for a short time,) there is no reason they should not continue to do so, now that this is the only ready passage open to them. If, on the other hand, (as I myself prefer, and as appears to be pretty well proven,) the theory of placental æration by endosmosis and exosmosis be taken, the fluid blood in quantity is there and in the right channels for the purpose, and there can be no reason, on this view, why this mere mechanical or chemical interchange might not take place even after the death of the capillary system of the mother, and inure much longer to the sustentation of the child. And if, again, the theory of the extravasation of the mother's blood into utero placental, or placental sinuses be adopted as physiological, the child would lose nothing by it. Well, then, if this kind of blood can for a time aid the child in the prolongation of its uterine existence, after the death of its mother, we shall assume it proven that it has it, and that in abundance. But, in reference to the child, it may, perhaps, be asked, of this dark blood, *cui bono*, since it could not sustain the life of the mother, how can it prolong that of the child?

Anological conclusions should always be received with the greatest caution, for where the analogies are not strictly accurate they are seldom true. If, therefore, the child has never breathed, I think the deduction is not logical. For until this circumstance takes place, the foramen ovale remains open, and the blood in both sides of the heart, in the aorta, in the cavæ, and in the umbilical arteries and vein, is alike dark, approaching in all its sensible qualities more nearly to that of the venous than the arterial blood of the mother. So much is this the case, that when the blood in the aorta is found of a redder color than that in the cavæ of a dead new-born infant, it may be taken as strong corroborative evidence to that furnished by the statictist, that the child was born alive. The defective analogy then seems to militate against the above apparently hasty conclusion. To which add, that we have not here an animal life moving upon a tripod, upon which sits a cubic half-inch of medulla elongata, constantly telegraphing to the lungs to hurry up their function or it will shut down on both them and the heart, though it should fall itself, as fall it would, in the ruins. It would be in vain now, after this "tripartite confederation," that the heart should say to the panting, suffocating lungs, never mind the tyrannical threat, I am not dependant on animal life for my excitation and contractility; I can pump along without it; I will send you blood, as in times past, for your organic wants; just have confidence, and let my blood pass your portals to our other minor confederates, and never mind the consequences. This banter is too late, now that they are all indissolubly bound in a common fate. But, before the great triple alliance, little oxygen was found necessary to the maintenance and growth of the mere organic being; for it probably imbibed, by reason of its parasitic connection, its nutrient particles, ready assimulated to its wants, from the rich fibrinous blood of the mother. It only required plastic, not respirable nutrient material—oxygen for building up, not for pulling down and removing the detritus of decay. It seems to be generally conceded, that animal heat is the result of combustion; and may we not also assert that the degree of combustion, or quantity of animal heat, is a *measure* of the quantity of oxygen required to produce it? Does not the circumstance of the low animal temperature of the new-born babe, then, physiologically prove its small demand for oxygen for mere organic growth? And is it not probable that the warmth of the child before respiration, and whilst surrounded by the bag of waters, (an equable conductor of the mother's heat,) is almost entirely derived from the mother? And is not the heat it generates *per se*, being *much less*, probably, *before* than *after* birth, a physiological reason, as above suggested, for inferring that a still less degree of oxygen may be necessary during uterine life?

Again, the great sugar organ of Bernard—the liver—is both relatively and absolutely a predominantly developed one. In the child both before and after birth, physiologically pointing to a predominance of functional labor and a requisition for a larger proportionate supply of blood for the execution of its function; and, in the child that has never breathed, as in the adult, the blood necessary for its functional uses is carbonized and dark. Now, the same blood that supplies the liver of the unborn child for its functional uses is part and parcel of that returned from the placenta, by the umbilical vein, through its continuance of the *ductus venosus* to the ascending cavæ, shortly to reach the heart, and to be propelled thence over the whole system, together with the blood from the liver, which has reached the same destination through the hepatic vein. Now, if the best oxygenated blood of the unborn child be dark, and such as is given for the performance of an important function in its economy, and that same function be found to be discharged in the adult by dark or venous blood containing a small degree of oxygen, it would seem to imply that the quantity of oxygen in the arterial system of the one is not greater than in the venous system of the other, and affords, by physiological deduction, another proof that the unborn child requires but a small percentage of oxygen in its blood. If, indeed, the quantity be not greater in its arterial blood than is contained in the venous blood of the adult, which we have attempted to prove, the proportions there would only be five per cent., and perhaps not more than two and a half in its venous. Well, it may be said, granting all that the argumeut asks for thus far, and still some oxygen has to be furnished by the mother to the unborn child. What evidence have we that the blood of a mother dead from asphyxia contains oxygen?

It has been long a conceded point among physiologists, (to which we have already adverted,) that, the animal function of calorification, depends upon a species of combustion; which of course, in this case, as in all others, requires the union of oxygen with a combustible material for its support. Although fully agreeing upon this point, they earnestly disputed among themselves whether the combustion (or which is the same thing, the function of calorification which is the effect of it) took place directly in the pulmonary, or in the systemic capillary system. The latter opinion now appears to be received by the best authorities as the most tenable; and is, I would suggest, much strengthened, if not entirely put to rest, by the continuance of calorification in a person dead from asphyxia, and long after the heart ceases to beat. Which circumstance, it is conceived, not only conclusively proves that, that function is performed in the systemic capillaries, but implies, beyond the possibility of being successfully controverted, that there is sufficient oxygen in the asphyxiated or dark blood contained in those capillaries to support combustion or calorification; also, it farther points to a species of molecular or chemico-organic life during the continuance of this function after death from asphyxia. The venous blood of an adult drawn during life, has, it is said, been found, by Professor Magnus, to contain five per cent. of free oxygen. But as I was in possession of no information proving that a person dead of asphyxia contained oxygen, it is believed that that fact has been sufficiently arrived at by physiological deduction, and will therefore be assumed as proven.

It is, then, the oxygen which has been demonstrated to exist in the blood of the asphyxiated mother, and which the child received the benefit of through means of the placenta, that may prolong its existence after the complete cessation of the mother's systemic circulation.

But the question we have been discussing seeks further to know "how long, if at all, after such cessation of the mother's systemic circulation this interchange may continue?"

The reply to this cannot be very satisfactory in the present state of our knowledge. Because many circumstances, it may be supposed, may combine to lengthen or shorten the time it would take the child completely to deprive or exhaust the available blood of the mother, supposed to be arrested in her uterine capillaries in reach of placental action, of its free oxygen, and which oxygen could not, so far as we are warranted in supposing, be again renewed for the benefit of the child.

But it would be right and in accordance with the dictates of humanity, seeing that the blood of the parent, after death from asphyxia, remains long warm and liquid, and that the body, too, is long in stiffening, to indulge the hope that the child may for some time at least, after the complete cessation of the mother's systemic circulation, continue to derive oxygen for its maintenance, from her utero-capillary blood in reach of its placental influence, since we have demonstrated that this oxygen there exists. But at the same time, it is believed, that it would not be practically safe, so far as the life of the child "in ventre sa mere" is concerned, to measure the time of its possible survivorship after the complete cessation of the systemic circulation of the mother, by the temperature and fluidity of her blood or by the absence in her cadaveric rigidity, if so in the case just reported, we might have deferred for about two hours and a half after the death of the mother the extraction of the child, which delay would, manifestly, have been too late for the preservation of its life.

And, too, we might, were this rule adopted for the guidance of our practice, in some other cases be tempted to wait for a much longer time, for it is well known that sometimes after death by asphyxia, and under certain circumstances, the above conditions,—namely, warmth, fluidity of blood, and absence of cadaveric rigidity,—obtain for many hours, not to say even days; and although there may be a possibility of the survival of the child in utero during the continuance of these physiological conditions, it would not be proper or safe, so far as its life is concerned, to rely on them.

But we continue, in the fifth place, to consider "how long after the complete cessation of utero-placental interchange, may an unborn child, without breathing, maintain its vitality?"

Doubtless this period, too, like that we have just considered, would be greatly affected by various circumstances, one of the chief of which we may suppose to be, whether the mass of the child's blood had continued, up to the very instant of such complete cessation of the mother's circulation, to derive an ample supply of oxygen, and to part freely with its carbon; or in other words, whether it had been in a state of commencing asphyxia before the time alluded to, a circumstance evidently controled by the mode of the mother's death. Should a mother die, very suddenly, from asthenia it may be supposed that her unborn child might be capable of surviving for as long a time as it would, were the entire ovum suddenly cast off from her organs, and received into a vessel of water at the temperature of her blood; which period was found by Velpeau to be upward of thirty-six minutes in a fœtus supposed to be six months old. We are told that Wrisburg and Osiander found the human fœtus to survive ten or fifteen minutes out of the womb when the membranes were not ruptured, but it is not stated whether the time might not have been prolonged had they, like Velpeau, put the ova in tepid water.

Buffon caused a bitch to bring forth her young in tepid water, and the pups to be kept submerged for half an hour after, at which time they were found to be alive; when they were transferred to tepid milk, and treated in the same way, for the same length of time, when they were still found to be alive: one was now given to the bitch, it sucked well and was reared, the others were subjected for a longer time to the experiment until they died. The great naturalist sought to draw from this experiment a different conclusion than that which I wish to deduce from it. For he says, "I have not pursued these experiments further, but I have seen enough in them to persuade me that respiration is not so absolutely necessary to the new-born animal as to the adult, and that it might perhaps be possible, by proceeding carefully, to prevent, by these means, the closure of the foramen ovale, and produce excellent divers; and species of amphibious animals, equally capable of living in air and in water." This is his conclusion. But I only seek to deduce from it and from the above quoted observations on the human ovum, the fact of the possible long survivorship of the infant, *in ventre sa mere*, without breathing, and completely independent of placental aeration.

This, then, I conceive to be the answer to the question, as near as we can arrive at it: namely, thirty-six minutes, or thereabouts, after complete cessation of utero-placental interchange may the child maintain its vitality in the womb of its mother without breathing. Why should it be accounted a marvel that a being just having completed one mode of existence and being only about to enter upon another, as soon as the physical conditions present themselves, should linger for a while, in *statu quo*, or in a state, as it were, of hibernation between the two modes of existence, requiring only the evolution of sufficient life-force to preserve its status; and may we not conceive it possible that the little life-force supposed to be necessary for this purpose might be generated and kept up for a time by decomposition and recomposition of elements within its own organism, in the placenta and cord, and perhaps in the amniotic fluid surrounding it, when they have not been previously drained off—but this, I admit, is very theoretical, and only given as such—though, to me, having an interest as taken in connection with the doctrine of Druitt and Barnes, in regard to placental degeneration in the latter months of utero-gestation, being a normal process whereby the organ (it being only a temporary one,) is curtailed of its functional activity, like other temporary organs, by means of earthy and fatty deposits in its cells, and this, pari passu, as the fœtus attains more and more to complete development, requiring less and less elements of growth and sustentation from without. But let this go, and in conclusion allow me to allude to the many recorded cases of Cæsarian extraction of living children, after the death of the mother; also to a case of extraction of a living child by turning, after the death of the mother, under circumstances very similar to the one the history of which has been given in this paper. For the particulars of which case, see the November number of the American Journal of Medical Sciences, for 1836, page 259, which case is also alluded to in Taylor's work on Medical Jurisprudence, under the head of post mortem births, page 434, of Hartshorne's edition for 1856. It may be well here to state, too, that in this case the child was not extracted for a quarter of an hour after the death of the mother, and twenty hours after the rupture of the membranes. Taylor also cites the Medical Gazette, vol. forty-sixth, page 713, for another case of the birth of a living child after the death of the mother. But the most singular case, perhaps, of long survivorship of the child after the death of the mother, is that of the unfortunate princess of Schwartzenburg, who died in consequence of a burn she received in Paris, at a ball given to the Empress Josephine or Maria Louisa, (I forget which). She was enciente, it seems, notwithstanding she was at the ball, and it is said she could not be opened after her death until the next day, notwithstanding the lapse of which time, the fœtus was found living. This case is referred to by many authors, and among others by Velpeau, who under the head of Cæsarian operation, also mentions on the authority of Gardien, that another woman was not opened until forty-eight hours had elapsed after death, and the child was found still to be alive. I think, though Velpeau is not disposed to vouch for the accuracy of these cases, some other authors, however, give credence to them; and they should, at all events, make us mindful of our duty to the unborn infant after the death of the mother.

Regretting that time will not allow of my considering the other branches into which this, to me, very interesting subject leads, allow me, in taking leave of it, to thank the President and gentlemen of the Society, for their kind indulgence.

EDITORIAL AND MISELLANEOUS.

Our New Volume.—With the present number we begin the fifth volume of the New Orleans Medical News and Hospital Gazette. We have no new promises to make for the future; we feel that we have hitherto done our duty to the fullest extent that our patrons have permitted us, and we can only promise to do more when they "pay up" like honest men, and contribute more liberally to our pages, like true lovers of their profession. To all those who have fulfilled their duty to us, we return our sincere thanks, with assurances that we labor for them most willingly. For those who are still in our debt, we have no thanks, and we shall continue to dun them until they do become ashamed of themselves.

Once more we say, we do not edit a journal as a pecuniary speculation, but will devote every dollar sent us to making ours the best journal in the country. Nothing but money can do this, and if every subscriber would pay promptly we should have enough for the purpose.

ARTIFICIAL RUPTURE OF THE AMNIOTIC SAC DURING LABOR.—Two late numbers of the Western Lancet have contained articles from the pen of B. F. Richardson, M. D., Adjunct Professor of Obstetrics, etc., in the Medical College of Ohio, on the subject of "Artificial Rupture of the Amniotic Sac during Labor;" the author takes positive ground against artificial rupture, and his articles have been pretty widely republished in our exchanges. Trusting that any and all critical remarks we may make will be received in the kind spirit in which they are offered, we essay to prove that Dr. Richardson has totally failed to subvert one of the long established rules of obstetric practice, viz.: that when the os uteri is thoroughly dilated, there is no longer any use for the liquor amnii, and, all things being fair, labor will be safely expedited by the artificial rupture of the membranes. In other words, when the os is thoroughly dilated, and, with normal contractions on the part of the uterus, the membranes do not rupture spontaneously, artificial rupture of the same is the palpable indication, and can never result in harm to mother or child.

We will not undertake to quote authority to prove our positions, nor to disprove those of the author, but appeal at once to the common sense of the reader who has reflected on this subject of the mechanism of labor. And here we would remark, that it is a most unfortunate circumstance, that the vast majority of men engaged in the study of the mechanism of labor, really make it an entirely mechanical business, leaving out of all consideration the *physiology* of the process. One man argues most strenuously, that it is the amniotic sac which dilates the os, and another contends, that the vertex ("covered by the soft, well lubricated scalp,") is the better dilator; and both, in their mechanical mania, ignore the beautiful truism, that it is physiology, and neither the sac nor head, which has done the important work. Physiology makes the os soft, patulous, dilatable, (or even absolutely dilates it; throws the gate back on its hinges), and the uterus then pushes the child through. Let physiology say that the os is rigid, and where then is the uterine force capable of stretching the child through? Does not physiology thus prepare the vagina and the vulva long before the child reaches them? And if they are rigid, what uterus can force its contents through them? Such is the mechanical spirit of the age, that the student is more apt by far to regard the os uteri as an India-rubber ring, than as a sphincter muscle governed by the same general laws which govern similar structures in other parts of the body. To our mind it appears quite as rational to say, that the morsel of food which is about to pass from the æsophagus through the cardiac orifice of the stomach is *pushed* through by the various muscular fibres of the æsophagus, as to say that the various muscular fibres of the uterus *push* the child through the undilated os uteri. No, nature has so ordained it, that a proper stimulus causes these various gates to open, or at least to *unlock* themselves, and thus any thing that impinges against them can pass through: it matters not whether it is a bone or a spoonful of water which is to pass into the stomach, or whether it is the head or breech of the child that is to pass through the os uteri. There is a proper stimulus, however, and as sure as there is an os uteri to open, the "bag of waters" is this stimulus in labor. Men may blind themselves if they will, by recording the exceptions to the rule; but Nature placed the "bag of waters" at the door of the uterus to gently arouse the doorkeeper, physiology, when the proper time arises for the release of the prisoner, and if any ruder summons is made—as when, if the waters be accidently evacuated, the head comes in contact with the parts—the doorkeeper rebels. The only difference that we can see between this part of the functions of the lips of the uterus and other similar structures is, that its process is, as a rule, conducted more slowly. Still, the individual who practices obstetrics for five or ten years, will not fail to meet with cases of really sudden dilatation of the os in labor. More than once have we been wearied in waiting on a rigid os uteri, and at last find that relaxation, and really complete dilatation would take place suddenly. Again, who has not seen a soft and yielding os uteri suddenly become rigid, under the influences of mental impressions made on the woman; and then, when the excitement has been allayed, this same os relax and dilate as suddenly? Is all or any of this dependent on the mechanical efforts of the "cervico-fundal," or any other propulsive fibres of the uterus? No, we must look to the still incomprehensible laws of vitality for such phenomena. When the door of the uterus is opened, then the propulsive muscular fibres urge the ovum through, but these fibres have no agency in opening that door. This is the great physiological rule in natural labor, and no array of exceptions taken from the catalogue of difficult labors can ever tend to subvert it. A man may, when necessity seems to demand it, apply a pair of forceps and really stretch a head through an os uteri not sufficiently dilated to admit of its passage under the influence of the uterine efforts, but this, and a thousand other like exceptions, can never prove that nature intended the os uteri to be mechanically stretched in every labor.

Again, what man of experience is there who would not freely acknowledge, that the premature rupture of the membranes, and the consequent impinging of the head or breech against the undilated os, almost certainly causes a tardy labor—the labor being made tardy because the os dilates slowly? As a practical proposition it stands undisputed: every obstetrician desires to keep the membranes intact until the os is dilated—until the door is not only unlocked, but is actually thrown pretty widely back on its hinges. And why all this? Simply because the "bag of waters" is the natural stimulus, is a *better* stimulus than the head or breech. Nature responds readily to the call of the one; she rebels at the other.

But we now come to the great point of Professor Richardson, viz: "that one of its most important uses (the amniotic sac) remains unfulfilled until the presenting part has passed through the superior strait; and that in all natural, *uncomplicated* labors, especially primiparæ, the membranes should not be ruptured at any time."

Again, and to make his meaning more evident to the reader: "protraction and injury are liable to result, whenever the liquor amnii is evacuated *before the head is properly adapted to the pelvic orifice*, no matter what degree of dilatation may have taken place."

In other words, Dr. R. thinks the liquor amnii exerts the prime influence in producing the first flexion of the head and bringing its short diameters into relation with the diameters of the pelvis. We hardly know how to express our surprise at such an idea. When we reflect that it is the very phenomenon of the child's *floating* in the liquor amnii—the walls of the uterus being thereby prevented from coming in contact with it—which causes the head, through the unerring laws of gravitation, to present with its long diameter (*occipito-frontal*) threatening to engage in the pelvis, we are totally at a loss to imagine how these waters can exercise the contradictory influence of *flexing* the head, and thereby adapting its diameters to the pelvis. The waters must do one or the other; they cannot do both. Hitherto flexion of the head has been regarded as the result of the head coming in contact with the pelvic brim, under the influence of the expulsive throes of the uterus, and all absolute practical observation has shown that in real normal labor (we now and forever exclude exceptions) these expulsive throes come on after the evacuation of the liquor amnii. Let the accoucheur examine a case wherein the liquor amnii is present, and grant that through the membranes he detects the head in a state of extension, the occipito-frontal diameter threatening to engage in the superior strait. Say that the os is dilated and all the soft parts prepared for the delivery of the child; then let him rupture the membranes, (the water being allowed to flow off), and on reapplying his finger to the head he shall find that the head is *flexed* and its short diameters are brought into proper relation with the pelvis; in other words, as the waters flowed away the head came to impinge on the pelvic brim, and the forehead being farther away from the point of union of the head and spine than the occiput, it was pushed upward, and, as a result of this, the occiput fell downward. Flexion, then, is a mechanical process, and is the result of the head and pelvis coming in contact—the uterine walls interposed, of course. If flexion is the result of this mechanical contact, it is plain that anything which tends to prevent the contact being complete will necessarily retard flexion. The liquor amnii is floating the child; the uterus, when it contracts, spends its force on the liquor amnii, and, so far from the liquor assisting the process of flexion, it must positively act injuriously; and this is the position hitherto held by obstetricians. Once the os uteri is well dilated—or, we say, considerably dilated and thoroughly *dilatable*, unlocked,—and the parts below in proper condition, the function of the liquor amnii, in labor, has ceased: to retain it is to retard labor. But a few months ago we were called to a woman, who had been laboring for more than forty hours in charge of a midwife. Mentally and physically she was, to use her own expression, "worried out." We examined her: the os was well dilated and the head presenting in a first position at the brim, but still extended. We ruptured the membranes; the very first pain (which followed promptly) flexed the head, and the child was born in less than five minutes. The membranes were tougher than any we ever before encountered.

Now, acting on the theory of Dr. R., we should have waited here for "complete flexion, and the adaptation of the sub-occipito-bregmatic circumference of the head to the pelvic brim." But the midwife says that the os was flaccid and open full thirty hours before we saw the patient; and yet Dr. R.'s "complete flexion and adaptation" did not occur; and our conception of the mechanical laws which govern this part of labor tells us that such would never have taken place while the membranes remained intact. This is one of the cases (and they constitute the large majority—the rule, really,) where the tone of the uterus and its attachments held it well up at the superior part of the pelvis, and not one of those with which all practitioners will occasionally meet, viz: where the uterus, with its contents, (child, liquor amnii and all,) comes dipping low into the pelvis, and, of course, as the head engages in the brim, flexion takes place. But, even in these latter cases, who has not found a flaccid os uteri, with the membranes entire, close to the vulva, and the woman laboring in vain? Then rupture these membranes, and who has not seen all hearts made glad by the speedy and happy termination of the labor? Shall any man tell us, then, that in either of the cases cited we shall part with one of the most valuable practical privileges we have to render both woman and child a signal service, on the mere hypothesis that "complete flexion and adaptation of the sub-occipito-bregmatic circumference of the head to the pelvic brim is by far the most tedious step in the mechanism of a primipara labor, usually requiring many hours for its accomplishment?" On the contrary, does not a study of the pelvis and the fœtal head, and then actual practical observation, teach us that this flexion is a simple process, and is really the work of but a few moments?

As to the idea of Dr. R., that, when the waters are artificially let off, before flexion is complete and the child ready to pass on, the "circular fibres of the body, meeting with the least resistance, contract upon and constrict the abdominal region of the fœtus," and thus prevent the child from gliding out of the uterus, we merely mention it: One moment's reflection will show the reader that this abdominal region, in the beautiful arrangement of the fœtal ovoid, is well fortified against any such attack of the "circular fibres" by the arms and legs which are thrown before it. With the arms and legs before and the spinal column behind, the child is safe from these "tender embraces" of the circular fibres.

Again, Dr. R. seems to think this process of flexion much more slowly accomplished in primiparæ than in multiparæ. Why, we are at a loss to conceive. How the *mechanism* of labor can vary in the two cases, we cannot imagine. That the physiological functions should not be so readily performed in a first labor, we can conceive. As to the practitioner being deterred from rupturing the amniotic sac where it is indicated, as we have shown, because a woman *may* thereafter have convulsions, or hemorrhage, or syncope, (said complications being *post hoc*, but not *propter hoc*,) and version become necessary, we cannot think it a question of serious import. The point is, is the labor retarded on account of unusual tenacity of the membranes? Then, is the way all clear for the delivery of the child after the membranes shall have been artificially ruptured? If there is an affirmative answer to these questions, then artificial rupture is indicated and should be practiced. To delay the operation because this, that, or the other complication *may* thereafter occur, is begging the question.

Dr. R. concludes his articles by reminding us of the "statistical fact, established by the observations of Burns, Collins, Naegele, Simpson and others: *that the liability to injury and death, upon the part of mother and child, increases proportionately with the duration of labor.*" To which we heartily respond, Amen! The only difference between Dr. R. and ourself is, that he *apprehends* difficulties that *may* occur to retard labor after the artificial rupture of the membranes, while we find in preternaturally tough membranes the direct and palpable *cause* of the duration of labor being increased. Of course it would seem preferable to pursue the substance rather than the shadow.

Better reasons than those given by Dr. R. must be advanced, then, before we can ever consent to abandon the good old practice of rupturing tough membranes in labor. Having so often had cause to congratulate ourself on the happy result of this procedure, and having, at the same time, never seen anything like evil result, we must continue to advocate the practice most strenuously. Of course the *abuse* of the practice can never be used as an argument against it. The point of issue is, that præternaturally tough membranes do retard labor. Then, all reason and proper experience ring in our ear the good old maxim—*sublata causa, tollitur effectus.* We act under this maxim, and all goes well.

MONSTROSITIES.—Our city has been blessed (!) with opportunities this season for occular demonstration of the strange freaks Dame Nature takes in moulding us poor mortals. We have had "the bearded woman," with as fine a pair of whiskers as any man would care to sport, and her little hirsute son, Esau. Then the Siamese twins, of world-wide reputation, and whom we remember to have seen when we were a boy. But stranger than all is "the double-headed child," or "North Carolina twins," now on exhibition at Vannuchi's Museum. Through the kind courtesy of the owner and exhibitor of these children (for they are plural) we have been permitted, together with several other medical gentlemen, to examine them professionally—if we may be allowed the expression.

We find two little negro girls, (Milly and Christina,) about six years of age, of fine physical and intellectual development, active and as merry as crickets, firmly united by apparent fusion of the sacral bones throughout their entirety. Christina is rather larger than Milly, but they are very much alike, and there seems to be no difference in intellectual development. They are active, and their ordinary mode of locomotion is sideways; but Christina, the larger can walk directly forwards, and then Milly follows her backwards and with perfect facility. They can walk up and down stairs with ease, (in fact, they can *run* down stairs,) and can jump from an eminence of a foot or two. They sit in the same chair quite gracefully, kiss each other readily, and, indeed, seem to be so little in the way of each other, that sympathy for them is lost in wonder and admiration. He or she who feels rebellious against the troubles of this world, has but to look on these twins and learn the power we possess to accommodate ourselves to circumstances.

We regret exceedingly that the owner would not permit a close examination of these twins by means of probe and catheter, but as far as we could see their connection seems to be as follows:

First. There is apparent fusion of the sacral bones, and we infer fusion of the sacral nerves, too, as when one child is pinched anywhere below the hips the other feels it; but there seems to be no more intimate nervous connection of this sort, as a pinch inflicted above the hips is only felt by the one pinched.

There is one perfect and one rudimentary anus—showing a direct communication between the lower part of the intestinal canals, at least. Still, the nurse (a very intelligent mulatto woman) says that the desire to defecate is rarely felt by both at the same time. One will express the desire now, and a few hours after the other makes the call.

Each child has the genital fissure quite well marked, and each has two labia majora (the same being fused at the median line between the children) and a rudimentary clitoris; but there is but one nympha on each side—that is, to each child.

At the median line there are two small circular openings (with a septum between them), each having all the appearances of a meatus urinarius. It is here that we were denied the privilege of probing or catheterizing, and all we can say is that we believe these openings belong to separate urinary bladders. The nurse says that the desire to urinate is as independent as the desire to defecate.

There is no vagina, as far as we could ascertain by occular demonstration.

In connection with this incomplete report, we furnish our readers with the following description of another freak of nature, clipped from a late exchange:

"A VARIORUM EDITION OF THE SIAMESE TWINS.—At the meeting of the Academy of Medicine of the 28th ult., a most curious phenomenon in embryogeny was communicated by Dr. Depaul. A woman, aged twenty-nine, already the mother of five children, living in the Rue des Poulies, at St. Denis, has given birth to a double monstrosity. It consists of twin children, joined together, and in some respects resembling that of the Siamese twins, but differs from it in other points of physiological detail. These twins are united, not by the side, but in the median line, from the umbilicus to the pubis exclusively. This prevents them from being suckled naturally; for it were impossible to give the breast to one without stifling the other. They are fed, therefore, from the bottle. These two children offer, as we have said, striking physiological differences. Thus the one has blue eyes, the other brown. They do not sleep at the same time, and it happens but too often that the one begins to cry when the other falls asleep; the immobility of the one seems to excite in the other wilful impulses of locomotion and of turbulence. In one word, and contrary to what was seen with the Siamese twins, who appeared to have but one will, and simultaneously performing the same acts, these twins are in constant opposition to each other."—*Lancet, November* 14, 1857.

DEATH OF DR. DANIEL BLAIR.—The January number of the British and Foreign Medico-Chirurgical Review announces the death of this most worthy member of our profession. Dr. Blair is dead, but his monographs on Yellow Fever will live as long as medical literature shall be preserved by men. Dr. B. died in November last, in British Guiana, "on his return from an exploring expedition into the interior of that wild country."

THE SYDENHAM SOCIETY.—The same Review announces the death of the Sydenham Society, but consoles us by announcing the fact that great efforts are being made to reconstruct the same, and to make it even more useful than it has hitherto been. A long list of valuable medical books and monographs are proposed for republication. With the Review we say: "Le Roi est mort, vive le Roi!"

PACIFIC MEDICAL AND SURGICAL JOURNAL.—We are in receipt of the January, (1858,) number of this new Journal. A short time ago we announced the death of the California State Medical Journal, and we were afraid that it might be long before another medical periodical could be established in the golden State; but both industry and talent are to be found there, and we see the evidences of the same in the number now before us. We wish the editors, Drs. Trask and Wooster, all the success they can desire, and welcome them to the corps editorial and our exchange list.

ATLANTA MEDICAL COLLEGE.—We beg to call attention to the advertisement of the Fourth Course of Lectures in the Atlanta Medical College. At the same time we must not omit to mention that the faculty have recently announced the fact that Professor Alexander Means has withdrawn his resignation of the Chair of Chemistry, and will lecture in the institution as heretofore.

REPRODUCTION OF BONES AND JOINTS AFTER THEIR REMOVAL IN IN CASES OF WHITLOW.—Some time ago Dr. Toland, of California, claimed the discovery of this important fact in surgery. In the February number, 1858, of the Buffalo Medical Journal, we find, however, that to Professor Dudley, of Lexington, Ky., is due the credit of having made the discovery, and to Prof. Hamilton, of Buffalo, N. Y., is due the credit of having first promulgated the idea through the medium of the medical journals. Doubtless we shall soon have some more discoverers in the field. But this is all right. We say, "honor to whom honor is due."

During five years practice in the country, it was our lot to encounter a great number of these cases of paronychia among the plantation negroes. Indeed, strange as the idea may seem to some, we are sure of having witnessed the disease once in an *epidemic* form. Throughout an entire neighborhood the disease was strictly prevalent, and we can now call to mind several negroes who lost the first phalanx of two or more fingers in one season. Of course the vast majority of the cases lost the first phalanx, as every overseer and old woman in the country imagine themselves fully competent to treat, or rather *cure,* whitlow or bone felon. When the doctor is called in, the bone is loosened from its attachments, and he has no alternative but to remove it. When we first encountered the cases, such was the degree of disease of the surrounding tissues, we amputated the end of the finger; but observation soon taught us that this procedure materially diminished the value of the cotton-picker, as the finger was not only considerably shortened, but the stump, unprotected by the nail, was continually subject to injury. We then resorted to the plan of picking out the dead bone, and the only deterioration the hand suffered was comparatively slight shortening of the finger. In no instance have we ever seen any thing approaching reproduction of the bone—and this notwithstanding we have carefully supported the finger by means of splints and bandages.

A New Medical School at Nashville.—We learn that the brilliant success of the University, has prompted a new enterprise in the way of a Medical School at Nashville. We understand that it is to be under the patronage of the Methodist denomination.—*Western Lancet.*

Contribution to the Museum of the New Orleans School of Medicine.—To Dr. T. A. Sheppard, of Fort Adams, Miss., the faculty are indebted for a fine specimen of anencephalus.

A New Journal of Physiology.—We learn from our exchanges that Dr. E. Brown Sequard, has issued a new Journal of Physiology, at Paris. He is assisted by MM. Robin and Thologan. The journal is issued quarterly—200 pages in each number—and will contain original articles and analyses of researches in physiology in all parts of the world. Mr. John Pennington, of Philadelphia, will receive subscriptions: price $5 per annum. Such an enterprise must surely succeed.

Medical Students at Home and Abroad, 1857–'58.—In London, 1050; in Paris, 1027; in Dublin, 573; in Charleston, S. C., 216; St. Louis Medical College, 121; New Orleans School of Medicine, 126.

Cæsarean Section has recently been performed twice in Paris—one case successful, both mother and child surviving.

"ANOTHER DEATH FROM CHLOROFORM.—A gentleman by the name of McChesney, died suddenly in Toronto, February 1st, in a dentist's chair, after taking chloroform for the purpose of having teeth extracted."
[*Boston Journal.*

THE IOWA MEDICAL JOURNAL comes to us, after a long absence, three numbers in one, January, February and March. The editors say, that for the present their journal will only appear quarterly.

STATISTICS OF MORTALITY IN NEW ORLEANS FOR THE YEAR 1857.—From the very excellent mortality tables arranged by Dr. H. D. Baldwin, Secretary of the Board of Health, we make the following extracts:

Whole mortality for 1857, 6,067. Men, 1,753; women, 1,010; boys, 1,761; girls, 1,533; colored, of both sexes, 1,096.

Nativities.—Africa, Austria, British America, Belgium, Denmark, East Indies, England, France, Germany, Ireland, Italy, Madeira, Mexico, Norway, Poland, Portugal, Prussia, Russia, Scotland, South America, Spain, Sardinia, Sweden, Switzerland, United States, West Indies.

Some Causes of Death.—Cholera, 29; Yellow Fever, 199; Scarlet Fever, 86; Trismus Nascentium, 199; Consumption, 661.

To these may be added: Stillborn, 398! Then add to these the 199 cases of Trismus Nascentium, and we have a total of 597 deaths, of which, according to the report of the President of the Board of Health, three-fourths of the stillborn, and three-fifths of the Trismus cases occur in the hands of the ignorant midwives.

It will thus be seen that about one in every nine deaths occurs from Consumption; about one in every fifteen falls under the head of Stillborn; and about one in every thirty occurs from Trismus Nascentium.

Subtract, then, as unnecessary deaths, half the Stillborn and half the Trismus cases (in all amounting to 298); and then deduct one-half the Consumption cases, as being deaths occurring amongst persons who have contracted the disease elsewhere, and who really come here only to die, (being 330,) and we have a total of 628 to deduct from the total mortality of 6,067—thus leaving us something like a fair total mortality of 5,439 for the past year. Assuming our population, then, to be about 170,000, (and this is thought to be a very fair estimate,) and we have a mortality of about one in every thirty-one of the inhabitants.

Report of the Board of Administrators of the Charity Hospital, to the Legislature of the State of Louisiana, January, 1858.—Another year has rolled away, and we are called on to record a synopsis of the statistics of the great Charity Hospital of our State. What medical man can read the following, and not see in it all the best practical school for the student of medicine to be found anywhere in the land?

This institution is free to the poor sick of all nations. It is called the Charity Hospital of our State, but the sick of our State form but a small proportion of the thousands who partake of its benefits. The income of the institution is derived from taxes laid on foreign immigrants, and on balls, concerts and theatres. The tax on immigrants for the past year has amounted to $30,201.75, an increase of $2,000 on the year previous. The tax on balls, etc., has fallen off in the past year from $8,189.92 to $5,480—and this in consequence of a reduction by the last Legislature of the rate of taxation. The cost of supporting the institution the past year was $84,803.11. Deficits in the treasury are generally promptly made up by appropriation on the part of the State.

During the year 1857 there were admitted into the institution 8,897 patients, of whom 7,913 were discharged, and 1,017 died.

Nativities—United States, 1,577; Foreign Countries, 7,307. Of the former number, 394 were from Louisiana, 241 from New York, and 132 from Pennsylvania. Of the latter number, 4,010 were from Ireland, 1,197 from Germany, 664 from France, 347 from England, 297 from Prussia, and 139 from Switzerland.

Monthly Mortality.—January, 761 admissions and 89 deaths; February, 650 admissions and 68 deaths; March, 632 admissions and 67 deaths; April, 605 admissions and 65 deaths; May, 544 admissions and 57 deaths; June, 586 admissions and 66 deaths; July, 655 admissions and 66 deaths; August, 705 admissions and 53 deaths; September, 847 admissions and 81 deaths; October, 1,085 admissions and 172 deaths; November, 952 admissions and 145 deaths; December, 905 admissions and 88 deaths.

Sexes.—6,848 males, and 2,049 females.

Diseases, including wounds, etc.—396 varieties, too tedious to recapitulate here.

Obstetric.—Births, 110—males, 79; females, 31. *Presentations*—Cephalic, 94, of which 92 were 1st, one 3d, and one 2d positions. Breech, 2; feet, 1; hands, 1; head, with umbilical cord around the neck, 1; uncertain, 1.

Of these 110 births, *thirty-one*, or *one in every three and a half*, were *stillborn!*

The number of patients remaining in the Charity Hospital, on the 1st January, 1858, was 572.

AMERICAN MEDICAL SOCIETY IN PARIS.—[It gives us great pleasure to publish the following paper, just received from Paris. As editors of this journal we can say, that we have been forwarding our numbers to the Society regularly since the appearance of the first appeal of the Society.—EDS.]

HALL OF THE AMERICAN MEDICAL SOCIETY IN PARIS,
January 12th, 1858.

To the Editors of Medical Journals and Periodicals in the United States:

GENTLEMEN: It is now upward of six years since a number of American Physicians in Paris united to form an association under the title of the "American Medical Society in Paris."

Under Article II of the Constitution, which was then adopted, entitled "Of the Objects," the first and third clauses read as follows:

"The objects of this Society shall be:

"First. The formation of a Library, especially of American Medical literature, which shall be freely open to the scientific men of every country.

"Third. The diffusion of American contributions to Medical science."

The Society was early impressed with the fact, which is apparent at the first glance, that the most important means for facilitating both these objects, the extreme utility of which cannot be doubted, was the regular reception by them of the journals and periodicals, wherein the daily progress of Medical science in the United States is chronicled. Owing, however, to the peculiarly and necessarily variable character of its numbers and resources, its members felt unwilling to impose upon those who should follow them, and who might find it in a much less flourishing condition than it then was, so heavy a responsibility as the subscription to even a small proportion of the journals in the United States would entail.

They, therefore, through their Corresponding Secretary, made an appeal to the editors of the profession in America to furnish them free of expense the respective journals which they conducted. This appeal, doubtless in view of the national character of the undertaking, was promptly and generously responded to, and for a time the table of the Society was well covered with American publications. These, however, have gradually been discontinued, until at present there are but two which are regularly received. Being fully aware that this discontinuance of your favors is owing to the fact that the existence and importance of the Society have not been recently sufficiently kept before the eyes of the profession at home, and relying upon your already experienced generosity, as well as in view of the fact that valuable periodicals are now in successful operation which at the time of this first appeal were not in existence, the Society, at present earnestly resolved to do all in its power to fulfill the designs of its founders, has instructed its Corresponding Secretary to renew the request, and to forward it to five of the prominent journals in the United States, requesting its insertion. Gentlemen wishing to accord this favor will please forward their journals to Mr. Paul Bassange, 20 Beekman street, New York, who has generously offered to transmit, free of expense, all books and papers intended for the Society.

BENJAMIN LEE, M. D.,
Corresponding Secretary.

BOOKS AND PAMPHLETS RECEIVED.

"The Principles and Practice of Obstetrics; including the treatment of chronic inflammation of the uterus, considered as a frequent cause of Abortion. By HENRY MILLER, M. D., Professor of Obstetric Medicine in the Medical Department of the University of Louisville. With illustrations on wood. Philadelphia: Blanchard & Lea, 1858."

"A Manual of Medical Diagnosis; being an analysis of the signs and symptoms of disease. By A. W. BARCLAY, M. D. Cantab. et Edin., etc. Philadelphia: Blanchard & Lea, 1858."

"Medical Opinion in the Parish Will Case. By PLINY EARLE, M. D., formerly Physician to the Bloomingdale Asylum for the Insane, etc. Extracted from the printed documents in the case. New York: John F. Trow, Printer, 1857."

"A Brief Description of the Yellow Fever, as it prevailed at Mt. Pleasant and in Charleston Harbor, during the summer of 1857. By R. A. KINLOCK, M. D. Extracted from January No., 1858, of Charleston Medical Journal and Review." From the author.

"Catalogue of the Medical College of the State of South Carolina, in Charleston. Session 1857–'58."

MONTHLY REPORT ON CHEMISTRY, ETC.—*By I. L. Crawcour, M. D., Professor of Chemistry and Medical Jurisprudence, New Orleans School of Medicine, etc.*—A question of considerable importance has recently been asked in Paris, viz.: as to the possibility of cigars containing arsenic causing death. It appears that some little time ago a priest died suddenly, and his death was attributed to another priest, who gave him a (supposed) poisoned cigar to smoke. Now, can arsenic be volatized in this manner, and mixing with the smoke, be absorbed by the lungs or by the saliva, and thus enter the system? The following are the conclusions come to by those who have investigated this subject:

1. There are four modes by which arsenic can be introduced into the system by means of cigars; the cigar may be impregnated with a saturated solution of arsenic—the arsenic may be placed in the end to be ignited—it may be introduced as an impalpable powder into the buccal extremity of the cigar, so that it shall thus gradually mix with the saliva, or the arsenic may be contained in the middle of the cigar, and thus gradually be introduced into the organism.

2. Under any circumstances the amount of arsenic which enters the mouth of the smoker is very small. In the first and second modes poisoning is barely possible, but the third method, viz., the placing of arsenic in the buccal extremity, might lead to dangerous results.

3. Poisoning is possible if we suppose all four methods to be employed, and there is also a possibility that arsenuretted hydrogen may be produced—as an ounce of boiling water will dissolve nearly a drachm of arsenic, and as a cigar will absorb fully half an ounce of this water, which, when evaporated, will diffuse the arsenic (thirty grains) through the substance of the cigar, I can see no reason why poisoning should not be produced, as the person smoking the cigar will allow fully four-fifths of this arsenic in the vaporized condition to traverse the lungs.

Mr. Saint Leger considers the phosphate of lime, as ordinarily employed, inert. The following are his conclusions:—"we have three forms in which this remedy may be administered—as calcined bones, or the officinal phosphate of lime—as precipitated phosphate, and as pulverized bones without previous calcinations. It is necessary that a remedy destined to repair osseous waste should enter the circulation easily, and should therefore be very soluble in the intestinal fluids. Mr. Saint Leger digested at a temperature of 36° cent., equal quantities of calcined bones, of non-calcined bones, and of precipitated phosphate of lime, in similar solutions of citric, tartaric, ascetic, and lactic acids. He observed that the organic acids dissolved only a minute quantity of the carbonate of lime of the calcined bones, the phosphate remained untouched; the non-calcined bones readily abandoned their calcareous phosphate; the precipitated phosphate dissolved promptly in the acids when recently prepared—after drying it dissolved only in the same proportion as the calcined bones. Mr. St. Leger, therefore, concludes that the calcined phosphate should be abandoned, and only the recently precipitated phosphate in pounded bones be employed; as the stomach always contains muriatic acid, in which the phosphate of lime in every form is very soluble, the above remarks seem to me an unnecessary refinement. It should never be forgotten, that the stomach is not a laboratory, but is emphatically under all circumstances a *stomach.*

Quinine is frequently adulterated with many substances, and it is advantageous to the country practitioner to have some rapid and easy mode of distinguishing these impurities. Mr. Boettizer states, the most common adulterations are plaster, sugar, salicine, quinoidine, and sulphate of cinchonine. Salicine is detected by saturating a small quantity of the suspected quinine with sulphuric acid, a beautiful red coloration is produced, if there be present only one per cent. of salicine. To prove the presence of cinchonine, introduce into a test tube a small quantity of the suspected salt, add a small quanity of pure ether, dilute with one-fifth of its bulk of aqua ammonia and agitate—if cinchonine be absent, the whole is dissolved—if it be present, a white caseous residue proves the existence of the alkaloid. The same deposit is produced if quinoidine be present.

It is well known that if sulphur be heated to about 300° Fahr., and then be thrown into water, it acquires the consistence of wax, being very pliable and tenacious, gradually hardening by exposure. A new application of this property has recently been suggested, viz., as a filling to carious teeth. If this soft sulphur be introduced into the cavity, it can be perfectly adapted to it, fills it entirely, and in the course of a few days, becomes as hard as marble, and perfectly indestructible.

LIST OF PAYMENTS.—*To March* 1*st*, 1858.—Dr. Weatherly, $5, 1857; Lewis, $5, 1857; W. W. Perry, $13, 1855–'56–'57; W. J. Moore, $10, on account; W. A. Smith, $10, 1856–'57; J. T. Metcalfe, $5, 1857; E.

R. Mordecai, $5, 1858; D. R. Fox, $10, 1856–'57; E. Banks, $10, 1857–'58; C. Clement, $10, 1856–'57; I. M. W. Picton, $5, 1857; F. M. Oatis, $10, 1856–'57; L. Johnson, (Algiers,) $5, 1857; R. O'Neal, $5, 1858; T. O. Meux, $5, 1857; J. M. Haden, $10, 1857–'58; W. B. Lindsay, $5, 1857; T. D. Hall, $5, 1857; J. A. Rawls, $5, 1858; D. T. Taylor, $5, 1857; Atlanta Medical College, for advert., 1858, $10; University of Louisville, for advert., 1857, $5

EXCERPTA.

THE ABORTION CASE.—*The Parties held to Bail in* $10,000 *Each.*—"On Saturday, Mr. Jas. W. Temple and Dr. Jas. Swanze, were taken before Judge R. S. Wilson, of the Recorder's Court, on a writ of *habeus corpus*, and after hearing testimony and argument in the case, the Judge decided to admit them to bail in the sum of $10,000 each to answer the charge of manslaughter.

"The medical testimony in the court was in the highest degree conflicting, illustrating the old adage that 'doctors differ.'

"Professor D. Brainard testified to the effect that the production of abortion by mechanical means which rupture the membranes, was not at all dangerous or injurious to life, if proper care were taken of the person subsequently. He said he considered that all the leading medical writers sustained him in this position. A patient who had advanced as far as the third month of pregnancy, would be no more injured by the production of an abortion than she would have been by allowing the full period to pass, and the child to come into the world in the natural way. He stated that not one case in two hundred, where abortion was produced at the third month of pregnancy, would prove fatal; but to make it safe, he would say one case in one hundred.

"Prof. N. S. Davis, on the other hand, testified that he looked upon the production of abortion by mechanical means as in the highest degree dangerous, and also as highly injurious to the system in its subsequent effects. Out of twenty cases of abortion that came under his knowledge, he could not recollect a single case where permanent and serious injury was not the result.

"The question arises as to which of the learned and talented Professors is right, for certainly their testimony is widely different."—*Chicago paper.*

The case here referred to, in which this testimony was given, occurred in Chicago, and was one of those of so frequent occurrence as to have ceased to startle the communities in which they occur from novelty of the crime; and commonly excite more of morbid sympathy and commiseration for the parties concerned, than any healthful expression of opinion and decided action towards meteing out justice to the guilty ones.

Such occurrences, indeed, excite no such interest as a beer-house row or a street garroting, though it must be evident to any impartial and reflecting individual that the crime is as much more heinous and indicative of a degraded sense of morality and responsibility, by as much as the iniquity of a cool, deliberate assassination of an innocent, unresisting and utterly helpless being, with the danger of a double murderer, exceeds that of the unpremeditated angry blow given in the heat of passion, which makes a thoughtless man an unwilling and repentant murderer; or as it exceeds the guilt of the goaded victim of avarice and guilty passions, who becomes the bold robber of a man's valuables while he spares his life.

We said that these crimes have ceased to startle from their novelty, and even while we are writing this, our own papers contain the report of a similar case in our own city, which has resulted in the death of both mother and child.

The object of this paper, however, is not to remark upon particular circumstances of this case, nor has it reference to the crime of abortion in general, its danger and immorality, the legal responsibility of the parties engaged, nor to means for its suppression. Upon this subject we took occasion to express some opinions in a recent number, among which was the following: "that we did not believe any considerable number of the members of the regular profession in any manner countenanced, or even tacitly supported the popular sentiment on this subject."

Our purpose now has reference to a portion of the evidence in the above report, which, coming from a man in Dr. Brainard's position, if not very surprising, is certainly—if contrary to good teaching, as we believe it is, as Dr. Davis believed, and as the evidence of leading authors conclusively shows—deserving of notice, from the authority which his position as a teacher might be supposed to impart to him, and the influence which might thence be exercised on the opinions of junior members of the profession.

And if he is sustained in these views, as he asserts, by good authority, it is well to know it; and if otherwise, as we could scarcely attribute the expression of such an opinion, in one holding his position as a teacher, to ignorance of authorities, it must arise from other causes. And when he thus dogmatically gives an opinion so opposed to the teachings of the best, and, indeed, of all authors on the subject, as we think we can show, we are inclined to ask ourselves if we uttered a mistaken opinion, that very few members of the regular profession in any manner countenanced, or even tacitly supported, the popular sentiment on this subject—at least so far as regards the danger of the accident; for this is evidently supporting the popular sentiment as regards the danger; and if we find such opinions amongst the teachers, how much more shall we not amongst the pupils; and, indeed, we fear there are no small number who construe their ideas of the immorality of the business according to their views of the danger.

But we will not judge Dr. B. without evidence. What are the statements of the authorities on this subject? and do they sustain Dr. B. in his statement, "*that the production of abortion by mechanical means which rupture the membranes was not at all dangerous or injurious to life, if proper care were taken of the patient subsequently?*" and again, "*that a patient who had advanced as far as the third month of pregnancy, would no more be injured by the production of an abortion than she would have been by allowing the full period to pass, and the child to come into the world in the natural way.*"

Although this accident is so very common, occurring frequently in the experience of every practitioner—so frequent that, according to Dr. Whitehead's* statistics, of 2000 cases of pregnancy, one in seven terminated in abortion—yet there is no want of evidence from good authorities to show that it is not without danger, even when resulting from natural causes. It is not without immediate danger to life, though the greater danger arises from the more remote but nevertheless direct sequences of the accident, which, from the fact of their remoteness and chronic course, are less liable to be regarded as the consequences; and hence arises the idea of innocuousness.

Thus says Ramsbotham on this subject :†

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* Whitehead on Abortion and Sterility.

† Ramsbotham's Process of Parturition, p. 487.

"Although, however, the discharge is the only symptom which need produce immediate alarm, abortions, as is well observed by Denman, especially if repeated, may either occasion local disease, or the time of an abortion is an era from which we may date the commencement of some *dangerous diseases of the uterus or its appendages.*"

"Abortion not unfrequently is also followed by hysteritis, inflammation of the uterine veins, and some of the other inflammatory diseases of childbed;" and again:*

"The principal, if not the only source of peril, is loss of blood."

This is, indeed, the great source of danger at this period, (third month,) at which time Dr. B. states that a patient, "would no more be injured by the production of an abortion than she would have been by allowing the full time to pass, and the child to come into the world in the natural way."

Says Churchill on this point:†

"It is always an untoward event, but not to be considered *dangerous*, unless accompanied by great hemorrhage;" and again:‡

"Generally speaking, the flooding is less the nearer gestation is to completion."

And thus Cazeaux:§

"Whence I conclude that an abortion is then (third or fourth month) more *grave* and *painful* to the patient, as also more *dangerous*, than in the fifth or sixth."

Colombat|| also remarks as follows:

"The prognocis of abortion regards both mother and child; for the former is generally *more dangerous* than labor, because the latter is the performance of a natural function, while miscarriage is a disease."

And thus Bennett:¶

"Abortion is often occasioned by inflammatory ulceration of the cervix uteri, and often occasions it."

And thus Lisfranc:**

"Abortion in particular has been designated as a frequent, and even as a formidable, cause of organic affections of the uterus."

Such are the opinions of some recognised authorities, in regard to the danger of abortion resulting from natural causes. If such danger exists in

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* Ramsbotham's Process of Parturition, p. 487.

† Churchill's Midwifery, p. 180.

‡ Churchill's Midwifery, p. 184.

§ Cazeaux's Midwifery, p. 265.

|| Colombat on Diseases of Females, p. 591.

¶ Bennett on the Uterus, p. 321.

** Lisfranc on the Uterus, p. 85.

natural abortion, how much more must it be in those artificially induced. On this point, we will also take the evidence of "leading medical writers," and first that of statistics, such as we are enabled to adduce.

Churchill collates from various authors 706 cases in which premature artificial delivery was effected for various causes. In these, death to the mother was the result in twenty-five, and from the means made use of, in twenty, or about one in thirty-five.

On this point says M. Chailly:*

"If we consult the statistics published in 1838, by M. Stolz, it will be seen that in 211 premature artificial deliveries, more than one half the children survived, and *scarcely one mother in fifteen died.*"

These statistics are also adduced by the authors to show the comparative safety of this method of procedure. They were, too, cases occurring in the hands of teachers and recognized authorities in medicine, and against whom, therefore, the charge of incapacity and neglect of proper subsequent care could not be brought.

There is no little difference, it will be observed, between one death in fifteen according to Stolz, and one in thirty-five, according to Churchill, and one in two hundred, or even in one hundred, according to Dr. Brainard's opinion. But perhaps Dr. B. may object to these facts, that here the operation was performed late in pregnancy, after quickening, with the view of affording a chance for a viable child, and that the danger was proportionably increased as the pregnancy had advanced towards the full term, and that at or about the third month the danger would be far less. Now, if this were so, is it probable there should be as much difference in the danger attending the operation about the third month, and that after quickening up to the seventh, as would be indicated by these figures? But, however much less the danger may be in natural abortions at this early period, and we have shown that even this is not admitted by all good authorities, still in artificially, and especially mechanically, induced abortions at this period the case is widely different. Here the danger is increased, as appears very evident, by just as much as the difficulty of safely manipulating with instruments about the undeveloped os and cervix at this early period exceeds that of manipulating in a later and more developed stage. But we will show what this danger is from the best authorities.

Thus says Cazeaux† on this point:

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* Chailly's Midwifery, 131.

† Cazeaux's Midwifery, Am. Ed. p. 265.

"The prognosis varies with the cause of the accident. Thus the most serious of all is an abortion brought on either by internal remedies or by manipulations."

And thus Colombat:*

"The most dangerous (is) that which has been occasioned by a violent exciting without any predisposing cause."

Velpeau† makes the following statement in relation to the consequences of using instruments to procure abortion:

"Those who make use of them most frequently fail in attaining their object, and succeed only in seriously injuring the womb."

And thus again Dr. Smith:‡

"Abortion is in general injurious to health, and is seldom unaccompanied with suffering. The administration of emenagogues to force a separation of the ovum where the constitution has no tendency to throw it off, is highly dangerous to the mother. No drug can act in this way upon the uterus, but by involving it in a violent shock given to the general system. It has frequently occurred *that the unhappy mother has herself been the sacrifice,* while the object intended has not been accomplished."

And then again Dr. Burns,§ who writes as follows:

"It cannot be too generally known that when those medicines (emenagogues) do procure abortion, the mother can seldom survive their effects."

Again Dr. Michael Ryan:||

"Irritation of the cervix uteri by mechanical means and piercing the membranes justify the truth of the remark '*sæpe suos utero quæ necat ipsa perit.*'"

Translation—frequently, she who would destroy her fœtal offspring perishes herself.

And again Baitley¶ as follows:

"Every woman who attempts to promote abortion *does it at the hazard of her life.*"

And again Male,** who writes:

"There is no drug which will produce miscarriage in women who are

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* Colombat's Diseases of Females, Am. Ed., p. 591

† Quoted from Beck's Medical Jurisprudence, p. 444.

‡ Smith's Principles of Forensic Medicine, p. 295.

§ Burn's " " " p. 283.

|| Manual of Midwifery, by Michael Ryan.

¶ Baitley's Treatise on Forensic Medicine, p. 5.

** Male, Epitome of Medical Jurisprudence.

Dr. Meigs[*] writes:

"But shall a man feel justified to enter on an important operation, one admitted to be dangerous to the mother, and uncertain for the child in the proportion of 50 per cent., upon the ground of a mere peradventure?"

Dr. Beck[†] characterizes the introduction of instruments into the womb not predisposed to it, without acting violently on their system, and probably endangering their lives."
for the purpose of rupturing the membranes as *vilainous* practice, and draws the following conclusions on this subject:

1. "That all of them (means of producing abortion) are uncertain in their operations upon the fœtus.

2. "That they always endanger the life of the mother.

3. "That they sometimes destroy the mother without effecting the fœtus."

Again we quote from Farr:[‡]

"The life of the mother as well as the child is endangered by such exhibitions."

Such are the views of some of our authors respecting the danger of inducing abortion. Had we the works of other writers on this subject to refer to, we doubt not that much more corroborating testimony might be adduced.

We will now offer some evidence respecting the particular source of danger in the use of instruments for puncturing the membranes; and let our readers judge from this evidence whether this danger is not far greater at the third month than at any consecutive period; that the danger from this source will be just in proportion to the earliness of the stage of pregnancy in which these means are employed.

Thus remarks Dr. Taylor:[§]

"Mechanical means are undoubtedly more effective in producing abortion than medicinal substances; yet from the fact of such attempts being made by ignorant persons, the woman generally dies from hysteritis, peritonitis, or other serious after consequences." And again:

"It is obvious that this mode of perpetrating abortion is only likely to succeed in the hands of persons *who have a complete anatomical knowledge of the parts.*"

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[*] Diseases of Females.

[†] Beck's Medical Jurisprudence.

[‡] Beck's Medical Jurisprudence, p. 70.

[‡] Taylor's Medical Jurisprudence.

This is an admission of the danger from the operation performed by inexperienced hands. But the operation is not attempted alone by ignorant persons, as this writer seems to think, nor is it alone unsuccessful with such persons, as he also asserts. That it is not attempted alone by ignorant persons, the statistics we have quoted will show; that it is not always successful with intelligent and experienced physicians, and those having a complete anatomical knowledge, particularly in the early months, we will proceed to show.

"Thus Dr. Gooch relates that Dr. Wm. Hunter attempted this operation (introducing an instrument to puncture the membranes on a young woman at about the third month of pregnancy. He found that he several times *punctured the cervix uteri*, and the case *terminated fatally*."

"If this happened to one of so much anatomical knowledge and skill, how much more probable must it be in the hands of those ignorant men, by whom, for the purpose alluded to, the operation is sometimes undertaken. No doubt these attempts often prove fatal, but the murdered do not tell tales."

On this point, Ramsbotham also remarks:

"That the performance of the operation demands a *most accurate knowledge* of the anatomy of the ovum and the maternal structures, *as well as of the state of development which the neck ofthe uterus* assumes at different periods of pregnancy."

Now, consider that the necessity for this operation arises so very seldom, and almost never at the third month, and it is apparent that the skill derived from experience, in legitimate practice, is not easily obtained, and hence the greater likelihood of injury resulting to the cervix and os uteri.

Moreover, it is generally admitted, and has been shown conclusively by eminent obstetric writers, that morbid conditions of the os uteri, ulcerations and inflammatory conditions, are one of the most frequent causes of abortions. Hence we see why abortions would be likely to follow in these cases, at the expense of injury, perhaps permanent, to the mother, and the strong presumption that abortion in the early months, by mechanical means, is the result of such injuries to the mother rather than of injury to the ovum.

What the danger, both immediate and ultimate, is, from these injuries, should be known to every medical man, and we presume it is.

E. P. C. [*Peninsular Journal of Medicine.*

MECKEL'S GANGLION.—In the January number of the *American Journal of Medical Science*, we find a paper from the pen of Professor J. M. Carnochan, of New York, on the Exsection of the Second Branch (Superior Maxillary) of the Fifth Pair of Nerves, for the cure of facial Neuralgia. On account of the originality of the operation, its uniform success, and the heretofore almost hopeless character of this disease in certain forms, we have deemed it proper to transfer from that valuable quarterly the entire article to our own pages. The *operation* proposed, and thrice successfully performed, by the distinguished reporter, meets with our unreserved approval. If it is objected by any one that the operation is horrible, he can be answered that is to eradicate a *worse* than horrible disease, a living and unending agony, to which even death itself is preferable, and that the use of chloroform, insisted on by the surgeon, removes in a great degree the strongest objection any one can advance against it.

It is not, however, simply to signify our approbation that we have begun the present notice. Dr. C., in his remarks previous to detailing his cases, in endeavoring to account for the success of his operation when other modes of procedure had failed—*inadvertently*, we are convinced—makes the statement of an opinion in regard to certain points in the physiology of the nervous system, which to our mind, with the experimental facts before him, should have been given with at least a certain degree of hesitancy, for it involves either the correctness or the *incorrectness* of views long held as among the established truths of the science.

After presenting a rapid but a sufficiently lucid sketch of the many forms of facial neuralgia and of the various operations proposed for their relief, and after reasoning, we think very justly, on the several causes operating to produce a continuance or a recurrence of the affection after measures taken for its relief, he says: "from these views we can perceive how futile the operation of division of the nerve at the foramen infraorbitale must be. When the trunk of the nerve is extensively diseased, no operation can rationally lead to a successful result, unless all the branches emanating from the trunk are cut off from communication with the brain. I believe that in such aggravated cases of neuralgia, the key to the operation is the *removal of the ganglion of Meckel*, or its insulation, from the encephalon. Where even a large portion of the trunk of the second branch of the fifth pair has been simply exsected from the infraorbital canal, the ganglion of Meckel continues to provide, to a great extent, the nervous ramifications which will still maintain and keep up the diversified neuralgic pains. Besides, the ganglion of Meckel, being composed of *gray* matter, must play an important part as a generator of nervous power, of which, like a galvanized battery, it affords a continued supply, while the *branches* of the ganglion, under the influence of the diseased trunk, serve as conductors of the accumulated morbid nervous sensibility."

Now, the objections which might be urged against the *confident* statement of the above theory are many and various; some of which we will here venture to suggest. In the first place, it is at least *questionable*, whether or not the centres of the Ganglionic System can be productive of sensitive phenomena. Secondly: The nervous ramifications, provided by the Ganglion of Meckel, are not distributed upon parts where the "diversified neuralgic pains are kept up," and especially is this certain after the trunk of the superior maxillary has been removed; but they are distributed to surfaces and parts located *internally*, as the fauces and the roof of the mouth to the Schneiderian membrane lining the nasal fossæ, and probably conjunctiva cornea, etc.; at least, so anatomical investigation and experimental physiology have uniformly determined their distribution.

But, in the third and last place, the most important objection which now presents itself, in our mind, to Dr. Carnochan's ingenious theory is, that the results of both experiment and observation are diametrically opposed to his deductions.

The trunks and ganglia and branches, of the ganglionic system, have been the subjects of physiological experiment for a period now extending over more than a hundred years; since the year 1732, when Petit made a division of the trunk of the sympathetic in the dog, down to the present day, we find ranged along the whole interval a number of illustrious names, whose multiplied and varied experiments have only confirmed his results, and go to prove that this ganglionic system has but little agency in either sensation or motion, but has mainly confided to it the important and more silent processes of secretion and nutrition. Whenever a nervous trunk in any part of this system has been divided, in experiment, or a ganglionic centre disturbed or removed, either by the knife of the physiologist or in the progress of diseased action, there have ever been found signs of modified nutrition or altered secretion; the only modification of *sensation* we are aware of is that observed by M. Bernard and others, where exaltation of temperature takes place in the parts answering to the branches of distribution of the divided nerve, or of the ganglion which has undergone evulsion. (See *Archives Générales.*) So much for observations and experiments upon the ganglionic system *in general.* Now let us examine if any experiments can be referred to, as illustrating the effect of the disturbance of Meckel's Ganglion *in particular.* There may have been others, but recent research has made us more familiar with those of Dr. Alcock, of Dublin, reported in Todd's Cyclopedia of Anatomy and Physiology. We will premise that Dr. Alcock did not draw the same conclusions from the results of his experiments which we have considered, taking them in connection with analogous experiments on *other* portions of the ganglionic system, as unavoidable deductions. He instituted the series to determine, as he says, the function of the nerves of Taste; and yet he more plainly established, than any one had done before him, the function of Meckel's Ganglion. In order to determine the nerves of taste, he undertook the removal of Meckel's ganglion from the dog: he attempted it several times and failed at different stages of the operation; but in almost every case, the eye of the *same side* became *bleared within the next two days,* a whitish puriform matter exuded from it, in quantity proportioned to the case; and in *one instance in which the ganglion was removed,* it actually produced *opacity of the cornea* and *ulceration* in that structure.

It will be seen in the above account that the effect of the injury or removal of Meckel's ganglion was always well marked, and of an entirely *different character* from that which results from injury or section of any sensitive nerve.

Returning now to Dr. Carnochan's Explanation, in his history of the cases reported; we find in none of the incidents there related anything which indicates the occurence of any of those results which uniformly have followed upon the evulsion of Meckel's ganglion. We can account for the absence of these sequences on three different principles of reasoning:

First. That these results may possibly have obtained in a certain degree, but Dr. Carnochan, regarding them as only the effect of the injury (traumatic results) inflicted on the adjacent parts, did not deem them worthy of record. Yet still, the *removal* of the ganglion should, according to all antecedents, have produced such an amount of perturbation in the nutrition and circulation of the eye as to have compelled his attention, and if they were present he certainly would have reported them.

Second. We might reason that this removal of Meckel's Ganglion in the human subject, (not previously performed, so far as we know, by any one,) is, perhaps, not followed by the same results as when it is experimentally done in the case of the lower animals, as the dog, etc. This, it would be difficult, without farther repetition to admit; for the admission would strike at the *very root* of the whole principle of, and destroy the confidence on which, Experimental Physiology is based—viz: the *identity of function* in the various parts and organs in man and the lower animals.

Then, Thirdly, another explanation suggests itself to us, in this dilemma, which is, that Dr. Carnochan might possibly have been mistaken in his belief, that he had removed the ganglion of Meckel, in the operations reported by him. This last, with all the lights before us, would appear to us the most probable supposition, were it not that Dr. C.'s high reputation and acknowledged familiarity with the parts in question, which, while they have given the subject importance in our eyes, have also rendered us averse to adopting this remaining supposition as our final conclusion. We therefore respectfully leave the conciliation of the adverse facts to himself.

Our object, in the foregoing remarks, has been neither to deny nor captiously question any conclusions which Dr. C. may have hastily put forth in his report of his remarkable operation. These conclusions of his are but incidental to the main object of his report, and perhaps no considerable amount of importance was attached to them by him, and no great thought or trouble expended upon them. They do not, in any way, lessen the merit of his operations for the relief of neuralgia. We were struck with the remarkable difference in the results reported by him, and in those results which have been so long accumulating in the records of Experimental Physiology. Perhaps ere this the discrepancy may have occurred to himself, but his manner of referring to this question, so important in a physiological point of view, was such as to leave it open for discussion. We have endeavored to do this fairly, and we know in a kindly spirit, carefully avoiding every expression which might bear any semblance to disrespectful or uncourteous criticism.—*Southern Med. and Surg. Journal.*

THE REFLECTING OTOSCOPE AND ARTIFICIAL DRUM—*By J. Henry Clark, M. D., of Newark, New Jersey.*—One day last winter I observed in front of my residence a man taking from his pocket a small mirror, and, turning it obliquely, throw the rays of light down a hole in the edge of the sidewalk that conducted to the faucet, by means of which the supply of water to my house was controlled. Stepping out, I observed that this small hole, about three feet deep and but a few inches square, was perfectly illuminated. It at once occurred to my mind that an instrument might be constructed in the same manner to shed light in upon the drum of the ear, affording the same kind of aid that the opthalmoscope does in the examination of the eye.

I have for five years had in my office, and sometimes used on a dark day, one of the half-dozen *illuminators* which I regarded so objectionable, in consequence of the lurid glare thrown upon the drum by the artificial light employed, that I had ceased to use it. I preferred the simple tubes of Wilde and Toynbee, made for me after the drawings furnished in their publications. Speculums of various patterns and shapes, from Cramer's forceps and Wright's polished metallic apparatus, which date before 1815, down to the simple tubes of Toynbee, have been invented to straighten the passage and shed sunlight in upon the drum. The "illuminators," so called, are of later origin. That of "Hutchinson" is perhaps the least excellent, but has been the most used in this country, because less expensive, being worth in London about one and a half pounds. After rejecting several other plans, the same mode of setting the mirror in the angle of a bent tube was adopted, also of fixing the lens in the eye-piece. In other respects the new instrument, which we call the *Reflecting Otoscope*, differs entirely, and serves a very far more useful purpose than merely the reflecting end of that instrument, if it was regarded expedient to buy it for the purpose of using so small a part of it. Mr. George Tiemann, the celebrated instrument maker of New York, took an interest in the matter, and taking the cue from very rude drawings and suggestions, has succeeded in perfecting an instrument that every one will desire who wishes to become familiar with the pathology of the ear. A large number are ordered in advance, and the first one that was finished was purchased by one of the Professors of a German University, who regarded it as better than anything produced in Europe that he had seen. It is afforded by Mr. Tiemann at a price less than the worthless imported "illuminators." The instrument is got up in a neat manner, and the case contains specula of various sizes, which may be affixed to the reflecting part as the operator may desire. The most important novelty in the new instrument is in the speculum, which is movable, and has a *concave* interior surface, which renders it very much more effective than the more usual form, and better applicable to a moderate light. Although several improvements are already contemplated, it is only necessary to see it in order to perceive that it fills an important place in aural surgery not before occupied.

This is the first instance in which an instrument of this kind has been made in this country. The difficulty of procuring lenses accurate, and excellent and perfect mirrors, has delayed its production for three or four months. The profession will feel obliged to Mr. T. for the ingenuity and perseverance which he has manifested in bringing the instrument to a state of completeness. This instrument may be used with advantage by affixing the larger sized speculum, for the examination of the eye, the mouth, or the rectum. The same principle may be usefully applied to a speculum vaginæ by simply enlarging the whole apparatus. I understand that Mr. T. has such a design, now that his workmen have become accustomed to make the lenses and mirrors, which have proved the really difficult part of the undertaking. It is applicable also to strong artificial light. Its advantages, to sum them up, are:

1. In this the first attempt to employ solar light in this manner to aural examinations.

2. In this the first endeavor to reproduce in this country, in an improved style, the lenses and mirrors which have formed the material part of several valuable foreign instruments.

3. In the combination of the lens, mirror, and improved specula, in a neat, convenient and not expensive instrument.

4. In the movable speculum, which enables the operator to use the largest size that will be admitted in the meatus externus.

5. In the peculiar and entirely novel form of the speculum which is attached to the instrument.

I have found that this instrument has so wonderfully assisted me in making a diagnosis of diseases of the tympanum, that I wish to introduce it fully to the profession.

The Artificial Tympanum, as made by our instrument manufacturer from the first drawings of Mr. Toynbee, of London, the inventor, is liable to several serious objections. I have introduced a great many which fail of adaptation because of their *shape* and the *length* of the *stem*, and which are destroyed by the secretions of the ear. The shape of Mr. Toynbee's drum was determined by the shape of the healthy drum when dissected out. My experience would indicate that a drum of elliptical shape is better; also that the rubber should not contain the material that causes it so readily to decompose. I believe that both these ends may be accomplished. The shape of the artificial drum should be determined, not by shape of the natural drum, but by the form of the rupture. It should, if possible, do little more than *cover the rupture*, leaving the rest of the drum to act as in the days of its integrity. In my New York office I have seen several patients lately entirely relieved by regarding this principle in the application of the artificial tympanum, who were before not benefited by this appliance. Unless carefully constructed, the rubber, after a few weeks, draws over the disk or button on its under surface. An intelligent clergyman thus writes: "I would not be without an artificial drum for any price, but I find they are soon worn out; the elasticity of the material causes it to stretch, so that it slips over the disk. I have placed it in a new place, which gives a new shape to the drum, placing the point of suspension in the foci of an ellipse. This materially aids my hearing." He goes on to remark that this fragment is of more service to him than the unbroken drum, and concludes, "that the configuration of the rupture in his drum is elliptical." I have found that he was not singular in this respect.

It would be very desirable if some other fabric could be found to take the place of rubber in the manufacture of the artificial drum. I believe that rubber is open to several serious objections, while its elasticity only fits it for the purpose; that article that can be so applied as to clear the orifice in the tympanum, and interfere in the least possible degree with its normal action, and produce the least degree of irritation, is the best. It must be thin and should be elastic.

The idea of an artificial tympanum was first suggested to Yearsley, of London, by an American patient, who told him that he could produce in his left ear a degree of hearing, quite sufficient for ordinary purposes, by means of a *moistened piece of paper*. Yearsley found the tympanum wholly destroyed, and regarded the case hopeless. He saw the marvellous relief afforded, but was unable to repeat the experiment with success. In 1842 it occurred to him to try cotton; this he used with entire success for six years, when he published his method of practice in the London Lancet. As late as 1853, and I believe later, he desired nothing better than this cotton. Gutta percha, coils of silver wire, and various other substitutes were used with no success. Yearsley found that the cotton *rather hindered* than benefited his patient until it was *placed directly over the orifice of the drum*. This patient, a lady, introduced it herself every day for several years, and probably does so still. Without it she is perfectly deaf; with it she hears ordinary conversation. Yearsley quotes many such cases. He tells of exactly the same result with his bit of cotton that I see produced almost daily with the rubber artificial tympanum of Toynbee. We need not despair, therefore, of finding some better material than rubber; less heating, less liable to irritate and decompose.

Before laying down my pen I would add a word about the use of rubber. I have long believed that its use in coats and shoes was productive of evil, especially without the exercise of judgment. The same is true of "patent leather." An example has been afforded me within a week that furnishes a valuable hint: A child who had been uniformly perfectly well from birth was seized with obstinate diarrhœa. I could not discover any cause, although several times I went into a very careful investigation; observing at length that the nates were terribly excoriated, and on being told that this had never happened before, and that it had grown worse for ten days, a more careful inquiry revealed the fact that *just at that time* the mother had commenced the use of an India-rubber diaper. I ordered it to be at once omitted. The diarrhœa was better the next day, and was well the day following, while the excoriation was *wholly* cured in about forty hours. It has since been tried twice with the same result. If this illustration is sufficient, I may be permitted to regard rubber, with the materials employed in its composition and admixture, as on the whole, not a desirable article to introduce into any of the cavities of the body, if a substitute can be obtained.—*Med. and Surg. Reporter.*

NOTES UPON AMYLENE. *By Dr. Adolf Schauenstein.* (Wochenblatt der Gesellschaft der Aerzt zu Wien, June 22d, 1857.)

Immediately after the first experiments which were made in Vienna with the new anæsthetic material, amylene, Dr. Schauenstein obtained for examination some of the preparations employed, both of Parisian and Viennese manufacture. They all appeared to be mixtures of solutions having different boiling points, and the greater part of the solutions was volatile at 100°. The most volatile part of the distilled liquid presented a smell resembling chloroform, so that it seemed necessary to examine this pretended amylene for some proportion of chlorine. This experiment is performed in the following manner: The solution to be examined is mixed with about an equal volume of potash or soda ley, free from chlorine, and a few drops of solution of nitrate of silver, and then heated with continual stirring up to the boiling point, in which operation the greatest care is taken that too sudden an evaporation does not occur to the solution to be examined. If now so much nitric acid, free from chlorine, is added, that the separated oxide of silver again comes into solution, then there remains (in case the fluid examined contains chlorine) the chloride of silver, in its well-known white flocculi, as an insoluble residue. A great number of organic combinations, containing chlorine, in which this element cannot be discovered directly by solution of silver, may in this manner be quickly and conveniently tested. The specimens of amylene examined in this manner, by Dr. Schauenstein, all showed the presence of chlorine. The presumption, therefore, formerly existed that the amylene found in commerce was purposely mixed with chloroform, and it was possible, by referring to this addition, to explain the narcotic operation now and then observed, until there was exhibited an equal, or indeed a greater proportion of chlorine in a boiling product of distillation, prepared according to Balard's direction, than had been formerly found in the commercial article. The necessity now presented itself of obtaining positive conclusions upon the proportion of chlorine existing in amylene. Some amylene was freshly prepared, with especial care that all impurity from chlorine should be excluded. The chloride of zinc, employed in the process, was treated for several hours at 160°, and after cooling, was brought to the test of ammonia, in order to discover by this means the accidental presence of free hydrocholoric acid; but it was quite free from this acid. The amylene collected from every part of the product of distillation, which is volatile below the boiling point of water, was left to stand for twenty-four hours, frequently stirring with some fragments of caustic alkali, broken into a coarse powder, and then rectified. Immediately this purified amylene exhibited a large proportion of chlorine.

The investigation of the commercial article had shown that no pure amylene, but only a volatile mixture, containing chlorine compounds, had hitherto fallen into the hands of medical men; and the circumstance that even in amylene, prepared with so much care, such a large proportion of chlorine is discovered, leads to the conclusion that in the method recommended by Balard, by distillation of amyl-alcohol with chloride of zinc, no pure amylene can be obtained. What remains upon the medicinal application of a preparation which shows itself to be a mixture of bodies difficult to separate from one another, and the purity of which cannot be tested, requires no further discussion; and as long as no method is discovered for the preparation of actually pure amylene, and as long as no actual amylene is employed for experiments, all further trials with this body must be wanting in the most necessary foundation of a scientific experiment—namely, sound logic.—*British and Foreign Medico-Chirurgical Review.*

Facial Neuralgia Cured by Division of the Little Nerve Passing from the Superior Alveolar Nerve to the Mucous Membrane of the Mouth. *By Professor Schuh.* (Wochenblatt der Zeitschr. der Gesellsch. d. Aerzte. Nov. 20, 1857.)

Marcus Weiss, aged fifty-five, miller, had always enjoyed good health till attacked by ague, seven years ago; this continued for six weeks, and soon after a continuous pain affected the right side of the face, between the angle of the mouth and the masseter, which, from time to time, became more severe. These paroxysms were accompanied by slight twitching of the muscles on the corresponding side of the face; they would recur frequently on the same day, but often intermitted for several days. Change of temperature, chewing, and talking, exercised no influence on the production of the attacks. After the pain had lasted two years, the patient had two teeth of the right upper jaw extracted on account of severe toothache; the remaining teeth fell out. He used leeches and blisters to no purpose, and remained in the same condition for five years. During the ensuing two years the paroxysms became more frequent; mastication now induced severe pain; he underwent a variety of treatment in Vienna without marked benefit. For a year before coming under Dr. Schuh's treatment, he suffered even at night, and sleep could not be secured by opium.

When seen on the 26th February, 1857, he was spare, but otherwise healthy-looking, prepared to submit to any kind of treatment that might be suggested. No change of colour or temperature was observed in the face during the seizures; there was no lachrymation, no rigidity of any kind; but there was slight perspiration at the time of the attack in the face. The permanent pain was described as residing at the inner side of the cheek, from where it radiated towards the ear during the paroxysms; less frequently towards the eye, and never towards the nose and upper lip. Neither gentle nor rough friction, pressure, pinching, nor talking, had any influence upon the attacks. The patient bore pressure upon the point of exit of the infraorbital nerve without pain. Dr. Schuh observed him during nearly three weeks before undertaking any operative proceeding, and satisfied himself of the futility of employing medicines. He was of opinion that the pain resided in the twig which passed from the superior maxillary nerve through the buccinator to supply the adjoining mucous membrane of the mouth. As it was manifestly impossible to isolate this nerve, Dr. Schuh divided the soft tissues from the angle of the mouth to the masseter; and having separated the mucous membrane from the areolar process of the superior maxilla, scraped off the muscular attachments of the buccinator from the spheno-maxillary fossa. Dr. Schuh made several parallel incisions in a slanting direction, so as to insure destruction of a portion of the nerve. No material hæmorrhage occurred while the operator's finger occupied the spheno-maxillary fossa as a director for the instrument; but on its removal a considerable amount of blood was lost. From the time of the operation all pain ceased, and the patient, with the exception of an intercurrent attack of epidemic tonsillitis, recovered satisfactorily. He quitted the hospital before the wound was entirely healed, as the exfoliation of a small lamina of the upper maxilla was not completed.—*British and Foreign Medico-Chirurgical Review.*

ON THE THERAPEUTICAL ACTION OF CHLORATE OF POTASH, WITH A NEW MODE OF ADMINISTERING IT. *By Dr. Dethan.* (L'Union Médicale, June 4th, 1857.)

Dr. Dethan considers that chlorate of potash is a powerful sialagogue, and that its elective action on the bucco-pharyngeal mucous membrane is well marked. To this physiological action is added a very remarkable and valuable success in pathology; its rapid and incontestable effects in mercurial salivation, by checking the formidable mercurial affection, have permitted practitioners to continue the mercury without fear, and thus to contend without remission against the constitutional infection. As an especial and incontestable remedy in ulcero-membranous stomatitis, this medicine need not, according to the physicians of the Hôpital Sainte-Eugénie, be swallowed; its topical application is sufficient, and in a short time the mucous membrane recovers its normal qualities and functions. Dr. Dethan concludes that the chlorate of potash, administered under a special form, which would permit the local action to be exercised slowly and certainly, although leaving the medicine to be carried into the stomach in a state of solution with the mixed liquids of the salivary, buccal, and pharyngeal glands, would be the mode of administration which would combine all indications and all opinions. He therefore suggests the use of the remedy in the form of pastiles, so that the patient may have at hand a remedy against the injurious effects of a mercurial treatment which he may be undergoing. The experiments of Dr. Ricord, and the publications of M. A. Fournier, testify incontestably in favour of thissuccessful simultaneous medication. In certain forms of angina, attended with fibrinous exudations, it prevents the intimate adherence of the false membranes to the mucous membrane, and facilitates their expulsion, and assists the action of emetics. In this affection the topical action of the chlorate, favoured by the bruising between the teeth, the natural solution in the liquids of the mouth, and its penetration into all the points interested, will be certainly efficacious. In debilitating diseases, such as diphtheritis, and gangrene of the mouth, the child will find an agreeable and reparative kind of food, together with the most appropriate remedy hitherto discovered, against these diseases.—*British and Foreign Medico-Chirurgical Review.*

Dr. Breslau's case of injection of liquor-ferri sesquichlorati shows the efficacy of this treatment in certain cases of excessive uterine hæmorrhage. A woman, forty-five years old, had worn a pessary for a long time, which she had given up on account of pain and hæmorrhages. Examined, there were found five grape-shaped polypi around the os uteri, with ulceration. These were removed by Siebold's scissors, and potassa fusa applied to the ulcerations. The hæmorrhages remitted somewhat. The uterus was, however, entirely bent upon itself, and the menses returned in great excess. The cavity of the uterus was somewhat enlarged. The return of the menorrhagia reduced the patient to an extreme degree of anæmia.. Ergot of rye failed to effect any contraction of uterus or arrest of flooding. Two possible conditions presented themselves to Dr. Breslau: 1st, The five polypi attached to the cervix favored the presumption that there existed a similar warty hypertrophy of the lining membrane of the uterus; 2d, There might be a destructive ulcerative process of a malignant character going on in the same structure. In the first case, the curette of Recamier seemed indicated, in order to scrape the uterine membrane clean from the presumed excrescences. In the second case, it seemed necessary to bring a caustic and styptic agent in direct contact with the diseased membrane. To answer this latter indication, he selected the liquor-ferri sesquichlorati of the Bavarian Pharmacopœia. Having first straighthened the retroflected uterus by the sound, and inserted an elastic catheter as far as the fundus, he injected one ounce and a half of the liquor-ferri, diluted with an equal quantity of water. The injected fluid was retained in the uterine cavity for a minute and a half by the pressure of the fingers at the os around the catheter. During the operation, the patient felt a dull labour-like pain, which lasted for two hours. The hæmorrhage, which had hitherto been unintermitting, now suddenly stopped, and was not renewed. After some days, brown, crumbling clots were discharged, but no fluid blood. Seven months afterwards there had been no return of menorrhagia. The patient menstruated regularly every twenty-one to twenty-four days; and through internal administration of steel and quinine, recovered robust health.—*British and Foreign Medico-Chirurgical Review.*

STATISTICS OF COILING OF THE FUNIS. *By Dr. Weidemann.* (Monatsch. für Geburtskunde, Bd. viii.; and Medical Times and Gazette, July, 1857.)

Dr. Weidemann states that among 28,430 deliveries the funis has been found coiled around the child in 3,379 instances. In 3,230 of these it was coiled around the neck, and in 149 around other parts of the body. Of the 3,230 cases, 2,546 consisted in a simple coil, and in 684 there were severe coils. In relation to the causes of this occurrence, it is interesting to notice that of 1,788 cases occurring at the Marburg Midwifery Institution, the funis was in 80 (1:22.2) under 15 inches, and in 183 above 25 inches (1:9.71) in length; that in 54 (1:33) there was very little liquor amnii, and in 41 (1:43.6) there was very much; in 165 (1:10.8) the child was under 5 pounds weight, and in 28 (1:61.7) it was 8 pounds. Therefore, among the favoring causes of the occurrences may be mentioned a long funis, abundance of liquor amnii, and a small child.

Among 2,930 infants born at Marburg, 182 (1:16.09) were dead, and 251 (1:12.41) were still-born. Of 725 born with coiled funis, 45 (1:16.11) were dead, and 72 (1:10.06) were still-born. Of the 45 dead-born, in 18 only could the death be referred to this alone, i. e., only 1:40.2 in the 725 examples of coiling. From an examination of the figures derived from the midwifery institutions at Dresden, Göttingen, Würsburg, Berlin, and Marburg, it results that of 13,720 new-born infants, 902 (1:15.21) were born dead; while in the 1,217 instances of coiling of the funis, 31 children were born dead, whose death could be attributed to that circumstance, giving a proportion of 1:39 to the coilings, and 1:19 to the number born dead.

Thus, as (1) the 16th child among new-born children in general, as well as among those in which coiling has taken place, is born dead; as (2) the 12th child among the new-born in general, and the 10th among those around whom the funis is coiled is born still-born; and as (3) in 1 child in 40 only can this coiling be regarded as really the cause of death, it is evident that this accident does not occupy a very prominent place.—*Half-Yearly Abstract of the Medical Sciences.*

YELLOW FEVER AT LISBON.—Since the 9th of September last, yellow fever has raged in Lisbon, and during one hundred and five days 13,482 cases occurred, with 4,759 deaths. The last cases were reported on the 22d of December, and the plague was apparently stayed by the setting in of cold weather. Twenty years ago it was generally believed that yellow fever, in its virulent epidemic form, was confined to intertropical regions; but of late years it has crept along the coast of America to places where it was formerly altogether unknown, and has now attacked, in a malignant form, a city of Europe, situated in latitude 38° 42′. The French authorities have therefore taken the alarm, and wisely adopted precautions to avert, if possible, the outbreak of the disease in France. The lazarettoes, which have been long disused, are preparing for the reception of invalids; and Dr. Melier, Inspector-General of the French sanitary services, is making a tour of the ports, and adopting rules in each of them for the due establishment of quarantine. One large transport ship, which arrived from Senegal with 186 invalids on board, has been prevented from landing her passengers at Brest, because she had unfortunately entered the Tagus on her passage, although no yellow fever appeared on board. In such cases, the International Sanitary Convention decrees that ten days must elapse without the occurrence of yellow fever between the touching at an infected port and the removal of quarantine.—*Lancet, January* 9, 1858.

ON THE FUNCTIONS OF THE THYROID BODY—*By Dr. P. Martyn.*—Dr. Peter Martyn has communicated to the Royal Society some very ingenious speculations as to the use of this remarkable body. He first calls attention to the necessity of rigidity in all round instruments. This is accomplished, he thinks, by the thyroid gland, which, being pressed upon by the muscles, during the act of speaking, becomes gorged with blood, and presses upon the larynx, rendering it tense. Furthermore, he believes this so-called gland acts as a loader. In musical instruments, loaders are used to render the vibrations slower and longer, and the tone in consequence fuller, louder and deeper. They thus give to the voicing part of a small instrument the power and quality of a large one. The human organ of voice is only three inches long, and yet "has the same power as, and better quality of tone than, the instrument which more nearly approaches it—the French horn—which is nine feet, or the 'vox humana' pipe of a moderate-sized organ, which is from four to eight feet long. This economy of size in the human organ has always been wondered at, but never, that I know, explained." Finally, the author of the paper believes that by the varying shape, bulk, density and pressure of this body, it aids materially in producing the qualities of modulation and expression belonging to the human voice.—*Am. Jour. of Med. Science and of Dental Science.*

Dr. Savage has more recently ("Lancet," December last) detailed two cases of obstinate menorrhagia, in one of which he injected first a strong solution of tannin and alum, which was successful for a time. The hæmorrhage returning, the uterine cavity was scraped out by Récamier's curette. The patient said the operation gave her no pain. Some days later, three drachms of the London tincture of iodine were injected, and repeated twice at intervals of three days. The floodings quite stopped, and the patient recovered strength. In the other case, two ounces of tincture of iodine, with an equal quantity of water, were injected, with an equally successful result. Dr. Savage lays stress upon the importance of dilating the cervix uteri freely before resorting to iodine injections.—*British and Foreign Medico-Chirurgical Review.*

Dr. Guillier relates an example of presumed forcing of air into the peritoneum through the uterus and Fallopian tubes. He hadapplieda pessary to a woman, aged forty, on account of prolapsus, and for cleanliness had ordered injections of warm water. After the first injections acute pains arose in the sides, which continually increased, and were attended with great anxiety. After some hours these disappeared with eructations and vomitings. This was repeated after every injection. This phenomenon seemed explained by the very low position of the uterus, and by its open mouth being put in the centre of the pessary, so that at every injection water and air would necessarily be thrown into the uterus. [The fact of air having been forced through the tubes is not satisfactorily proved in the above case. The accident is, however, possible, and should serve as a caution in throwing injections into the uterine cavity.—*British and Foreign Medico-Chirurgical Review.*

Medical Students in Paris.—The number of medical students taking out inscriptions for 1857–8 amounts to 901 for the Doctorate, and 126 for the grade of *Officier de sante*—total, 1,027. The number of new inscriptions amounts to 158. Last year the total was 1,000, of which 126 were new. The following is a comparison of the last eight years:

1850	1,223	inscriptions,	429	new.
1851	1,300	"	313	"
1852	1,437	"	334	"
1853	1,055	"	158	"
1854	964	"	151	"
1855	966	"	180	"
1856	1,000	"	126	"
1857	1,027	"	158	"

Perineuron, a New Element in Nerves—*By C. Robin, M. D.*—Dr. C. Robin has recently described a new tissue altogether distinct from the neurilemma investing nervous matter. Its characters are:

1. Its situation around the primitive bundles of nerve tubes and sometimes around the tubes themselves.
2. Its tubular form and its ramifications accompanying the ramifications of the nerves.
3. Its chemical reactions with various tests.
4. A thin, transparent wall, finely granulated, often presenting longitudinal striæ and nuclei, more numerous in the small tubes than in the large.

The perineuron is to the nerve tubes what the sarcolemma is to the muscular fibres, the immediate investment of the proper nerve tissue. It is found in all the nerves of animal life, including the vagus, in all their length from the ganglia for the sensitive nerves, and for those which do not pass through ganglia, from the cerebro-spinal axis. The three nerves of special sense are the only exceptions to this, they not having the perineuron. In the sympathetic nerve this envelope exists only where there are white bundles or ramifications, and not among the gray or gelatiniform bundles.

In large nerves, the tubes of the perineuron are large (two to five-tenths of a millimetre); in ramifications they become smaller, containing then only two or three nerve tubes, and sometimes only one. The capillaries do not go inside these tubes.—*Amer. Jour. of Dental Science.*

STATISTICS OF PLACENTA PRÆVIA.—*By Dr. Schwarz.*—(Monatsch. für Geburtskunde, Bd. viii. 1857.)

Dr. Schwarz, of Fulda, in Hesse-Cassel, having heard the frequency of placenta prævia stated in a medical society as far greater than he had hitherto believed it to be, examined in reference to this point the official returns by the Hesse practitioners. These were supplied by one hundred and fifty accoucheurs, during a period of twenty years, *i. e.*, from 1835 to 1854 inclusive. They related to 519,328 births, and among these were only 332 cases of placenta prævia—the numbers varying from eight to twenty-eight per annum. Of these 332 cases, 246 women recovered and 86 died; 251 children were born dead, and 85 were born living. In 40 instances the women were primiparous and in 292 pluriparous. Podalic version was performed in 259 cases, and cephalic version in 7; while in 23 instances the children were removed by the forceps, in 6 by craniotomy, and in 13 by post-mortem Cæsarian section. In 8 instances the placenta was removed, and in 16 the plug was resorted to.

[*Ranking's Abstract of Med. Sciences.*

DANGER OF CARBONIC ACID USED AS AN ANÆSTHETIC.—It is well known that lately Professor Simpson of Edinburgh, and afterwards M. Follin of Paris, have reintroduced the practice of using carbonic acid as an anæsthetic. It is particularly in painful affections of the uterus that this substance has been employed. Whatever be its mode of action, which we believe to be at first a temporary stoppage of circulation, in consequence of a contraction of the bloodvessels and of the agglutination of the blood corpuscles, it truly diminishes pain for a time, but there is danger in employing it for a long while, as M. Charles Bernard has recently shown. The carbonic acid is absorbed, and it produces symptoms of asphyxia, vertigo, headache, nausea, an undefined feeling of pain, and weakness, etc. It is necessary, therefore, not to make use of this gas too long and in too large quantities.—*Brown-Séquard's Report of French Medicine.*—*N. Y. Journal.*

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THREE CASES OF CONGENITAL DEFICIENCY OF THE INTESTINAL CANAL.

By M. Schuppert, M. D., New Orleans.

Case I.—*No indication of an anus, lower part of rectum forming an imperforate cord of half an inch in length.*

Case II.—*Unnatural termination of rectum in the bladder, with total absence of any indication of an anus.*

Case III.—*Total occlusion of anus, and entire want of rectum. Colon transversum terminating in the left kidney.*

[Two of these deformities presented to the pathological museum of the New Orleans School of Medicine.]

Case I.—M. K., aged twenty-one years, of a delicate constitution, had given birth to a female child on the night of the 24th of May, 1853. On the 27th I was called to the infant, probably to insure the friends a legitimate certificate of death. The child was then in a state of exhaustion; abdomen tympanitic, tense, skin transparent, vessels much injected. On examination, I found a total deficiency of the anus, and, questioning them why they had not sent sooner, and stating that there might have been a chance to have saved the child's life by an operation, they stared in my face, unable to comprehend what I meant by an operation. To my astonishment, I then found that none of the attendants, neither the "midwife" nor physician, who had been called in before me, had had any idea of the deficiency of the anus, that they had drugged the child with castor oil and calomel *ad infinitum*, and that they had even given it *injections* (by the vulva!) but "all of no use." I was told that the child had thrown up freely and had cried so much that they thought "it had cramps." I refused to do anything on account of the dissolution of the child soon to be expected, and which occurred six hours afterwards. On the following day I made the post mortem. As already stated, there was a total absence of the anus, and half an inch above the point where the external termination of the "rectum" should have been, the intestinal canal ended in an imperforate cord of half an inch in thickness. No other malformations coexisted.

Case II.—In September last my attendance was requested to see a female child of Mrs. P., born on the day previous, without an anus. The child presented a good appearance and was otherwise of a healthy constitution. The urine, which was passed freely, contained a small quantity of fæcal matter. Not the least indication of an anus could be observed, and two days afterwards no prominence or even obscure fluctuation in the anal region could be detected. Seeing the quantity of meconium in the urine abating, I stated that the opening of the bowel going into the bladder must be very narrow. Actuated by a just consideration of the impropriety of an operation in the case, I stated to the father that I did not expect the child would be favored in any way by an operation, but that it was the only chance it had to survive the present state of affairs. On being asked what I would do in case the child were my own, I answered that I would not operate. The father therefore resolved to let nature demolish a work so ill conceived by her. The child had refused its mother's breast from the time of its birth, and I told the nurse not to give her anything except a small quantity of chamomile tea at times. After lingering five days, without having shown any signs of suffering, it expired. A post mortem examination was made four hours after death. Abdomen tense, vessels of the skin much injected, small intestines distended with flatus, no exudation of lymph, the contents of the bowels had changed into a thin, greenish-yellow mass of an odor by no means agreeable. The "rectum" was adherent to the back wall of the bladder, and communicated with the neck by a narrow canal, of the diameter of a twentieth part of an inch. There was also just below this opening another and smaller one terminating in a blind sac and not communicating with the rectum.

Case III.—On the 30th of May, 1856, I saw a male child of Mr. S., at Algiers. I was told that the child had been born two days previous, and that after the deficiency of the anus had been discovered a physician was called, who tried to make an artificial passage with a trocar, but, failing to accomplish this, had abandoned the child. An examination of the infant revealed a perfect want of the natural opening of the alimentary canal. Its place seemed to be indicated by a spot of a somewhat brownish color, presenting a wound which no doubt had been caused by the trocar, and into which a probe could be introduced to the depth of one inch. The abdomen was distended, and the child was in so debilitated a condition that it was only by the urgent request of the parents, that I should try every means to save its life, that I finally consented to operate. A cut was first made in the mesial line, about an inch in length and one and a half inches deep, but, no fæcal matter exuding through the opening, I thought it not advisable to cut deeper, and resolved at once to try the method of Callisen, so much recommended by Amussat, viz: establishing an artificial anus in the descending colon; but here also no natural parts of the colon could be secured. Any further surgical interference was thought out of the question. The child died two days after. A post mortem examination, which, with some difficulty, we were allowed to perform, revealed a complete malformation of the descending colon. The left kidney was displaced forwards and its anterior part blended with the transverse colon, which terminated in it. From the kidney downwards, as far as the floor of the pelvis, was traceable an imperforate cord adhering to the pelvic fascia—without doubt the rudimentary elements of the descending colon, covered partially by the peritoneum and mesenterium. Two inches before its termination it formed a cyst containing a yellowish fluid.

In having thus contributed a small part to the statistical annals of a special kind of natural curiosities, (rather a bad kind of lusus naturæ,) we may be allowed a few remarks respecting surgical interference in cases of malformation of that part of the intestinal canal, which has been called (lucus a non lucendo) the "rectum." In reviewing the published cases to which we have had access, we find the "rectum" either entirely or only partially absent, being prolonged either as a narrow tube or an imperforate cord, blended mostly with adjacent parts; or we find it occluded by one or more membranes at different points above the external coverings of the pelvis, or terminating in the vagina or bladder, or it has been found to open in different regions of the body, as on the neck, head or hypogastrium, etc , etc. In the observation of the latter deformity, Littré first conceived the idea of establishing, in cases of emergency, an artificial anus in that region—a method which still carries the name of that celebrated anatomist, although it was Callisen who *executed* the idea on the living, but chose the region above the crista ilii on the external edge of the sacro lumbalis muscle, where the descending colon can be easily reached, as the field of operation. Of these deformities, unfortunately, those seem to be most prevalent in which surgical interference will be of none or little avail; we mean those malformations where the natural orifice is entirely occluded; those cases of "imperforate anus" with a partial absence of the "rectum." In the most favorable of those cases in which, by a surgical operation, an opening for the outlet of fæcal matter has been established, great difficulty arose in keeping that opening pervious. Most surgeons who have operated "successfully" have finally seen their patients perish. The children operated on seldom acquired a perfect control over their bowels, and died by closure of the artificial opening soon after the canules had been withdrawn, if no other so-called "unfortunate accident" occurred to which to assign their death.

The only cases where success from surgical aid may reasonably be expected are those in which the anus or lower end of the bowel is obstructed by a mere membrane.

Some authors, in recommending the operation for artificial anus on the descending colon, in so tender an age, give preference to the method recommended by Littré, as more easy and safer to be performed, having at the same time the advantage of an opening at the lower part of the colon; but we think it an ill-begotten idea of humanity to preserve a life destitute of those natural attributes which can render it happy. A surgeon who is not influenced by the "eclat of his knife" will listen to the experimental facts medical history teaches him. Still there may be circumstances under which he is obliged to forego his better judgment.

VESICO-VAGINAL FISTULA—OPERATION FOR—CURE.

By M. Schuppert, M. D., New Orleans.

A. K., aged thirty-four years, mother of one child, which had been delivered by the breech two years ago, by a physician of this city. On the first of November, 1857, at four o'clock A. M., she was again taken in labor, pains recurring half-hourly, and at five o'clock the unavoidable midwife made her appearance. On the following night the woman in labor wanted the attendance of a physician, but this was objected to by the "midwife," who assured her that she was fully competent to do all that was necessary, and that "after two to four pains all would be over." The night passed off, and it was not until ten o'clock the next morning that the liquor amnii was discharged, which very probably would have been delayed had not the midwife come to the original conclusion to order her to take a sitz-bath of cold water, the consequence of which was a heavy chill, producing rupture of the membranes. (This remedy may be taken under consideration by those who regard the amniotic sac as a noli me tangere); and at the time of the chill the movements of the ovum were felt by the patient. Between four and five o'clock in the afternoon the doctor was sent for, but contrary to the wishes of the midwife, who stood her ground with all the impertinent obstinacy characterizing that legion of female obstetric pretenders. The doctor came between six and seven o'clock, and, after "hard working" of about three-quarters of an hour the patient was delivered of a still-born child, (breech presentation,) but, unluckily, not without leaving behind a vesico-vaginal fistula, which was found on the next day by a female friend attending the delivered woman. In the beginning of the present year an operation for the fistula was determined on, and was performed on the 14th of January by the doctor who had attended her in her confinement. Unfortunately the operation was wholly unsuccessful. It was on the 21st of February that I was sent for, and on the 26th I operated, assisted by Drs. Eylandt and Maas of this city. The fistula was situated near the neck of the bladder, nearly two inches from the external mouth of the uretha, and was three-eighths of an inch in diameter. Having evacuated her bowels the day previous, she took two grains of opium on the morning of the day of the operation, to constipate her bowels. The mucous membrane, with parts of the cellular tissue of the vagina, were taken off with the knife to the extent of about half an inch around the edges of the fistula, which was cut out with the part of the bladder in contact with it. After the bleeding, which was not very considerable, had ceased, the wound was sewed up by the application of five sutures of silver thread, and the sutures closed by shots over a silver plate (combination of Sims' and Bozeman's operations. In applying the sutures the surgeon has been urged to be very careful not to wound the bladder. We took especial care not to *avoid* it, but to have all five stitches absolutely penetrating its cavity, and the organ proved to be utterly insensible to the needle. After the sutures had been applied the patient was removed to bed, and an ordinary female catheter placed in the bladder and fixed to the thigh. Five days afterwards an enema of water was given and the bowels relaxed. The vagina was injected twice daily with cold water. On the 7th of March, nine days after the operation, the sutures were removed. The fistula was perfectly closed and a fine set of granulations prognosticated a durable result. The catheter was removed four days later, with advice to employ it several times daily, so as to prevent too early distension of the bladder with urine. Patient was allowed to walk about the room. On the 20th of March (the day we write these lines) she attends again to her duties, and can be looked upon as perfectly cured from one of the most loathsome diseases women are liable to.

It seems to us that the good result in operations for vesico-vaginal fistula depends mostly on the extensive use of the knife and in taking off all the mucous membrane of those parts which have to become united by sutures. By including a part of the urinary bladder in the sutures the threads can be drawn tight, without having to apprehend that they will cut through the tissues, and at the same time we will secure the wounded parts against the noxious influence of the urine.

The above related case shows at once the detrimental nature of an uncontrolled practice of a class of female pretenders in obstetrics—a state of affairs assuming in this city so fearful a character, that we, at least, will not be found guilty of having missed an occasion to utter our *cæterum censeo hanc infamissimum ordinis utilitatem esse delendam!*

ACCLIMATION; AND THE LIABILITY OF NEGROES TO THE ENDEMIC FEVERS OF THE SOUTH.

In the last (*March*) number of that influential and widely-circulated journal, the *North American Medico-Chirurgical Review*, we find an extract from the work of Nott and Gliddon, on the *Indigenous Races of Mankind*, expressing observations and opinions on the above subjects so entirely different from our own, and, as we believe, so wide of the truth, that we have concluded to invite the special attention of Southern physicians to them, with the view of ascertaining, if possible, to what extent such opinions prevail. We do this in no captious spirit towards our talented and esteemed friend, Dr. Nott; but solely for the purpose of eliciting *truth* on a subject of vital importance. Constituted as men are, they cannot be brought to see things in the same light, nor to reason alike from given premises; yet there must be *truth* on one side or the other in all such discussions, and when it cannot be satisfactorily demonstrated, we have to receive as truth the concurrent observations and conclusions of the largest number of equally competent observers and thinkers. The following is the extract alluded to:

"The fact is so glaring, and so universally admitted, that I am really at a loss to select evidence to show that there is no *acclimation* against the endemic fevers of our rural districts. Is it not the constant theme of the population of the South, how they can preserve health? and do not all prudent persons, who can afford to do so, remove in the summer to some salubrious locality, in the pine-lands or the mountains? Those of the tenth generation are just as solicitous on the subject as those of the first. Books written at the North talk much about acclimation at the South; but we here never hear it alluded to *out of the yellow-fever cities.* On the contrary, we know that those who live from generation to generation in malarial districts become thoroughly poisoned, and exhibit the thousand Protean forms of disease which spring from this insidious poison.

"I have been the examining physician to several life-insurance companies for many years, and one of the questions now asked in many of the policies is, '*Is the party acclimated?*' If the subject lives in one of our Southern seaports, where yellow fever prevails, and has been born and reared there, or has had an attack of yellow fever, I answer, '*Yes.*' If, on the other hand, he lives in the country, I answer, *No;*' because there *is no acclimation against intermittent and bilious fevers, and other marsh diseases.* Now, I ask if there is an experienced and observing physician at the South who will answer differently?"

Dr. Nott here proclaims that "*there is no acclimation against the endemic fevers of our rural districts,*" and boldly asks "if there is an experienced and observing physician at the South who will answer differently?"

With all due deference we feel bound to reply that there is at least *one*, and we believe *thousands*, not only of physicians, but planters, too, who differ with him on this question. We were born and raised in the South, and have practiced medicine in the country, in villages, and in the city of New Orleans. We have been a planter also, and have had no ordinary opportunities to observe the effects of acclimation, not only on the white and black races of men, but also on the lower animals, and we must express the opinion, that, *but for the attainment of a greater or less degree of acclimation*, the extensive malarious region of the Southern and Southwestern States never could have reached its present state of population and improvement. So far from the fact being "glaring and universally admitted that there is no acclimation against the endemic fevers of our rural districts," as is maintained by Dr. Nott, we contend that exactly the converse opinion obtains; hence the additional value attached by all practical men in these parts, both country and city, to the *acclimated* negro, horse, ox and milch cow. Perhaps it would not be going too far to say that even the dogs, turkeys and chickens have to undergo acclimation before they can do well here.

We thought the fact was universally admitted that all immigrants from more Northern regions to the South were very liable to suffer from the endemic fevers, for the first two or three years of their residence, but that after that period they obtained comparative immunity, and in the course of time many enjoyed as good health here as they did where they came from, and some of them *much better*. They become habituated to the climate and its diseases, and may live as long as the *creoles*, or natives, who certainly make no contemptible display of longevity in the mortuary statistics of the country.

There appears to be a slight inconsistency in some of the remarks of Dr. Nott respecting the *acclimation of negroes* and their liability to the endemic fevers of the South. In one place he says negroes are "comparatively exempt from all the endemic diseases of the South." In another—"The acclimation of negroes, even according to my (his) observation, has been put in too strong a light." * * * * "They never become proof against intermittents and their sequelær." * * * * "Whenever the whites are attacked with intermittents, the blacks are also susceptible, though not in so great a degree." And again: "Certainly negroes do suffer greatly on many cotton plantations in the middle belt of the Southern States; and I have seen no evidence to prove that they can, in this region, become accustomed to the marsh poison." Dr. Nott applies these observations to the region of country removed from the rice country, and adds: "We shall see further on that the negroes of the rice-field region do undergo a higher degree of acclimation than those of the hilly lands of the interior." Upon this he throws out the suggestion that there may be "difference in the types of those malarial fevers which originate in the flat tide-water rice-lands, and those of the clay hills, or marsh fevers of the interior."

Dr. Nott says in a note: "A medical friend, (Dr. Gordon,) who has had much experience in the diseases of the interior of Alabama, South Carolina and Louisiana, has been so kind as to look over these sheets for me, and assures me that I have used language much too strong with regard to the exemption of negroes. He says they are quite as liable as the whites, according to his observation, to intermittents and dysentery." On a closer examination of the different remarks of Dr. Nott, above quoted, we confess ourselves somewhat at a loss to decide which side of the question he has raised they tend chiefly to support. First comes the general remark that "there is *no acclimation* against the endemic fevers of our rural districts;" then, "negroes are comparatively exempt from all the endemic diseases of the South;" again, "negroes do suffer greatly on many cotton plantations in the middle belt of the Southern States, and he has seen no evidence to prove that they can, in this region, become accustomed to the marsh poison;" and finally, "that the negroes of the rice-field region do undergo a higher degree of acclimation than those of the hilly lands of the interior."

A reviewer of Dr. Nott, in the *Southern Medical and Surgical Journal* for January, 1858, says, at page 22: "We must differ from the learned author when he affirms that negroes are comparatively exempt from all the endemic diseases of the South. Such is certainly not the case in this section of Georgia, (Augusta) and in the adjacent portions of South Carolina and Alabama, where every planter knows that his negroes suffer, equally with the whites, annual attacks of intermittent and remittent fevers, dysentery, malarial pneumonia, etc. This writer then goes on to "out-Herod Herod" in his oppositon to malarial acclimation, reaching the *ne plus ultra* when he says: "We may safely affirm the liability to *our* fevers are (i.) in a direct ratio with the length of time the individual has resided in the malarial district, and that *natives* are the most susceptible." * * * * "Negroes born and reared upon the plantations of Georgia and South Carolina are fully as liable to fever as new comers, and we think much more so." But finally, he concurs with Dr. Nott, that in the *most malarious* portion of the whole malarious region, the low country, "negroes do become acclimated and comparatively exempt from fevers." From this he infers also with Dr. Nott, "that the low-country fever, like yellow fever, must then be essentially different from any form of fever in the upper sections of the Southern States."

Now, to our apprehension, the attainment of more perfect acclimation in the latter region is more justly attributable to the *steady continuance* than to any *peculiarity* of the febrific cause; for we find no material difference in the types of fever prevailing here and the uplands.

There are malarious *seasons* as well as malarious *regions*, and it appears to us that in those climates and localities where the operation of the morbific cause is most *continuous*, the effects should be most *durable*. Thus, by the inherent conservative powers of the animal constitution, it is capable, in the course of time, of habituating itself to the deleterious influence of deadly poisons, as is actually witnessed in the use of opium, strychnine, arsenic and corrosove sublimate. The following quotations from respectable authorities will serve to show how widely different from the commonly received opinion on the subject of acclimation are the positions held by both Dr. Nott and his reviewer.

Dr. E. M. Pendleton, of Sparta, Georgia, in an interesting paper "*on the susceptibility of the Caucasian and African races to the different classes of disease,*" published in the *Southern Medical and Surgical Journal* for November, 1849, after showing the greater liability of children than adults to idiopathic fevers, remarks: "But why the adult should be less liable than young persons is not so easily determined, unless young people in miasmatic districts have to undergo a kind of acclimation, as foreigners, and afterwards become less subject. I have observed that parents seldom have fever where they have lived a long time in unhealthy sections, while their children are frequently every one prostrated at once. Inquire of them, however, and you will find that in former years they were equally as subject to it as their children seem to be in later days."

From Dr. La Roche's valuable work "*on Pneumonia and Malaria,*" we take the following: Speaking of the *protection* afforded by acclimatization, he says: "That such a protection is thus obtained, to a greater or less extent, in regard to all malarial and some other forms of fever, no one who has examined the subject with attention will feel disposed to deny. By long habituation to infectious localities, and to the high temperature of hot regions, the system becomes acclimatized, and thereby acquires the power of tolerating perfectly and permanently the poison, or of eliminating it as soon as received, without succeeding reaction. The observation is of old standing. Pliny, nearly twenty centuries ago, called attention to the fact, *'that they who are seasoned can live amid pestilential diseases,'* and the statement has been confirmed by all subsequent observations." P. 403.

Dr. La Roche goes on to show correctly that "the dangers of infection to strangers is in proportion to the coldness of their native land—that the protective influence of acclimatization is lost by a prolonged residence in cold climates—that the children of the natives and acclimatized do not enjoy the same advantages in regard to protection as their parents, but acquire them rapidly as they advance in age, etc.;" the most of which he also shows to be equally true of yellow fever. P. 404–5.

Dr. Nott says: "An attack of yellow fever does not protect against marsh fevers, nor *vice versa.*" This is very true, but we go farther still. We maintain that a *mere* attack of yellow fever does not protect completely against the same disease. To effect this purpose there must be *a good strong attack*—one that is capable of modifying the system and leaving a permanent impression. So, likewise, with malarious or country fevers. It requires longer exposure and often numerous attacks before the human system becomes seasoned to the deleterious influence of malaria. Yet La Roche demonstrates the fact by voluminous testimony, and it fully accords with our observation, that persons who have lived long in southern malarious districts suffer much less from yellow fever, when first exposed to it, than those from the North who have not been thus seasoned. What does this prove, if not that there exists a consanguinity between these types of fever?

The following extract, from *Watson's Lectures,* presents this subject in its proper light:

"Another fact worthy of notice, in respect to the agency of the malaria upon the human frame, is that it affects strangers much more readily and decidedly than the natives of the place. In other words, habit mitigates the injurious effects of the poison. Persons become seasoned to it. At Walcheren, though almost every adult among the lower classes had labored, in the course of his life, under the endemic intermittent, yet they were infinitely less subject to it than strangers: and they will not believe that their beloved birthplace is unhealthy. Sir Gilbert Blane says that persons of education, and even medical men, denied indignantly that their country was less healthy than any other; and attributed the sickness which raged among our troops to some trivial circumstances of diet or habits, and not to any insalubrity of the air. This is a curious moral feature, but a very general one. In the pestilential plains of Estremadura the superstitious natives, unable or unwilling to account for a disease of a type so uncommon, among the soldiers, from any unwholesomeness of the air, declared that they had all been poisoned by eating mushrooms. It was found, also, at Walcheren, that the strangers who survived the first attacks became thereafter much less liable to the endemic fevers. The French general, Monnet, who had held the command at Flushing for seven years, had acquired a knowledge of this fact, and endeavored to turn it to practical account. He recommended that troops should not be frequently changed; for when it was the custom to send battalions from Bergen-op-Zoom every fourth night in succession, to work on the lines of Flushing, these men never failed, upon their return, to be taken ill in great numbers. General Monnet therefore advised, however displeasing it might be to the officers, that a stationary garrison should be retained at Walcheren, in order that the men might be habituated or seasoned to the air, (*acclimatés*) and he adduced the instance of a French regiment which suffered in the second year of its being stationed there only one-half the sickness and mortality which it suffered during the first year, and hardly suffered at all in the third year." P. 446.

Dr. Watson then speaks of the generally injurious effects of malaria, besides the violent and distinct forms of fever it produces in new comers, and gives the following testimony in respect to the exemption of negroes:

"One remarkable exception is mentioned by Dr. Ferguson. From some peculiarity or idiosyncrasy (which he conjectures may be somehow connected with the texture of the skin) the negro appears to be proof against endemic fevers. To him marsh miasmata are in fact no poison, and hence his incalculable value as a soldier, for field service, in the West Indies. The warm, moist, low and leeward situations where these pernicious exhalations are generated and concentrated, prove to *him* congenial. He delights in them, for there he enjoys life and health, as much as his feelings are abhorrent to the currents of wind that sweep the mountain-tops, where alone the whites find security against endemic fevers."

There is doubtless much truth in the remarks of Dr. Ferguson, but his assertions are altogether too strong for our temperate region. Malaria is certainly a *poison* to the negro as well as the white, but it is less deleterious to the former, and he more readily becomes seasoned to it.

It would seem that those types of idiopathic fever which, according to Dr. Campbell's plausible theory, depend chiefly on disorder of the *cerebro-spinal system of nerves*, as all the well-marked *paroxysmal* fevers are more subject to recurrence and require a longer period for acclimation than those of the *continued types*, as yellow typhoid, typhus and the eruptive fevers, measles, scarlatina, variola, varicella, etc. Quotations on this subject might readily be multiplied to any extent, but, perhaps, to little purpose. The testimony would be found to be very conflicting, and difference in opinion would still remain.

To show that we have not penned these remarks in a spirit of captious criticism, but only for the purpose of eliciting the expression of opinion by others on the subject, we will now give our own views in regard to *acclimation*, published in our first volume of *Southern Medical Reports*, (1849.) Nearly ten years of additional observation has only served to strengthen the opinions here expressed :

"In connection, it may be expected of us to say something in regard to what is termed '*acclimation*.' This term is in very common use, and is well understood to mean, *the habituation of a person to a special climate.*

"It is but reasonable to suppose, that man, 'the master-piece of his Creator,' and 'the inheritor of the earth,' was designed to live in every portion of the globe which is supplied by the hand of nature with the means of subsistence, or accessible to commerce and the arts. Yet, so great is the difference of soil, climate and attendant circumstances, in the various regions between the tropics and the poles, that no race of animals is capable, at once, of enjoying equal health in them all. There is required a certain adaptation of the constitution to each, which can only be attained through the gradual changes effected by time and exposure. Independent of the peculiarities of climate, soil, water, etc., to be found in different regions, it is presumable that there exist in the atmosphere over certain localities, deleterious gasses, effluvia, or emanations from the earth, which exert their most powerful effects upon the living system when it is first exposed to their influence ; but to which the system may become gradually inured in the process of time. To become accustomed to these peculiarities of soil, climate and noxous effluvia, is what we term being *seasoned* or *acclimated ;* and it is wonderful to witness the capabilities of the human system in this respect. There are but few individuals who can make a great change of residence with perfect impunity. With the great majority of people, it is done at the peril of their lives; but the effects are very different upon different constitutions. Some do not become seasoned until they have suffered the severest form of endemic fever belonging to the climate and locality; others become gradually and thoroughly acclimated without ever suffering an open or severe attack. There are persons who have resided in New Orleans twenty years without ever having had yellow fever, whilst others have had it two or three times. Strong attacks of the severest forms of our remittent, bilious and yellow fever seem to cause a modification of the system, which secures to the individual a greater or less immunity from subsequent attacks. Attacks of the milder forms, as ordinary intermittents, effect no such immunity; but, on the contrary, when frequent, lead to permanent engorgement of the spleen, and cause an increased liability to the complaint. The term acclimation is just as familiar to the inhabitants of all the southern portion of the Mississippi valley, as it is to the citizens of New Orleans, and is used to express the same idea, viz: that persons coming from a northern climate and settling there are very liable to have attacks of fever during the first two or three years, but afterward become quite exempt. This fact is so well known as to cause a considerable difference in the valuation of negroes, and even horses and cattle. An acclimated negro, horse, or milch cow, commands a higher price than an unacclimated one. We shall not, at this time, attempt to explain the nature of the change effected by acclimation, nor the manner in which it is brought about; but it is a fact confirmed by long experience and common observation.

"Believing, as we do, that yellow fever is only one of the forms or types of endemic, malarious fever, witnessed almost annually in this city, and less frequently at many other places in the South, we may state the fact, that those who have suffered *severe* attacks of it, or even *mild* attacks, during severe epidemic seasons, certainly remain quite secure from subsequent attacks; especially if they continue to dwell in the same locality. But that they are equally as secure as those who have had small-pox, measles or hooping-cough, as is maintained by some physicians, we cannot for a moment admit. Our own observation, if we have seen aright, is at variance with this position. It is a common remark among persons who thought themselves acclimated, when attacked by fever again, during a sickly season, to say, that 'if they were not certain of having once had yellow fever, they would think they had it again.' The truth is, *they did have it again*;* and if, by neglect or malpractice, the disease had run on to a dangerous stage, or to death, all doubt would have been removed.

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* Dr. Harrison says he had known persons to have yellow fever two or three times, but he never knew such cases to terminate fatally.

But, fortunately, the partial acclimation attained so fortifies the system against the malign influence of the morbific cause, that the tendency to death is not near so strong as in unacclimated subjects; therefore, most generally, they are easily relieved, and come to the conclusion they have not had yellow fever a second time.

"It is a common belief, both in the profession and out of it, that the creoles, or natives of New Orleans, do not have yellow fever at all; but in our accounts of the epidemics of 1847 and 1848, we have given the testimony of some of the most respectable physicians to the contrary.†

"From the foregoing remarks it follows, then—

"1. That persons coming from more northern latitudes to this, have to undergo an acclimation or seasoning, before they become secure in the enjoyment of good health.

"2. That this acclimation may be attained without sickness; but that, most generally, it requires the endurance of one or more spells of the customary endemic fevers.

"3. That an attack of the endemic yellow fever effects greater security against subsequent attacks, than any form of fever seen in the country; but the remark is applicable, in some degree, to all of them, excepting the ordinary mild intermittents.

"4. That persons may have yellow fever more than once, though it is evident that those who have had one plain attack, usually have little or nothing to dread from subsequent attacks.

"5. That creoles, or natives of New Orleans, may have yellow fever—though generally they have it in a very mild form.

"So much for our present views of acclimation. The subject is full of interest, and we shall probably recur to it, from year to year, as our experience is enlarged."

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PELVIC ABSCESS.

By D. Warren Brickell, M. D., Prof. of Obstetrics, New Orleans School of Medicine.

In the last number of this Journal I reported a case of pelvic abscess which resulted fatally. The following case, the result of which has been most satisfactory, will, I think, in conjunction with the first, tend to substantiate the mode of treatment adopted for the evacuation of the contained purulent matter:

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† New Orleans Medical and Surgical Journal, 1848 and 1849.

Case.--On the 5th of October, 1857, I was requested by Dr. A. Hart, of this city, to see the slave woman of Mr. A., in consultation. Dr. H. had but recently been called to see her, and seeing that she was laboring under some grave pelvic disease, he asked my assistance.

I found a fine-looking and stout negro woman, about thirty-five years of age, lying on her back in bed, with her thighs flexed on the body, and complaining of constant suffering of a "dull aching kind" in the pelvic region. Her countenance was indicative of distress; her pulse was about 95 per minute, but full and regular; skin of natural temperature, but rather dry; tongue moist, but coated white; bowels rather constipated; sleep much interrupted by pain whenever she turns at night. She said she had been suffering in the pelvic region, more or less, for two or three months; had been examined by physicians, and had been pronounced a case of "slight displacement of the womb." Her last physician said "the womb was pushed a little to one side, but would soon get well."

External examination proved the existence of a well defined tumor in the left iliac region, about the size of the fist, prominent, rather painful on pressure, and, to my mind, fluctuating under the fingers. Vaginal examination showed a perfectly healthy vaginal mucous membrane, (slight leucorrhea excepted,) but there was a great deal of pain consequent on dilatation of the speculum. It was very difficult to expose the os uteri, and, when exposed, it was found looking directly across the vagina from right to left. Evidently, the body of the organ was pushed over by the tumor on the left side. Finding about half a teaspoonful of creamy discharge immediately in the region of the os, I at first suspected communication of the tumor with the cavity of the uterus, but subsequent investigation proved it to be leucorrheal discharge from the vagina.

Patient now told us that she had been troubled with fevers, and sometimes "slight shiverings," for several weeks past. I was satisfied of the existence of an abscess, and of the propriety of exploring it through the abdominal walls, as it was there most prominent, but we determined to call Dr. Choppin in to see the case. On the 8th of October we met at the house of the patient, examined her carefully, and determined on exploration. We were satisfied that we should find pus, but, such was the immobility of the tumor and the protracted duration of the disease, we could not determine the seat of the same. The tumor was too movable to be under the pelvic fascia, and it was too firmly fixed to be abscess of the ovary, unless it had become secondarily tied down by adhesive inflammation. That it was the latter, I have always been disposed to think, though no positive conclusion could be arrived at.

After due reflection, then, the fine exploring needle was plunged carefully but deeply into the most prominent point of the tumor, when out gushed pure pus of a light chocolate hue. Of course we let it run, and in about half an hour there was nearly a quart discharged, with almost complete subsidence of the tumor, and great relief to the patient.

It was now determined to make a free opening, allowing the pus to flow uninterruptedly, and contraction and obliteration of the walls of the abscess to take place. To make a larger opening now, with either knife or trocar, would be to run the risk of escape of pus into the abdominal cavity, and consequent peritonitis; so we determined to effect adhesion of the peritoneal surfaces by means of caustic potash, and then open freely with trocar and bistoury. This was done precisely as in the other case, and in ten days after the opening was made, the patient was sitting up with the wound closed, and nothing of the tumor remaining except a lump about the size of a hulled walnut adherent to the under surface of the abdominal walls. One month after the operation, I saw her, and she said she was perfectly well, and when I last saw her (about three weeks ago) she was well.

Remarks.—There can be no doubt as to the propriety of the operations resorted to in these cases—first, the operation of exploration, and then the operation for complete evacuation of the contents of the tumor. The unfortunate result in the first case was more to be attributed to delay than any thing else, though the disease was of a graver nature than in this last case. That the tumor in the last case was not under the fascia I feel satisfied, because pus had evidently been formed long before we saw the patient, and if it had been formed under the fascia, it would have been diffused as in the first case. Instead of this, however, we have none of the constitutional disturbance consequent on diffusion and extensive adhesion as a result, and when the pus is ultimately let out, the evacuation seems to be complete, the walls of the tumor collapse and speedy restoration is the result.

That the mode of operating (with the caustic) is the proper one, is clear, and I have no doubt that many of the cases of dropsy of the ovary might be most successfully operated on in this manner.

OXIDE OF SILVER A REMEDY FOR TAPE WORM.

MARKSVILLE, LA., 13th March, 1858.

D. Warren Brickell, M. D.:

Should you deem the few remarks herein made, in relation to the oxide of silver, worthy of notice, you can give them a place in the New Orleans Medical News and Hospital Gazette:

Dr. David Murdock, one of the most skilful and eminent physicians in the South, and a close and indefatigable observer of pathological phenomena, stated to me in a conversation some days since, that having to administer the oxide of silver, as a palliative to a patient under his care for organic disease of the stomach, much to his surprise, and contrary to his expectations, large portions of tape worm came away. This induced the Doctor to continue the use of the medicine, when still larger portions of the worm were brought away, much to the relief of some of the most painful and urgent symptoms. Query? Did the oxide act as a specific vermifuge in this case? Or did it modify or remove the pathological lesion of which the presence of tape worm was an evidence?

With high respect, I am sir, your obedient servant,

G. E. ELMER, M. D.

SPECIAL SELECTIONS.

[Notwithstanding some of our readers may be subscribers to the *London Lancet*, and, consequently, may have read the following selection in the late number of that journal, still we know there are many who would not see the same if we did not republish it, and we think the ideas advanced are so valuable that they should be disseminated as thoroughly as possible. Not that many of us who practice medicine in this Southern country have not learned by sad experience the strength of the arguments advanced against blood-letting, but because these arguments are nowhere else to be found thus forcibly set forth in print. We have long been convinced that a revolution was surely, though slowly, arising in England on this subject of bleeding in idiopathic affections, and we now predict that a change will also come over the spirit of the dreams of our Northern brethren on this important subject. As surely as there are lancets and blood, there is no sense in bleeding a man under the circumstances so plainly marked out in this paper. We heartily commend the paper to our readers, and ask them to study it carefully.—Eds.]

ON THE INFLAMMATION AND BLOOD-LETTING CONTROVERSY.

By W. O. Markham, M. D., F. R. C. P., Physician to St. Mary's Hospital.

(*Continued from February No., page* 135.)

The above facts, then, seem to suggest to us reasons why bleeding should not in many cases be the proper remedy, and is frequently found inefficacious in internal inflammations. With regard, indeed, to all those internal inflammations which manifestly arise as secondary to some other disorder, the pleurisy and pneumonia attending phlebitis, for example; the pneumonia incidental to typhus; the pneumonia associated with pericarditis and rheumatic fever, etc.,—with regard to these, it is admitted that the treatment of the local pneumonia is a matter of quite secondary consideration to the treatment of the general disorder. But the so-called idiopathic pneumonia—the pneumonia which has no such relations to any primary disorder—is placed in quite another category of diseases. It is, if I mistake not, generally regarded as an acute inflammation, arising from a local source of irritation, much in the same way that inflammation is seen to rise, consequent to a local injury on the external surface of the body; but a little consideration may perhaps show us that such a view of its nature is not altogether satisfactory.

Idiopathic pneumonia arises obscurely—that is to say, without any distinct or manifest cause to which it can with certainty be ascribed. It cannot be looked upon as a purely local inflammation in the sense in which we should regard pneumonia arising from, say, a gun-shot wound. Every one must admit, on the face of it, that there is a distinction somewhere in the two cases. Of a hundred persons who are exposed to the self-same external conditions, atmospheric, hygienic and dietetic, one only is attacked with so-called idiopathic pneumonia. But this is clearly a very different result from what would have happened had they been all subjected to a similar traumatic injury of the lungs; then in the whole of them some degree of local inflammation, varying doubtless much in extent, would probably have been excited.

In the first case, I think it must be admitted that there exists a peculiar, an abnormal, condition of the system which predisposes the individual to the inflammation. It cannot be imagined that the mere local application of the cold air,* or of any other of the exciting causes to which he had been subjected, excited the pneumonia, for then assuredly the other ninety-nine of his companions could not all have escaped the potent influence. Now, if we admit the fact of this predisposing condition of the system, we admit, I imagine, that the system is diseased anteriorly to the moment that local inflammation arises; and, therefore, that the pneumonia is not, strictly speaking, idiopathic, but falls under the class of those constitutional disorders in which the local inflammation is merely of secondary consideration. In the indiopathic inflammation there is some obscure influence to which the fluids or solids of the body, or both of them, have been subjected before the pneumonic symptoms arise; but in the traumatic inflammation, on the contrary, whatever constitutional disturbances appear are consequences, not antecedents, of the local inflammation. Is it, indeed, unreasonable to believe that we shall one day place idiopathic pneumonia in our nosological systems amongst the fevers, ranking it, for example, in the same class of diseases as rheumatic fever? Acute rheumatism is no longer called arthritis, however prominent and severe the arthritic symptoms attending it may be; we do not consider them the essence of the disease, but only as certain of its consequences. Neither do we treat the arthritis; we direct our attention mainly to that disordered condition of the system which has, we know, provoked the arthritis. We find a fever also raging coincidentally, and we call the disease rheumatic fever. Why, in like manner, may we not be led to call idiopathic inflammation of the lungs pneumonic fever? Now, if this view of the nature of ordinary pneumonia be correct, it is evident that the inflammation, as regards its treatment, must be considered under the head of those diseases which, for want of a more particular knowledge of their intimate nature, we designate as blood diseases; it is a disease in which constitutional disturbance exists anterior to the manifestation of local inflammation. The same may be said, by way of illustration, of ordinary catarrhs, cynanche tonsillaris, pleurisy, herpetic and other eruptions, erysipelas, and, in fact, perhaps, of almost all inflammations, whether internal or external, which do not take their origin from direct local injury.

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*Cold has been, from the earliest ages, and still is, regarded as a most powerful exciting cause of disease. Admitting it to be so, it is surely evident, in the instance of pneumonia, for example, that the cold acts in some manner on the system, not directly on the lungs. The lungs do not feel the effects of the cold directly; we must rather suppose that some change is brought about in the body, anterior to the manifestation of the local inflammation. The disease falls on the lungs—it might have fallen on the kidneys, on the joints, on the heart, on the membranes of the brain; or, to put the matter in another form, can we believe that cold would excite pneumonia in the lungs of a person perfectly healthy, unless there were preëxisting in that person a predisposition to the disease—i. e. an abnormal state of some parts of the body—its fluids or its solids?

Now, it cannot be denied, that since the days when modern pathology took its rise, the treatment of these disorders has undergone a complete change, and that our views concerning the nature of diseased processes have become clearer and juster. We have learnt—a most important lesson—the extent of our ignorance concerning them, and can measure pretty accurately what is positive and what is hypothetical in our knowledge of them. Amongst other things, we have learnt this, viz: that in disorders of the kind here spoken of, bleeding is not an appropriate remedy. We see distinctly enough the fallacy upon which was chiefly, if not entirely, based the theory of bleeding in former days, viz: "that the morbific matter should be evacuated at the orifice made by the lancet"—the corrupted blood removed from the system. And we find, now-a-days, at all events, that in such diseases loss of blood is not well borne, and is sometimes at certain periods of their progress little less than loss of life.

If, then, we regard idiopathic pneumonia, not as a local inflammation, but as a general disorder, manifesting itself in a particular organ, we shall at once have reason, by analogy, to believe that bleeding is not a suitable remedy for it, and that it never has been so, and is only to be practiced for the purposes defined elsewhere. And the proposition gains strength from the fact that traumatic inflammations of the lungs do seem to demand large bleedings for their cure; at least, this much is certain, despite all theory of change of type, that the extremely vigorous treatment by venesection practiced by Mr. Guthrie fifty years ago, in such injuries of the lungs, is exactly the same treatment as was practiced and found successful, for the same injuries, the other day in our army before Sebastopol.

I have already referred to the undoubted good which results from the local abstraction of blood in the case of external inflammation arising from local injuries; and I think, from this fact, we may very fairly argue that, in traumatic inflammation of the lungs, local abstraction of blood, if it could be effected, would be equally beneficial, and for like reasons. And I cannot see reason to doubt that in such cases large venesections do act as local abstractions of blood, and so act favorably. But in the case of idiopathic pneumonia, as I have conjectured, we have an inflammation of a totally different nature to deal with. The inflammation bears the characters, more or less well defined, of a specific and adynamic disease; the whole body is disordered. We may bleed deeply, and relieve the lungs mechanically, perchance, as in the former case; but the poison is still in the blood—we cannot drain it out; and experience warns us that a stage of deep depression is at hand, which is always dangerous, and is greater in proportion to the degree of loss of its vital fluids which the system has previously undergone. We do not hope to remove an erysipelatous inflammation—an asthenic inflammation—by bleeding or by the use of leeches; but we infallibly know the relief which the application of a dozen leeches gives to an inflamed ankle-joint—asthenic inflammation.

But let us consider a little more particularly why large bleeding should, theoretically, be of such essential service in traumatic inflammation of the lungs, and at the same time mark in how many and important items the case of traumatic differs from that of idiopathic inflammation. In this way we may, perhaps, gain a hint as to the actual amount of benefit which bleeding may be supposed to effect in the idiopathic inflammation.

Now, the subject of traumatic inflammation of the lungs was probably in the enjoyment of vigorous health at the moment of the receipt of the injury which caused it, and we are enabled to seize the inflammation at its very earliest periods. We can bleed during, and even previous to, the congestive stage; and all who advise bleeding are agreed that this is the moment when bleeding is of the most essential service. And, moreover, we have here no primarily deranged and weakened condition of the system to contend with; the malady is sthenic.

But in the case of idiopathic pneumonia, the patient is already in a condition removed from health; his fluids or solids are, in a sense, corrupted before the local inflammation manifests itself. Then, again, the bleeding is not practiced during the early, congestive stage of inflammation. This period, as we have elsewhere argued, has passed away before the remedy is applied. It is during a later period, the stage of exudation, that the blood is taken, and therefore at a much more unfavorable moment. And, lastly, we must not forget that the inflammation of the lungs is, possibly, a necessary part of the specific process attending the fever; not a mere accident, as in the case of the traumatic inflammation.

The bleeding, in both cases, most probably produces a similar change in the condition of the inflamed parts, whenever it is effective for good, and whatever be its mode of action, whether indirectly by dulling the nervous force, or directly by diminishing the mass of fluid, or by both these modes. The bleeding, which is most markedly beneficial in its immediate results, is that where the blood is rapidly taken, and in large amount; and such a bleeding, we can without difficulty believe, may actually derive blood indirectly from the inflamed part itself. We see the fact, indeed, demonstrated occasionally, as when blood is taken in inflammation of the eye, and in erysipelatous inflammation of the skin. Then, while the blood is still flowing from the arm, the red injected part may be seen to assume, for a time at least, its natural hue; and it is corroborated by the cessation of pain, and the freedom of the respiratory movements, etc., which, in inflammations of the chest, so immediately result from the bleeding.

Hence, then, we may anticipate that, in traumatic and in idiopathic inflammations of the lungs, large bleedings will have like local effects on the inflamed parts, provided always that they be practiced at like periods of the inflammation—during its congestive stage, or, at all events, before large portions of the lungs have been rendered impermeable to air by the exudation. In the latter condition, it is evident that the bleeding can be of no local benefit, when the capillary vessels and the air-cells have undergone more or less complete occlusion through compression induced by the exudation thrown out, into and between the air-cells. Bleeding, it is certain, cannot remove this exudation, but it is pretty sure, at such a period of the inflammation, to act ill, in so far as it enfeebles the powers which carry on the process that must be ultimately gone through before the exudation can be removed.

And again, it is not improbable that the good effected by bleeding in traumatic injury of the lung may be effected independently of any influence which it exercises over the consecutive inflammation. It must be remembered that, in such cases, for manifest reasons, the respiratory function is suddenly, and violently and largely oppressed. The injured lung is in great part at once rendered unfit for its proper uses, so that the office of aëration of the blood falls almost wholly upon the healthy lung. The same quantity of blood, deducting, of course, the amount, much or little, which may have been accidentally lost in consequence of the injury, is contained in the body, as it held before the injury; but the aërating surface of the lungs, adapted to it, is diminished by one-half. A large bleeding here would naturally free the obstructed circulation, especially after the manner referred to by Dr. Bennett, viz: by removing the pressure upon and distension of the right side of the heart; and by bringing the quantity of the blood in the body into better accordance with the extent of efficient aërating surface. Moreover, whatever freedom is given to the lungs by bleeding is of immediate service; exudation has not taken place, and there is no complicated process of absorption to be gone through before the compressed air-cells and the capillaries are again rendered free to act; neither, for reasons already given, have we so much to dread a period of depression, as in the case of idiopathic pneumonia.

In idiopathic pneumonia the interference with the respiratory function is more gradually brought about; and, therefore, there is, as a rule, no sudden oppression of the lungs and heart. When the oppression is great, there is also, in all probability, much exudation; and the bleeding, therefore, cannot *pro tanto* be effectual in removing the oppression, for the reason that it has no direct power to remove the exudation. And so it may happen in idiopathic pneumonia that, at the very moment when the symptoms may seem most imperiously to demand it, bleeding is least likely to be of any avail. The exudation causes the oppressed breathing, and bleeding cannot remove the exudation; but, on the contrary, it may, by weakening the system, interfere with its removal.

These are facts of which we must all, in a greater or less degree, admit the truth. They were not understood, and therefore the consequences which result from their interpretation could not have been appreciated by our forefathers. Those two first handmaids of Medicine, Pathology and Diagnosis, have at all events changed their type since the days of Cullen and Gregory; and this alone explains satisfactorily enough to my mind why the practice of to-day differs from the practice of former times, especially in this particular of blood-letting.

When I look back to those times, and find that the most illustrious physician of his day took "spasm of the extreme vessels" to represent his idea of the pathology of inflammation, and when I find that the same physician thus exemplifies his diagnostic powers—"an acute inflammation of the pericardium is almost always a part of the same pneumonic inflammation I have been treating of, and is not always to be distinguished by any different symptoms, or if it be, does not require any different treatment;" and when I compare this pathology and diagnosis of diseases with our own, I must say that, so far from being surprised at an associated change in the treatment of those diseases, I can only regard it as a necessary and logical conclusion. Most cordially do I agree with Dr. Bennett, in considering this change of treatment as the pure and consequent result of our own advanced pathology and diagnosis.

Loss of blood is in all cases, in itself, an evil; and we may, I suppose, admit as a general proposition that in diseases of a specific nature it is an especial evil, inasmuch as that the specific disease indicates a general weakened condition of the system—a condition wherein all the vital powers are more or less lowered in force. In such diseases there is a certain process, of greater or less duration, and of greater or less violence of action, which must be gone through before the system is disburdened of it; and the physician's care is, not to arrest its progress, (for of that his art is incapable) but to guide the subjects of it through the struggle which awaits the vital powers until the process is perfected. He has to watch, on the one hand, lest individual organs be fatally injured, and, on the other, to sustain the general system. And for this reason it is that large bleedings, *in such cases*, are fraught with danger, and for this, that we should be cautious in giving temporary relief when we have such a contingency to provide for. The general effects of loss of blood are, as we have seen, different in this case from what they are in a case of inflammation resulting from a traumatic injury. In the latter instance there is not that same struggle to be expected, that same period of depression to be provided for, as in the inflammation associated with a specific disease.

In idiopathic pneumonia, for example, which, as we have already suggested, may perhaps be a specific disease, bleeding is therefore not a desirable remedy. We resort to it, not to relieve pain or uneasiness—that would be unjustifiable—but because an organ, the constant integrity of whose functions is absolutely necessary for existence, is impaired. Life is threatened if immediate relief be not given. The bleeding disgorges the capillaries (directly and indirectly) and gives that relief.

And just exactly in the same way does bleeding give ease and relief in cases of acute rheumatism. In such cases there is abundant evidence to show that the local abstraction of blood has a power of subduing, temporarily at all events, the arthritic inflammation, and of so giving relief. Whether the present comfort thence obtained compensates for the future injury and weakness resulting from the loss of blood, is another question, which most physicians, I take it, now-a-days, will answer in the negative. And the real fact in practice is, that we do not thus give temporary relief to those suffering from rheumatic arthritis, however much we may be assured that the leeching would give relief; and the reason why we do not act here as in the case of idiopathic pneumonia is to be sought in the fact, that in rheumatic arthritis the inflammation of the joint does not imply injury to an organ, such as threatens life, which pneumonia does. The sacrifice implied in loss of blood, therefore, is not demanded by present necessity, as it is in the case of inflammation of the lungs. Moreover, what has been said concerning the effects of general bleeding upon internal inflammations is not contradicted, but rather confirmed, by what we observe in local abstractions of blood from inflamed external parts. The immediate and positive benefits which so frequently are observed to follow upon the taking of blood from the neighborhood of an external inflammation are so manifest that we cannot for a moment doubt the reality of the fact. The relief to the pain and the diminution of the swelling, in such cases, are not to be misinterpreted. But, however well marked the effects of such local bleeding may be in such cases, they are not so clearly manifest in the case of internal inflammations, where the blood has been taken externally from a part in the neighborhood of the inflammation; and those, indeed, who do not deny the benefits arising in the former case, hesitate to admit the fact as applicable in the latter case; because they cannot comprehend how leeches upon the surface of the thorax, for instance, can draw blood from an inflamed lung, or, applied to the abdomen, can subtract blood from the stomach or intestines.

Now, if we take for our guide here merely anatomical considerations, there appears to be much force in the objection; for it may be clearly shown that, with a very slight exception, there is no direct communication between the bloodvessels of the skin and those of internal organs lying beneath them. But, nevertheless, that local abstraction of blood does, in certain cases of congestion and of internal inflammation, give much relief, I think few people can doubt; and it is, therefore, necessary to explain how this happens, and to show that the fact does not in any way tell against the views here held concerning the mode of acting of venesection.

Now, the main symptoms for which the local bleeding is used in pneumonia, for example, are pain and interference with the respiration; and these are the symptoms to which it mainly gives relief. But it is clear that, in ordinary cases, the relief does not arise through the abstraction of blood directly from the lung itself; it comes indirectly. We well know that pneumonia is seldom unaccompanied by pleurisy, and that the pleurisy is the chief source of the pain complained of; but pain is not constantly present in pneumonia, and for the reason, we may suppose, that the pleurisy does not invariably attack the parietal layer of the pleural membrane. When this layer of the membrane, which lines the thoracic walls and has a distinct vascular connection with the parts which form the walls, is inflamed, then it is that pain especially arises; and this is the case in which local bleeding is of service, for here the bleeding acts directly upon the inflamed part, and undoubtedly can abstract blood directly from the inflamed pleura and its adjuncts. The same facts, I believe, hold good in the case of pericarditis, explaining the cause of the pain and the mode in which local bleeding acts, when it really does give relief. The pain—excluding that which may result from oppression of the circulation produced by paralysis or other interference with the heart's action—which attends pericarditis arises, probably, not from the inflammation of the pericardium itself, but from the extension of it to the neighboring pleural surfaces, and to the parts in the anterior mediastinum. Pain is far from being a constant attendant upon pericarditis, just as it is in pneumonia; but when the pain does exist we may surmise that the inflammation has reached beyond the pericardium,* to parts where it is more or less amenable to the local abstraction of blood, and after the manner above supposed.

In the case of pleurisy, the ease to the respiration which may follow upon such local bleeding must result from the return to the thoracic walls of that freedom of action which the pain attending their movements had temporarily abated. Besides this, it is not impossible that local bleeding may abstract blood from the inflamed lung; and perhaps does so more frequently than we imagine, and especially in cases of long-standing diseases of the lungs. Every one who is familiar with post-mortem examinations knows how frequently the opposing surfaces of the pleura are found extensively attached by false membranes, even in cases where the lungs are healthy, and where there was no suspicion of any pulmonary disease during life. Where there is a history of pulmonary disease we almost invariably find such attachments. But these false membranes are vascular, and therefore form channels of direct communication between the lungs and the surface of the thorax. Does not this explain the temporary relief which the application of one or two leeches beneath the clavicle, in cases of tubercular disease, in emphysema, etc., so frequently affords? The vascular nature of these false membranes, therefore, must be taken into consideration when we speak of the mode of action of external local bleedings on internal inflammations, and of the action also of blisters.

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*I am, of course, as said above, excluding from consideration here the painful feelings of oppression and suffocation which result from large effusion into the sac, and from interference with the heart's action thereby produced.

There is one very important fact which seems to come very positively out of this argument, viz: that local bleeding can be of no *direct* service in pericarditis, or in pneumonia, or in congestion of the lungs, excepting when pleurisy exists, or when the pleural surfaces are adherent by vascular attachments. Its chief benefits, therefore, must be derived from its effects on the walls of the thorax. The same observation may be applied in the case of inflammation of the abdominal walls. The fact, though simple enough, is worthy, perhaps, of more consideration than is generally given to it, and explains why leeches frequently give such marked relief, and why they sometimes fail to do so. It also points out to us those cases in which we may especially anticipate that local bleeding will give relief.

The more any one reflects on the subject of bleeding and its effects, the more, I think, he will be convinced that the principles which have been at various times laid down for our guidance in the matter are of an unsatisfactory nature. What we evidently need is, that the proper uses of the practice should, if possible, be so explained as to bring them into some better accordance with the pathology of the present day. The principles referred to are the principles of the ancients, and therefore cannot but be ill-suited to modern pathology. One cannot deny, it is true, that in the present condition of medicine, the conclusions arrived at respecting the general therapeutical effects of remedies must be drawn from their results as observed in individual cases, rather than from any *à priori* theoretical considerations of their action; and in so far, physicians of former days might be considered as able judges of the matter as ourselves: but, nevertheless, theory, in the matter of bleeding, aided by modern light, may be allowed to suggest a few things as regards its practical effect. We cannot bleed, indeed, without in some way exercising our judgment as to the *modus operandi* of the loss of blood. And if, indeed, it be possible to argue usefully of the way of acting of any curative means, we may surely anticipate that we should especially be able now-a-days to do so in the case of bleeding—the operation of the remedy being so direct, and its immediate effects so clearly appreciable. A candid consideration of the subject will, I am persuaded, show us that theoretical considerations of the action of blood-letting are quite in accord with the results of our practical knowledge of it at the present moment.

Admitting that a typical case of pneumonia, as described by Cullen, was in his days, and in all days before him, and still is at the present day, a case in which bleeding is a right remedy, *if practised at a right moment*, and that the necessity of the operation may be judged of from the severity of the symptoms—from the state of the pulse, of the breathing, from the fever, etc.—admitting this, still it does not follow that the same symptoms could have been a sufficient guide to practice in ordinary every-day cases. We know, indeed, that such well-marked symptoms are not our usual guides, and for the reason that they belong only to the rare and exceptional case. And if we enter a little into this matter, I believe we shall hardly fail to arrive at the conclusion, that he who trusts alone to these signs, as guides to the practice, will find himself in the greater majority of cases, reposing his confidence in very fallible aids.

Let us consider the question a little more closely. Every one will admit, I suppose, that bleeding is a present sacrifice which is made for the future good of the patient; that it is in itself an evil, but that the diseased condition for which it is practised is a greater evil; that, in short, when practising bleeding, we choose the least of two evils. Remembering this, let us now endeavor to see how, according to theory, bleeding may act beneficially in inflammation. If we reflect on the series of symptoms, as described by Cullen, which are said to be, in an especial manner, indicators of a necessity for bleeding in inflammation, we shall find that they are signs which point out, in a marked manner, that the action of some vital organ is seriously interfered with; and, indeed, so interfered with, as immediately to endanger life. This was observed, in an especial manner, in what was called by him pneumonia, and for the very manifest reason, that here the lungs—organs, whose action can only be momentarily arrested with impunity—were involved in the disease.

I have mentioned elsewhere facts, which seem clearly to show the immediate and manifest relief to urgent symptoms which large bleedings give in cases of traumatic injuries of the lungs; and this fact affords us, I think, a clue to their beneficial effects in the pneumonia of Cullen, as above referred to. How do they act? They lessen the sum of the blood in the body; they reduce the action of the heart; they lower the nervous force of the heart; and, consequently, they diminish the quantity of the blood in the vessels of the inflamed organ. They remove the congested condition of the organ, and so leave it free again to perform its vital function. But here we may be met with the argument, that if the loss of blood does not arrest the inflammation, it had been better for the patient if the congested condition had not been interfered with, for now the inflammatory process has been improperly interrupted, and will not come so satisfactorily to its conclusion. Are not, it is said, most—perhaps all—diseased processes, as we call them, little more than efforts of Nature to relieve the system of something within it which is injurious to its well-being, or to repair its injuries? Is it not the physician's part rather to guide the patient happily through the disease which oppresses him, than to attempt to suddenly arrest and destroy the disease? We are little more than Nature's handmaids here. We do not heal the torn skin, or the broken bone; we only abstract the patient from those conditions, and those surrounding circircumstances, which may arrest the healing and the union: the special act of healing is as much a vital action as the growth of the man himself. The elimination of tubercle from the lungs involves a diseased process—of a most fatal nature; but still is not that diseased process necessary to its elimination? With that process the physician does not directly interfere. He can only hope so to regulate and guide it, as to bring a happy issue with its termination. What proofs are there that we can cut a case of specific fever short? And if, as we have suggested, it may be, that idiopathic pneumonia is a fever of this kind, it is not impossible that neither can we cut pneumonia short; that in spite of all our efforts it will run its course; and that large bleedings may affect for bad the progress of the disease. No one will deny, it is said, that large bleedings, which do not cut the inflammation short, are frequently followed by results injurious to the cure of the inflamed part; and hence it is supposed, that the congestion of inflammation is a right and necessary part of the process, and that the inflammation *per se* may come to a better conclusion, if left to itself, than if directly interfered with.

If these arrangements are correct, how, then, may we suppose large bleedings to be of great service? Clearly, I think, by giving immediate freedom to the play of a vital organ, whose action the inflammation has seriously impeded—so as to imperil life. In certain inflammations of the brain, and of the lungs, the effect of bleeding is strkingly exhibited, because these are organs whose arrest of function is instantly appreciated. Bleeding may all the while be a thing which acts not well on the inflammatory process *per se.* This may be; but I do not stop to discuss the question now: it is practised, as I suppose, here, not so much to reduce the inflammatory process, as to set free the action of a vital organ. And we may by analogy fully believe that bleeding may give a like freedom of action in like cases of inflammatory congestion of other organs—of the kidney and liver, for example—whose functions when deranged give less peremptory and immediate signs of their disturbance; and where, consequently, the useful effects of the bleeding are less likely to be so clearly and instantly manifested.

The chief good effects of bleeding, then, in pneumonia, result not from any power which the bleeding has in arresting the inflammation, or in removing (as the ancients would say) the ill-concocted humors, but from the immediate relief which it gives by disgorging the congested capillaries, and giving freedom to the action of a vital organ. The large bleeding acts here exactly as it acts in traumatic injuries of the lungs. It is an evil as regards the future of the patient; but it is an evil which his present condition calls upon him to submit to. Its main object is immediately to relieve the impeded respiration.

The corrollary of this proposition is, that bleeding will not produce any beneficial results in cases where that symptom for which it is chiefly practised—the impeded respiration—does not depend upon congestion of the vessels. This is a most important particular; and, if true, explains, to a great extent, the discrepancy of observers' opinions relative to the effects of bleeding. Its full appreciation, moreover, will, perhaps, enable us to predicate, with some degree of certainty, as to what may be the results of a large venesection in a given case. If, true, moreover, it gives us, I venture to think, a plain, practical guide to a knowledge of the case in which bleeding may be most successfully practised. To attain possession of all the facts requisite to such an appreciation, in the case of thoracic diseases, a knowledge of stethoscopy is absolutely necessary. Those signs which guided the ancients might have guided them aright in many cases; but that they also frequently guide them amiss, I cannot, from these considerations, doubt for a moment. They could not tell when, and to how great a degree, the difficult respiration of pneumonia depended upon congestion of the lungs; nor did they know when it depended upon the presence of inflammatory exudation; nor when upon diseases of the heart, etc.

We can appreciate these facts, and their due appreciation is clearly of the highest importance in the matter of treatment of diseases by bleeding, and gives us an infinite superiority over our forefathers, such as will account for the difference of our treatment from theirs. And I must add, that I do feel a degree of surprise when I find high authorities asserting that we of this day have gained so little over our forefathers in a knowledge of the application of remedies. This very dispute, as I see it, is a manifest proof of our vastly superior information, and of our advances towards a better knowledge of the art of medicine—to a more correct knowledge of it most assuredly. It is something, at all events, to have knocked away the stays of ignorance and prejudice which girded up the loins of old Medicine. Our advances in the power of curing the sick may be small, but, in one sense, the progress is admirable. We have begun to learn the bounds and limits of our powers as physicians and curers of disease; we are beginning to learn what we can do and what we cannot do; we cease to arrogate to ourselves the merit of those kind offices which nature, not our art, has performed. And all this our forefathers did not know, and could not have known, for they had not the means of knowledge which have led us to such better things. They thought, through their remedies, that they had a power over and could control and destroy the disease after a fashion which we know is impossible; we have learnt to guide the patient gently to his cure through the *via mala* of disease. If we have gained nothing more than the being rid of the vicious theories which have hitherto directed—and of course viciously directed—the hand of the practitioner to his work, our gain is immense. Error was an essential and necessary associate of the practice of former days. It is not so now; our errors are voluntary errors, for which we are responsible. We can mark where our positive knowledge ends, and we are capable of estimating at their proper value the worth of the theories and of the practices which we follow out in curing diseases.

But to return to the subject. It is evident that interference with the function of respiration may result, not only from congestion of the lungs, but also from many other causes, and, indeed, from a further stage of the inflammatory process—from exudation; and it is equally evident that no amount of bleeding can remove the exudatiion. In reducing the distension of the blood vessels of the lungs, we have supposed that large bleedings may have a direct action; but we may be sure that, once the inflammatory lymph has passed beyond and out of these vessels into the air-cells, and into the parenchyma of the lungs, bleeding can have no further direct influence over it; and we may suspect, on the principles above referred to, that removal of blood will be now merely an interference with the process, which has to be perfected, before the exudation is removed.

The cases of pneumonia that we ordinarily meet with in practice are partial pneumonias—where only limited portions of the lungs are affected by the inflammation. In such cases, as a rule, we do not bleed; and why? because, as I suggest, in accordance with the views here stated, we do not find the respiratory functions seriously endangered. And it was this interference with the function of the part which was most probably the main guide of Cullen and Gregory in calculating the degree and the necessity of bloodletting in pneumonia. Like ourselves, they did not bleed in these cases of limited pneumonia, because, as Dr. Alison suggests, they probably never discovered their real nature, and looked upon them as simple fevers. But if this impeded respiration was of so much value to those physicians in deciding them as to the necessity of bleeding, we may understand how erroneously they must have often practised it, when we call to mind the meaning attached by them to the term "pneumonia." They included, as before observed, under that term all affections of the thorax which give rise to certain given symptoms. But diseases of most opposite natures give rise to those same symptoms, and they, therefore, must have included under one head, and treated in a similar manner, diseases differing completely one from the other. We shall presently see how fatally erroneous such practice must have been.

The useful effects of large bleedings I suppose, then, to arise—not altogether, but in the main—from their mechanical action; they take the blood substantially and directly from the inflamed part; they so reduce the action of the heart as to cause a diminution in the quantity of blood sent to the part.* Bleeding, therefore, must be of material service only during the congestive period of inflammation, or at a very early period of the stage of exudation—i. e. before the exudation is great, or has extensively stopped the aëration of the blood. It is not, and, as it would seem, never has been needed in pneumonia, except when the inflammation is so extensive as immediately to endanger life, by arresting respiration. Partial pneumonies do not demand it; they do not prevent an aëration of the blood sufficient for the purposes of life. If this view be correct, it would seem to follow that the bleeding is not a remedy especially directed against the inflammation, *per se*, but that it serves its purpose by ridding the lungs of that interference with their vital functions which is constantly endangering life.

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* Abstraction of blood from an inflamed part *must* influence, in some way, permanently or temporarily, and ill or well, the inflammatory process. This is evident, and independently of its mechanical action; but this point does not concern the main argument here held.

Bleeding, by like reasoning, is more useful, the more extensive the inflammation; but it is then also most dangerous, because the subsequent stage of depression of vitality will be greater. Bleeding also must be of less use, and more dangerous, in proportion as the signs of exudation (dull percussion and crepitations, etc.,) are observed more widely over the thorax; for under such circumstances we are certain that the difficult respiration cannot be fully relieved by the bleeding, inasmuch as it depends in great part upon the exudation, which, as we have seen, bloodletting cannot touch. It is dangerous now, because, in order that the lung should regain its integrity, the exudation must be removed, and strength and vital activity are required for its removal—things which loss of blood is not likely, especially at such a moment, to promote, but the very reverse.

Now, from what has been here stated, it would appear that bleeding in partial pneumonia is, as a rule, not requisite during the congestive stage; but it would probably produce no marked ill effects—a fact which we shall have occasion again to refer to. When the pneumonia is extensive, it may be of great service by mechanically freeing the action of the lungs. But as the exudative stage comes on, and the congestive passes away, and in proportion to the extent of the inflammation, the bleeding becomes injurious and dangerous. In the purely exudative stage it is useless and highly injurious: it is useless, because it cannot free the pulmonary circution; and it is injurious, because it weakens the vital force, that is, removes the blood which is requisite to carry on life during the removal of the exudation.

And this explains how it is that, in some cases of inflammation, a little bleeding may be of marked benefit, and how in others it may be a fatal bleeding; and how, again, larger bleedings may apparently produce no ill effects at all. When the exudation is extensively thrown out in pneumonia, the respiration is proportionally impeded or arrested; and we must remember that arresting of the respiration means arresting of the process of sanguinification—stoppage of the blood-making process. So that, at the very moment when much vital force is required to carry out the inflammatory process to its completion, the production of that main agent which supplies—which is—the vital force is in great part arrested. In partial pneumonia, the sanguinification still goes on.

It also now becomes apparent, according to the views here held, of how little avail, or rather how injurious, must have been the large bleedings practised by Cullen and others, in cases where the cause of the pulmonary congestion—that is, of the difficult respiration—was fixed in the heart; for be it remembered, it is not in the primitive and early stages of acute endocarditis or pericarditis, that the lungs begin to suffer, but as a rule it is in a secondary way, and as a consequence of the mechanical impediment to the circulation, which is situated in the heart itself. In such cases, before the lungs become involved in the disease, the system has already reached a condition of feebleness such as renders bleeding a formidable and dangerous practice. The congestion of the lungs marches *pari passû* with the obstruction in the heart, and, so long as this remains, is an irremediable fact; and it increases in degree as the action of the heart becomes feebler, and the injury of the organ greater; in a word, the congestion is most complete, and bleeding (in the sense inferred) most needed, at a moment when its practice would be most dangerous, and, for the reason given, most ineffectual. Moreover, the removal of the congestion, even if effected, could not give relief, as supposed in the case of pneumonia, for its cause is still alive and active in the heart. Now, previous to the days of auscultation, there were admittedly no means of distinguishing between the different diseases of the thorax, so as to tell with any degree of precision the cause of the impeded respiration in any particular case; nor was it possible, from the symptoms of fever and impeded respiration alone in pneumonia, to learn how much of the obstruction to the play of the lungs was caused by congestion, and how much by exudation.

A consideration of the preceding particulars given us, I think, some explanation of the manner in which bleeding acts; and also indicates that there are many cases of internal inflammations in which bleeding must be considered a fit and proper remedy. Perhaps, therefore, in these modern days, we may have wrongly run into an opposite extreme of practice from that which characterized the practice of our forefathers. The total abstinence plan may be as really wrongin practice as it appears to be in theory.

Moreover, we from time to time note, in the dead-house, a condition of certain of the internal organs, quite independent of inflammation, which seems to me to indicate, that bleeding would have been an appropriate remedy in the case, and might have done something during life towards removing the immediate cause of death, and so, perhaps, have saved or prolonged existence. Congestion of the lungs—such a degree of it as is incompatible with life—appears to be the main pathological condition in such cases. This is a state which may, or may not be associated with a mechanical impediment to the circulation, situated in the heart or its great vessels.

In these cases, it is true, that the active cause of the congestion is not situated in the lungs themselves, but in the heart; but the immediate cause of death in them, as in the case of death from congestion and inflammation of the lungs, is alike—viz.: the arrestment of the respiratory function; and in both cases, I take it, that the good effects of the bleeding are produced much in the same way. In the one case, the oppressed and distended heart is relieved, and so regains vigor enough to force onward the blood in the lungs; and, in the other, the removal of the congestion of the capillaries gives freedom to the pulmonary circulation. Hence, in both cases the respiratory function is restored. We should here keep in remembrance the experiment of Dr. John Reid, who showed that when the heart (in cases of asphyxia) had ceased to act, a few ounces of blood taken from the jugular vein—i. e. from the right side of the heart—would once again give activity to the organ. Of course, where the congestion is of long standing, and is the consequence of extensive disorganization of the heart, we cannot hope that such treatment will be of any avail. *A priori*, we should expect that the bleeding would be effective in cases where the congestion has been rapidly produced, and where the degree of disorganization in the heart itself is not such as to render recovery hopeless. And do we not, indeed, continually meet with cases of cardiac diseases in which, with due care, and under ordinary conditions, the disorganized heart is still capable of carrying on a tranquil life, but in which any high excitement, bodily or mental, frequently suffices, in the language of metaphor, to throw the organ off its balance, and rapidly induce a degree of fatal pulmonary congestion? In such cases we may fairly hope that the heart, if relieved from its pressing distress, will once again gather up strength so as further to carry on life. This is another class of cases, then, wherein I fully believe that bleeding is, now-a-days, less practised than the welfare of the patient demands.

The indication for treatment in these cases is clear; it is derived from the fact of the rapid and sudden invasion of the pulmonary function. In the heart and lungs there is suddenly accumulated more blood than these organs in their diseased conditions can duly dispose of. Weakness of the heart (as indicated possibly by the pulse) is in cases of this kind a reason for, rather than against the bleeding; the organ will gain strength as it gains freedom from the accumulation of blood in the pulmonary vessels.

I have just examined a case, which I will give as an example of the point here discussed. A man, strong, and in the prime of life, fell and struck his head; he was carried to the hospital quite sensible. In the course of twenty-four hours he became highly excited, and then his breathing rapidly grew difficult; symptoms of extreme pulmonary congestion supervened. The house-surgeon would bleed the patient, but waited for the surgeon, such is the dread of bloodletting in these times. When the surgeon arrived, the patient was dead. Now, in this case, I found the brain and its contents perfectly sound in appearance, with the exception of one red congested spot, the size of a florin, situated on the upper part of the anterior and right lobe of the brain, corresponding to the placw here the injury had been received on the scalp; the membrane was merely congested. There was assuredly no cerebral injury here to account for death; but the lungs were most highly congested with black, fluid blood, and the right side of the heart was much distended. The heart itself was small, was much covered with fat, and its muscular tissue was attenuated and pale and flabby; it did not at all correspond to the size and vigorous appearance of the man. Is it not fair in this case to look to the heart, rather than to the brain, as the immediate cause of death? May we not argue, that here the blow on the head, in so far as it produced a shock to the brain, diminished the action of this enfeebled heart, and that this, coincidently with diminished vigor of the respiratory muscles (resulting from a like cause), had permitted the blood to accumulate in the lungs, and had so caused death? Would not, probably, for this reason, bloodletting have freed the organs by assisting them and relieving them of a task to which they were unequal? It might have relieved the embarrassed heart, and have thus enabled it to resume its former state. The organ was equal to any ordinary, but not to any extraordinary occasion, and therefore it failed.

Cases of such a nature, and the principles here insisted upon, seem to me to point clearly to the fact that bleedings timely used are oftimes of great benefit to the patient, and are less frequently resorted to in diseases of the lungs and the heart, etc., than the nature of the cases demands.

In conclusion, then, I would add, that we have satisfactory reasons enough to explain how the revulsion in practice came about, quite independently of any ideal change of type in disease. There can be little doubt that one efficient cause, at all events, which wrought this modern fashion in practice--abstinence from bloodletting—may be referred to our better observation of diseases, of their progress, and of the effects of remedies upon them. Men began carefully to record the results of their experience in modern clinical form, while still pursuing the venesecting practice of their forefathers. The facts accumulated, and the fatal tale they seemed to reveal at length excited the attention of thoughtful observers, and they became suddenly frightened at its use. Thirty years ago, while still pursuing the ancient practice directed at phlegmasiæ, Andral was struck by the constant failures he had to record. Some misgivings must have come across his mind, we may suppose, when he wrote—"Nous trouverons dans cet ouvrage de bien fréquents examples de phlégmasies qui, attaquées dès leur débût ou pendant leur cours par d'abondantes saignées n'en continuent moins leur mache, soit qu'elles doivent se terminer por la santé ou par la mort. *Il y a, je crois, très peu de cas dans lesquels une maladie puisse etre ainsi enlevée tout-a-coup par une émission sanguine.*" And no one can, I fancy, carefully read these recorded experiences and practices of the past generation, and compare them with the experience of modern days, without feeling that the doctrine which guided the hand of the physician in the treatment of inflammations frequently guided him to the most fatal conclusions. Andral's misgivings did not at those times affect the practice of the clinique whose history he records. Whoever reads—and who does not ?—the truthful pictures of his pen. cannot fail, now-a-days, to be struck by the persevering energy in bloodletting there related, and the mortality in disease such as we see not happen now. The worse the patient grew, the more was he deprived of blood; and the more he lost of blood the worse he became, until death decided the struggle.

The lessons taught by these minutely-detailed and authentic histories were not taught in vain. Physicians began to doubt the correctness of their method, and then to change it; and, as usual in such cases, frightened inordinately, they rushed into another extreme of practice. An ill interpretation, also, of certain of the facts regarding vital processes, their modes and courses, revealed by modern science, may have assisted, perhaps, in strengthening those false fears.

With regard to the practice of men of earlier date, of the Cullens and the Gregories, which has been so confidently referred to as proof of the high efficacy of such large bleedings, this may be fairly said. We have little more than their own assertion to prove the excellence of their practice; they have, unlike the authors just referred to, left no authentic clinical records by which posterity may judge their general mode of practice, and the value of their diagnosis, in any large number of cases. We may, I think, fairly assume that they derived their general rule of practice from a very partial observation of the remedy in a few particular cases. It is interesting, also, to observe that there is scarcely one of those men (who have been appealed to as authorities for such bleeding) who has not thought it right, incidentally, to warn his readers of the ill consequences which he had seen result from ill-judged bleedings in particular instances.

We possess now-a-days, indeed, proofs of the ill effects of large and continued bleedings—recorded proofs, in M. Buillaud's writings, for example. And we may, by inference, guess how it fared with patients treated like his in other days, and of whose maladies we have no recorded history. The long delayed convalescence, the ruined constitution, the kindled-up latent morbid diathesis—these were consequences which came too late in the day to be noticed as results of the practice; but who can doubt that they did come as logical sequelæ to those enormous and much lauded bloodlettings? The good which the physician did, or seemed to do, had in this case, at all events, lived after him; the evil was, happily, interred with the bones of his patient.

Clarges street, Piccadilly, Dec. 1857.

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EDITORIAL AND MISCELLANEOUS.

Second Annual Commencement of the New Orleans School of Medicine.—The second annual Commencement of the new Medical School in this city has just taken place, (March 31st) and under the most flattering circumstances. We have previously announced the fact that the whole class for the session of 1857–58, just closed, is one hundred and twenty-six; of these, thirty-three have been granted the degree of M. D., and we can say, without flattery, that they are fully deserving of the high honor conferred on them. Never have we seen a more diligent set of young gentlemen in attendance on lectures; and with no small degree of pride do we revert to the fact, clearly demonstrated to us as examiner in one branch, of their having, in the end, given the fullest evidence of their close attention to our teachings, as well as their accurate conception of the principles inculcated. We feel that we have not worked in vain, and that our graduates will go forth prepared to do good in the communities of their choice, as well as honor to the institution whose portals they are now leaving. Nor, while we express so great satisfaction with our pupils, do we believe that they leave us with feelings of a different nature. Universal satisfaction with their teachers has been the expression of the class, and we feel that we are indeed sending forth so many staunch friends, who will work for their Alma Mater with all the zeal imaginable. May Heaven bless them all!

LIST OF GRADUATES IN THE NEW ORLEANS SCHOOL OF MEDICINE, SESSION 1857–58.

J. F. Glass, Georgia;
Thomas Haughey, Alabama;
Charles L. Evans, Mississippi;
William Sandel, jr., Louisiana;
A. J. Thompson, N. Carolina;
J. F. Grall, Germany;
Thomas H. Powell, Tennessee;
James E. Keator, Louisiana;
Sandford Perry, Louisiana;
Mark Andrews, Mississippi,
D. W. Wilkinson, Mississippi;
N. R. Payne, Mississippi;
J. W. Chandler, Louisiana;
M. P. Quinn, Louisiana;
E. P. Stubblefield, Louisiana;
W. C. Hicks, Mississippi;
J. M. Scaife, Louisiana.
P. J. Gibert, Cuba;
William P. Smith, Alabama;
S. L. Singletary, Louisiana;
E. T. Gazley, Texas;
W. R. Capehart, N. Carolina;
Richard S. Boyd, Tennessee;
W. A. Stanley, Tennessee;
A. A. Carruth, Louisiana;
J. F. Allen, Tennessee;
R. G. Sanders, California;
J. W. Fitzpatrick, Alabama;
J. W. Tottenham, Mississippi;
G. W. Pearce, Mississippi;
S. Wallace, Arkansas;
W. S. Rogers, Texas;
J. T. Jenkins, Alabama;
Total—33.

Prize Examinations in the New Orleans School of Medicine.—In order to stimulate to exertion, and to arouse a spirit of honorable rivalry, the Chairs of Chemistry and Obstetrics offered prizes (a silver cup and a complete set of obstetric instruments) to the gentleman who should stand the best written examination in these branches. We are proud to say that the result of the examinations, which were close and searching, has been in the highest degree satisfactory. All those who entered the lists did well, but, of course, there must be winners in the race. The Chemical prize cup was awarded to Mr. Sanford Perry, of the State of Louisiana; and the Obstetric prize to Mr. J. F. Grall, of Germany.

THE AMERICAN MEDICAL ASSOCIATION AND THE MEDICAL SCHOOLS.—As the time draws near for the meeting of the Association at Washington City, the question is being more and more freely asked—"Will the Association, can the Association, prevail on the Schools to modify their system of instruction?" We sincerely hope that all will meet this spring with an earnest will to make mutual concessions, which shall redound to the public advantage. We trust the Schools will be willing to commence a wholesome system of reform, and we trust that the anti-school men will not be too ultra in their reformatory recommendations. All who have ever read this journal know full well that we are the warm advocates of the most radical system of reform; indeed, we stand before the world as a part of an institution which has attempted radical reform and has thus far succeeded in the effort; but we are not disposed to be unreasonable. But, in the event of the failure of the Association and the Schools to come to terms, let us suggest the idea of a convention of the Schools themselves. We make the offer. Who says yes? Much good can surely be done.

PROFESSORIAL.—Late numbers of the Southern Medical and Surgical Journal and Atlanta Medical and Surgical Journal announce the fact that Professor Alexander Means has resigned the chair of Chemistry, in the Medical College of Georgia; and on this subject the Atlanta Journal remarks: "From the above announcement it will be perceived that the *Atlanta Medical College* is now in *exclusive* possession of the services of the eminent gentleman referred to."

Professor J. C. Nott announces, in the late number of the New Orleans Medical and Surgical Journal, his resignation of the chair of Anatomy in the Medical Department of the University of Louisiana.

MEDICAL JOURNALS.—Henceforth the Southern Journal of Medical and Physical Sciences will be conducted by its present able editor under the title of the East Tennessee Medical Times.

The Peninsular Journal and the Medical Independent have been merged in one, under the title of the Chicago Medical Journal.

Medical Students at Home and Abroad.—As we procure reliable data we shall continue to publish a list of Students for the term 1857–58:

London	1,050
Paris	1,027
Dublin	573
Medical College of South Carolina	216
St. Louis Medical College	121
New Orleans School of Medicine	126
Jefferson Medical College, Philadelphia	501

Contribution to the Museum of the New Orleans School of Medicine.—To Dr. M. Schuppert, of this city, the Faculty are indebted for two beautiful specimens of incomplete rectum, taken from newly-born children. The history of the cases will be found in this number.

Index to Volume IV.—We beg leave to call attention to the index to the past volume of the Gazette. Also to the title page for binding with same volume. They should have been furnished in the last number, but a press of engagements prevented.

To our Subscribers.—We beg leave to return our sincere thanks to those of our subscribers who have paid up all arrearages, and especially to those who have paid in *advance* for our 5th volume. And here we beg to call attention to a few important points in connection with this matter of paying, trusting that all interested will carefully note what we say:

1st. In our prospectus in the back of the Gazette, we have requested that all communications, of every description, shall be directed to "D. Warren Brickell, M. D., 165 Carondelet street, New Orleans." Instead of following this direction, our subscribers continue to direct to the "New Orleans Medical News and Hospital Gazette," and to the "editors" of the same. To do this is to direct the attention of all who handle such letters to the same as probably containing money, and the result is, trouble between us and our subscribers—they declaring that they have sent us money, and even sending us postmaster's certificates to the effect that they saw the money put in the letter; we, finding ourselves obliged to declare that we have never received the same, certificates of postmasters to the contrary notwithstanding. Some of our friends have even gone so far as to hand the postmasters their money and ask them to send it. Of course they say they sent it; but we have never received it. And is it not a curious *coincidence*, at least, that *every dollar* thus carefully mailed to us, has failed to come to hand, while not a dollar, directed as we have requested, has failed? We sincerely hope our friends will direct their communications as we suggest in future, and, whenever feasible, send us drafts instead of bank bills.

2d. We will take it as an especial favor if all who fail to receive our journal regularly would notify us promptly. It is really provoking, after we have been sending a journal to a doctor regularly every month for one, two or three years, to be told at last, "the journal has reached me so irregularly that it has been of no use, and I cannot pay for it." Now, we mail our numbers as regularly as clock work, and all failures must be attributable to the country mails. But here, again, another interesting and provoking *coincidence* arises. No matter how "irregularly" the journal has been reaching the subscriber, the number containing the bill for arrearages *never fails; the mail contractor could not lose it if he were to try.*

3d. Do notify us promptly of removals from one location to another. We have subscribers who seem to think us possessed of omniscience; but certain it is, we cannot be reasonably expected to change any man's address unless directed so to do.

4th. Can't all our subscribers pay up and give us a fair opportunity to show them what we can do as journalists? Not for our sakes, but for your own, gentlemen, pay up. Cast your bread upon the waters, and we will send it back to you promptly and cheerfully.

BOOKS AND PAMPHLETS RECEIVED.

"A Report on Diseases of the Cervix Uteri; read before the Medical Society, of the State of Georgia, at the Annual Meeting in Augusta, April, 1857. With Additional Notes. By JOSEPH A. EVE, M. D., Professor of Obstetrics and Diseases of Women and Children in the Medical College of Georgia, 1858." A very interesting and highly practical paper, which we would be pleased to notice more at length, but for want of space.

"Report of the Pennsylvania Hospital for the Insane, for the year 1857. By THOMAS S. KIRKBRIDE, M. D., Physician to the Institution. Published by order of the Board of Managers: Philadelphia, 1857." Like its predecessors, the best annual report issued from institutions of this kind in our country.

"Review of the Annual Report of the Board of Health to the Legislature of the State of Louisiana, January, 1858; being a Memorial addressed to both Houses of the Assembly, now in session at Baton Rouge. By Dr. ARMAND MERCIER, of New Orleans. Reprinted from the New Orleans Medical and Surgical Journal, 1858." From the author.

LIST OF PAYMENTS TO APRIL 1, 1857.—Drs. S. S. Simmons, $5, 1858; C. Johnston, $5, 1856; C. H. Stone, $5, 1858; D. L. Blackman, $5, 1858; C. C. Meredith, $5, 1857; W. Berry, $10, 1856-57; G. W. Lane, $5, 1858; S. C. Farrar, $5, 1858; Robert Fenner, $15, 1856–'57–'58; W. Harper, $5, 1858; J. Kleinpeter, on account, $10; S. H. Cheek, $5, 1858; W. R. Brandon, $5, 1858; S. S. Murphy, $10, 1857–'58; Wm. Johnson, $5, 1857; J. Fennimore, $5, 1858; W. S. Mixson, $5, 1857; H. C. McCall, $10, 1857–'58; H. J. Snow, $5, 1858; L. Estorge, $10, 1856–57; W. O. Barker, $5; 1858; Thos. Haughey, $5, 1858.

EXCERPTA.

ASPHYXIA FROM IMPERFECT COMBUSTION OF GAS.—[*Committee's Report of the Case, read before the Boston Society for Medical Improvement, February 8th,* 1858, *and communicated for the Boston Medical and Surgical Journal.*]—The Committee appointed January 25th, to investigate the circumstances of the recent fatal accident, attributed to a gas stove, presented the following report of the case:

Mr. J. H. B., æt. forty-eight, resided in Roxbury. For fifteen years he has suffered from severe dyspeptic symptoms, recurring more or less frequently, and occasionally attended with great distress and pain. Latterly the paroxysms had come on every three or four weeks, commencing with great pressure in the epigastric region, which was soon followed by severe pain. These attacks were always an hour or two after a meal. He is reported to have been at times rather imprudent in his eating.

Generally his bowels were in a loose condition, with now and then a passage of some undigested food. His nights were frequently, from the above causes, sleepless, restless and painful, so much so that he has been known to have often started from his house, at one or two o'clock in the morning, and walked into Boston as far as State street and returned. In connection with this, it should be mentioned that, for the above reasons, he was in the habit of lying on the outside of the bed without taking off any of his clothes.

He appeared as well as usual the day before his death. On Sunday night, January 24th, he probably had one of these dyspeptic paroxysms, and did not undress himself, as only the outside of the bed was tumbled. His sleeping room was below the one where he was found. It is not known when he went up stairs, nor how long the gas stove had been burning. This stove, known as Davis' Gas Stove, was purchased about six weeks previous to his death. As it was not in his sleeping chamber, he was not in the habit of going much into this room, except to take a bath in a small room leading from it. It is not known how long he had remained there at any one time with the gas stove in operation. The gas was usually lighted when he took a bath. So far as could be ascertained, he had never made any particular complaint against the stove, either in regard to any smell or effect upon his general feelings, though the domestic said that she had noticed at times a peculiar smell in the entry, and heard him complain lately more than before of suffering from headache.

He was seen by a domestic in an opposite house, on Monday, at five A. M., standing, with his dressing-gown on, at the window near the bath room. Her attention was directed there, on account of her seeing a bright light in the room at that early hour. No one saw him alive afterward, so far as can be ascertained.

At 8 A. M., his nephew, attracted by a stong smell in the entry, went to the door, and on opening it saw his uncle in a sitting posture on the lounge, not reclining, with the side of his head resting on his shoulder. Seeing him so pale, he supposed the case one of fainting, and carried him immediately to the window and poured water upon his face. But he found that he was dead. On entering the room, the nephew stated that it was full of a bluish mist and a strong acid odor. Both gas-lights were burning, as also the stove burner. There was a circular red-hot band about the stove. He was obliged to leave the room as quickly as he could, from the effect upon his head, eyes and breathing.

Dr. Windship was immediately sent for, and Mr. B. was placed in another room. Dr. W. supposed he had been dead for some time.

Post-mortem Appearances.—There was a general pallor of the surface of the body. The countenance was pale and tranquil. There was a pretty firm general cadaveric rigidity of the body. On opening the chest, the lungs were found perfectly collapsed, with two or three old adhesions at the apex of each lung. The color was similar in each, consisting of alternate transverse bands of an almost scarlet redness, and black pigment spots of the size of a pea. This was uniform over their whole surface. On pressure, there was fine crepitation, and as much as the thin, collapsed condition of the lung would admit. Every portion had a natural, soft, yielding feel, and there was no sign of any venous congestion, not even the usual *post-mortem* change in the dependent portions. The pericardium contained about an ounce of thin, yellowish liquid. The heart was perhaps smaller than natural, hard and firm to the touch, as if in a strongly contracted state. On removing it, the blood that escaped was fluid, dark colored, and without any coagula. The walls of the left ventricle were of a color difficult to describe, being something between a cherry red and scarlet. The valves were perfectly natural.

An incision being made in different parts of each lung, there escaped a fine frothy, thin liquid, in a small quantity, the color of which on the hand was different from that of usual venous or arterial blood, and resembling a sealing-wax redness. Very little dark blood was seen in a large vein that was divided. The substance of the lung, throughout the divided portions, was of this sealing-wax redness, as was likewise the mucous membrane of the bronchi.

The whole of the interior of the upper half of the intestinal canal and of the stomach, except a small dark spot of cadaveric softening near the cardiac orifice, was in a highly injected condition, the color of which was of a bright sealing-wax redness. The blood pressed from any part of the cut surfaces was of neither the usual arterial or venous color, but of a peculiar shade of red, not pink nor scarlet, but decidedly more like sealing-wax, and giving the appearance almost as if painted. This color gradually became fainter in the lower half of the intestines, and was nearly absent at the commencement of the large intestine. The cardiac and pyloric orifices were both considerably contracted.

The stomach was about two-thirds full. Throughout the stomach and intestines there was not the slightest appearance of any ulceration. In some spots Peyer's patches were seen to be prominent, but not ulcerated. The left kidney was much longer than the right, and larger than natural. Its substance was about an inch and a half in thickness, and of the same color and consistency as the walls of the heart. The bladder was full of urine. The vessels of the surface and substance of the brain contained much less blood than usual. There was not the slightest appearance of any congestion there, nor in the vertebral canal, as far as could be seen. The lateral ventricles had about a drachm of fluid in each.

The above sealing-wax color was very striking and peculiar. As far as can be ascertained, no parallel case has occurred; but in those most resembling it, as in the deaths from the fumes of burning charcoal, though the appearances were in some respects similar, they were not alike in all cases. The anatomical characters alone furnish no positive data upon which to base an opinion of the cause of death.

When the committee visited the house of Mr. B., the gas stove had been lighted about two hours and a half previous to their arrival. A distinct odor was noticed in the entry leading to the room in which the stove is placed; and, on entering, the room was found filled with a bluish vapor, of a suffocating odor, and highly irritating to the throat when inspired. The odor was that of the mixed products resulting from the imperfect combustion of coal gas. About twenty cubic feet of gas were found to be consumed per hour. The room is about thirteen feet square, with a rather low ceiling. The fire-place was closed up, except an opening for a stove funnel, also covered.

The sole products of the complete combustion of pure coal gas are gaseous carbonic acid and the vapor of water. Whether the gas is burned for illumination, or mixed with atmospheric air previous to its combustion, as when used to furnish heat, the products of perfect combustion are the same, and no odor is evolved. Incomplete combustion furnishes entirely different results. Besides water and carbonic acid, there are evolved carbonic oxide gas, vapors of aldehyde and formic acid, and probably other substances. Aldehyde and formic acid have powerful odors, and are irritating to the respiratory organs. Carbonic oxide is known to be an active narcotic poison, and is now regarded as the chief cause of the deadly effects of burning charcoal. The products of imperfect combustion are far more deleterious than carbonic acid gas, or unburned coal gas. Carbonic oxide especially is a remarkably insidious poison, and is dangerous even when mixed with a large proportion of atmospheric air.

The committee are of opinion that an adequate and sufficient cause of the fatal result in this case is found in the deleterious products resulting from the imperfect combustion of coal gas. As they were appointed to investigate the case before them in its medical bearings only, they have not carried their examination farther, nor inquired into the causes of the defective operation of the gas stove.

JOHN BACON,
CALVIN ELLIS, } *Committee.*
GEO. H. GAY,

The following account of an experiment upon a cat, placed in a room with one of the gas stoves above referred to, was read at a subsequent meeting by Dr. Ellis:

The cat was placed in an iron-grated cage, elevated two and a half feet from the floor, the room being eight feet square and ten feet high. The thermometer indicated 90° when taken out. The first thing noticed was a snapping of the eyes, then a crying, (as is frequently heard in the night from cats,) this becoming, in fifteen or twenty minutes, pretty loud. In a short time this crying stopped; the mouth moved, but without any noise. She sneezed fifteen or twenty times, and rubbed the nose and face with her paw; afterward tried to bite the iron grating of the cage. There was a flow of watery fluid from the mouth, but without any frothing. The mouth was opon most of the time. While lying down she would try to get up on her back legs, and would fall over immediately. In thirty minutes there was a tremulousness and throwing back of the head. The respiration became long and stertorous. Convulsive movements came on about the epigastrium, which increased over the body generally, and in forty-eight minutes the animal was dead.

A CASE OF EMPYEMA, TREATED BY INJECTIONS OF WATER INTO THE CAVITY OF THE PLEURA.—*Reported by Sternes De Witt, M. D., of Baker county, Ga.*—When I first saw the case, the patient, which was a girl, aged sixteen, had been sick for four weeks—was very feeble and much reduced in flesh; pulse quick and weak; had some fever; was very restless, could lay no other way than on her left side; breath very short and quick. According to the best information that I could draw from her parents, she was taken with a chill after being on the road on a journey five or six days. She had been confined to her bed for four weeks—had been laboring under febrile symptoms, and had cough, which grew worse; had spit some blood. She had felt considerable pain in her left side, which was in the wall of her chest, rather than deep-seated. After two days, she had in that locality considerable *swelling*, which, in two or three days, had increased to a remarkable extent, reaching as far as the sternum on one side, and filling completely the cellular tissue, around to the spinal column, on the other side. This, however, did not much alarm the parents. In the lapse of about four days more there had formed over the false ribs, an abscess about the size of the top of an ordinary coffee-cup, having the appearance of an enormous boil. This was the state of things when I first saw the patient.

On examination, I found that the upper swelling was very hard; and at about the third intercostal space the motion of the heart could be seen at a distance of several feet—the movement communicated to the chest, extending over a space of three or four square inches. My attention was next called to the tumor, lower down, which, as I have said, was on the false ribs. This felt hard, did not fluctuate, was very red, with little tumefaction around it, except in the upward direction; here I could hardly be satisfied that there was any connection between the two, as the swelling was diffused from the axilla half way round the body in either direction, and nearly down to the false ribs, on which was situated the pointed and more determined tumefaction.

Being inexperienced, and having never seen a similar affection before, I told the parents I would see the case on the next day, having determined to operate on the lower swelling, as I had no doubt it contained pus; thinking that it could have no connection with the difficulty above, and suspecting the upper swelling might, possibly, be an aneurism, of which I knew nothing by experience.

The abscess, which I had determinded to open, had by this time got considerably softer and less inflamed. I made an incision about three-fourths of an inch long, down to the pus, which flowed freely for several minutes. In the meantime, I had noticed that the upper tumor had wonderfully abated, and the breathing had become easier. The matter was quite thin, and darker colored than ordinary pus; this continued to run from this opening until the swelling above was almost entirely gone. I inquired into the state of the patient's bowels, and found that she had not had a passage for several days; I gave her a gentle cathartic, and directed a poultice, which had been used hot, to be applied cold.

I returned on the second day after; the matter had ceased to be discharged, and the swelling had increased above, almost as much as at first. On examining the lungs by auscultation, I found that the patient did not breathe at all in the left side, and that the other lung was laboring under great compression—nearly as much so as when I first saw the case; pulse feeble, with some fever.

The next day, I saw the patient; the swelling had become as large as at first, and the breathing very difficult; the pulsation in the tumor as apparent as before. I immediately decided to let out the matter; which I did, by making an opening in the centre of this pulsating tumor, three-fourths of an inch long. The matter flowed in a large stream with such force as to jet from the body several inches; this time it did not discharge less than *two quarts* of matter in five minutes, and continued to discharge slowly for several days. In the meantime, I bandaged the body and placed a plaster over the opening to prevent the ingress of air, removing the same every day, with the patient reposing on her knees and breast, thereby aiding the discharge of matter, which was materially facilitated by the introduction of a small tube through the opening, to expedite the escape of the matter, which continued to discharge one or two ounces per day for several days, which discharge did not abate as rapidly as I had anticipated.

I prescribed iron by hydrogen, three grains, three times per day. The patient improved, grew stronger, ate heartily, but still the discharge did not abate; the lung did not expand on the left side, breathing confined entirely to the right side; left side seemed to lack its fullness, and the spine was flexed laterally, the concavity on the affected side. I now tried injection, first preparing two quarts of tepid spring water, and with Matson's patent injecting instrument, introduced two or three inches into the opening—threw the entire quantity of water prepared into the chest, it escaping around the nozzle of the instrument, after the cavity was full; thus effectually washing out the part. I afterwards bandaged the body as tight as the patient could tolerate it, applying an adhesive plaster to the opening, and permanently closing it. After this, loosening the bandage daily.

On this treatment, the lung began to expand, which continued until the function of the lung was entirely restored. The opening cicatrized, and the patient was discharged, apparently well.

[*Southern Medical and Surgical Journal.*

MEDICAL ETHICS AND THE AMERICAN MEDICAL GAZETTE.—*Mr. Editor:*—May I ask the insertion of a word or two in answer to some strictures which have been made upon my paper on Medical Ethics, published in the last number of your journal?

The able editor of the New York "American Medical Gazette" charge my opinions with "apparent and palpable inconsistency." I think they are not fairly liable to this imputation. They refer to several matters entirely unlike, and occupying ground widely apart. I consider a moral offence as a very different affair from an intellectual error or absurdity; and while I would stamp the former with an indellible denunciation, I might be willing to overlook or excuse the latter. The cases alluded to in my brief essay are so dissimilar in kind, not in degree merely, that the language employed is not in the same sense applicable to them promiscuously. In morals I know, and am bound to know, what is right and what is wrong: there is an infallible rule to which I may appeal. "*Video meliora proboque.*" Even if I am forced, with the ancient, to make the remorseful confession, "*deteriora sequor,*" I must still pronounce upon all cases of this nature which come before me, and such decision does not imply either a pharisaical arrogance or a severe and relentless temper of mind.

The editor, intimating the latter condition as requiring correction, says, that he knows of no "unpardonable sin." Such an offence is distinctly enough affirmed in sacred things. In human jurisprudence there are many such. We hang men—we imprison them—we fine them every day for unpardonable offences. So much for ethics. As to intellectual misconduct, on the other hand, having no right to judge or condemn for whatever differences of belief or doctrine, so I have no right even to speak of pardoning.

But to reply categorically and in detail. 1. An offender may have the punishment which follows his wrong-doing remitted, and thus, and in this sense and degree only he may be pardoned, while we still frown upon his criminal course. If we relieve him, upon his repentance and reformation, from the special penalty he has incurred, we are not called upon specially to honor or elevate him. Nay, it is clear that he must not be rewarded for his penitence. With whatever kindly feelings a father may look upon a prodigal son, all others would esteem it a dangerous precedent to purchase and pay for his reformation, and buy him back to virtue. Yet this seems to me precisely what the editor has aided to do, and maintains to have been rightly done in the present controverted instance. A distinguished appointment to a profitable place in one of the largast and most respectable hospitals in the United States is the *reward of merit* with which an erring brother has been welcomed back to the regular ranks of a profession which had been dishonored by his delinquency.

2. I have suggested that the appointment of a Homœopath as a Professor in a medical college did not make it incumbent on the members of a faculty, into which he had been intruded by the trustees, to resign. The case is by no means a new one. In the time-honored school of Edinburgh, where taught Cullen and the Munroes, the Gregories, and Allison, we find Prof. Henderson, a champion of homœopathy, occupying a chair, with Simpson and Bennett, Laycock and Christison as his colleagues. I have never heard it suggested that there was any reason to expect or require the resignation of these elevated representatives of scientific medicine on this account. They hold their places, so far as I know, with the unanimous approbation of high-minded physicians all over the world—certainly with my hearty and respectful approval.

3. If I am right thus far, I cannot be wrong in maintaining the propriety of conjoint hospital attendance with the homœopathists, so far as it does not imply consultation, and absolutely avoids interchange of patients. Prof. Henderson does hospital duty in Edinburgh. Why should not Bennett or Christison prescribe for patients in the same institution? I repeat, that I think the service "safe and honorable." A homœopath may be sincere, truthful, earnest and morally trustworthy as I or any other man. No one is farther from neutrality than I am, and ever have been as to his doctrines and therapeutics. But a difference upon these points is not admitted by the world to be proper ground for denouncing him. Laymen, indeed, demand for him "equality and fraternity." We repel the claim, but accord him a cold toleration and a reluctant forbearance. We refuse to consult with him for abundant and sufficient reason, without any moral or social condemnation. There is between us no possible middle term or place for compromise, and every patient must select one of the two, to the absosolute rejection and exclusion of the other.

4. I have protested "against the readiness with which physicians sometimes lend themselves in aid of malicious or envious prosecutions for malpractice." I must have expressed myself more strongly than I intended, to have conveyed to my intelligent and courteous commentator the idea that I would object, as he has phrased it "againt all public censures upon medical opinions given before courts and juries." Surely my language will not bear out this construction. I admit fully the obligation to state, when called upon an honest disagreement, a plain, downright dissent, a marked expression of disapproval. I insist, at the same time, upon a nice and considerate delicacy and a prudent reserve. My words are, "I will not deny the duty of interference in certain supposable cases which I must leave to every man's own conscience and sentiment of honor; but I would inculcate the absolute propriety of the greatest delicacy and reserve in the statement of opinions; the positive demand that every one should be allowed 'the advantage of a doubt,' if a doubt possibly exists;—and how few cases are free from that possibility! But I do protest against the outcry made so often by those who, having no opportunity for fair and minute examination, yet join in clamor and denunciation; which, whatever may be its effect upon the individual case, cannot fail to be injurious to the character and standing of our profession." This is what I said, and what I am prepared to maintain; and in this position and protest I am confident that I shall be supported by the great body of my professional brethren. I do not intend, nor does my language imply any censure whatever upon such physicians or professors, as thinking and feeling differently, may prefer to shrink from contact or association, in all forms and modes, with Homœopathists or other schismatics. What I have laid down is simply that there is no ethical or even conventional necessity or obligation upon them to do so, and that both they and I are free to choose in the premises.

On each of the above questions, then, I conceive that the views I have taken are tenable and correct, and therefore, cannot involve any inconsistency. Even as to the phrases in which they are conveyed, I am not aware that I could make any change for the better. The word 'conjoint' applied to hospital service with Homœopathists may perhaps be liable to be misunderstood. Yet it seems clearly enough announced that I did not propose to admit any association with them, either in debate or action, but only in place or duty; and this an enforced association, for which we are in no way responsible; submission to which I regarded as a lesser evil than evasion of the contest or comparision thrust upon us by ordained and competent authority.

I am strengthened in this opinion, in addition tô the rational arguments that may be alleged in its favor, by the example of our honored representatives in the school of "the modern Athens," the metropolis of Scotland.

Very respectfully, your obedient serv't,

SAM'L HENRY DICKSON.

[*Charleston Medical Journal and Review.*

FOREIGN CORRESPONDENCE.—The following interesting letter, from Prague, has been placed at our disposal by Dr. I. S. Dodge, of this city, to whom it was directed by his friend:

PRAGUE, September, 1857.

MY DEAR DOCTOR:—The fall vacation having begun at Vienna, I took the cars for Prague, in order to spend from six to eight weeks at the Bohemian capital, in the pursuance of obstetrical studies; for, as the Austrian Medical Institutions justly deserve the reputation of being prominently practical, I expected to find this especially true of the University of Prague, with respect to her obstetrical department, and I am happy to say that I was satisfied beyond expectation.

There is, perhaps, no branch in medical science, the practical study of which will consume so much time as that of obstetrics, and fortunately I had now no time to lose on other branches during the vacation.

Arrived at Prague, in the beautiful month of July, I first tried my best and succeeded in securing lodgings, and, meeting the next morning with some medical friends of Würzburg, we ascended the high hill of the "Winneberg," crowned on the top with the stately buildings of the obstetrical institution. Looking upwards from below the hill, you would imagine to see a finely situated castle or a convent.

It was early in the morning, not yet seven o'clock, when we arrived on top of the hill, and, looking down once more, we were reflecting to what degree such exercise in climbing might tend to quicken labor in pregnant women, and procure a favorable (?) position to the child; for the majority of these contributors to the human race are unable to pay for a carriage. On the hill there was quite a number, I think about thirty, of medical men, that were going to attend the clinic, and the Professor having arrived, I rushed with the whole company into the ward where the delivery of females takes place. In this ward there are twelve beds, six of which are usually occupied by the females, who are actually exerting themselves in the duty of increasing the population of the Austrian Empire.

Professor Seiffert, a gentleman who stands decidedly on his own feet, examines the inmate of the first bed, while the rest look on. At first he makes the external examination with his hanls on the abdomen of the woman, who is quite exposed to the eyes of bystanders. If the pelvis present any abnormal dimension, he takes Stein's pelvimeter to ascertain the diameters, and measures the circumference of the pelvis; after this he auscultates for the fœtal pulse, then he examines per vaginam, after which the attending sons of Æsculapius exercise the same manœuvering. There were usually about thirty students present, every one anxious to make a thorough examination, and thus the poor female had to submit to a great deal of uneasiness; for it sometimes happened, that *with their finger* some of these young gentlemen had not so much experience in exploring these regions, which was evident by the shrieks of the female, and some streaks of blood on the finger when it was withdrawn from the vagina. The examination having been made, and some questions answered by the patient, Professor Seiffert, with a pelvis in one hand and a leathern baby in the other, proceeded in a very instructive manner to demonstrate the case, illustrating this particular presentation, as well as also modifications of it in other cases. This discourse was always calculated to transfer knowledge to his auditors; only, once in a while, he took occasion to give his opinion of the foolish notions which the gentlemen at Vienna and other places entertained on the subject. He then went to the next bed, where in a similar manner the peculiarities of that case were put into the proper light. His discourse was concise and to the point, his attention to the students' examination inexhaustible. If a female is going to be delivered during the clinic hours, and Nature finds her powers insufficient to terminate the process favorably, the forceps are applied by one of the students, while Professor Seiffert assists him where he is wanting in skill. Not unfrequently the forceps were made use of, where it certainly was not always necessary, probably for two reasons: to give every student a chance to try his skill in the application of the forceps, and, perhaps, to teach the infant that important lesson at the beginning, how necessary it is to yield a little to the outside pressure, if it wishes to glide pleasantly into and through this world.

After the close of the clinic, which lasts every morning for two hours, from seven to nine o'clock, Professor Seiffert pays a visit to the different wards of the house, where the females are placed after their delivery, and we had thus an opportunity to witness some anomalies consequent upon the period of parturition. There were always some mild cases of puerperal fever. Professor Seiffert has the custom to step from one bed to another, and counting the pulse of the patient, without asking a great many questions of her. If the pulse was not frequent, he would almost systematically pronounce the word "healthy," without making many further inquiries, and if the pulse was found to be increased in frequency, he was in the habit of ordering a few doses of aqua Viennensis, essentially an infusion of senna, and he insisted on it with much fervor that it was best in such cases to move the bowels, and not to give opium. Students and physicians not belonging to the institution are allowed to accompany the Professor only into those wards which are on the second floor, for in upper regions there are private apartments, visited only by the Professor himself or by his assistants.

After having attended the clinic for a few days, I was fully convinced that I had come to the proper place for the cultivation of this branch in medical science, and procured tickets for the clinic, for the "Inspections course," and for the diseases of females, the clinic of which was held in the female department of the general hospital—"Allgemeines Krankenhaus." These tickets are good only for eight weeks, after which time they must be renewed and paid again, if the holder wishes to continue his visits longer.

Now permit me to tell you of the other arrangements in this institution, and let me know if there is to be found the like in any other city. By taking the ticket for the inspection course I was entitled to the right—

1. Of entering the Lying-in Hospital at any time, day or night, to inspect the females—excluding the attending midwives—occupying the delivery ward;

2. Of staying in the "inspector's" room every twenty-four hours, when my turn would come;

3. Of taking my lodging in the "Kaserne," if I could secure a bed.

The inspector's room was adjoining the delivery ward, and furnished with four large sofas, besides a table and chairs. It was destined to be the abode of four "inspectors," i. e. four students taken by the order of the list, for the time of twenty-four hours. By looking through the window you would see a long one-story building, running parallel to the obstetrical hospital, and separated from it by a narrow passage; this long building was termed the "Kaserne," which name is usually applied to the barracks of soldiers. In the inspector's room is a handle fixed to a wire, which passes through the wall over the road into the Kaserne, and there it is attached to a bell. The Kaserne consists of six rooms, each containing four beds furnished by the institution; these beds are let to students without any additional charge, and the bell is rung by the midwife on service, whenever the labor of a female seems to bring her delivery near. Now, sir, this is an excellent arrangement.

I tried every means to become an inhabitant of the Kaserne. I spoke to Professor Seiffert about it, and tried to engage his assistant in my favor, but without any other result than hope for the future. When a bed became vacant there were more than half-a-dozen of applicants, who claimed priority, and it seemed to me as though the native Austrians were a little favored. It is true I had a fine room in town, but when I came on the hill in the morning I heard what had taken place during the night: five births, all natural, or one breech presentation, or the forceps applied in two cases, or twins, etc. Then I felt deeply what losses I had sustained, and patiently waited after the clinic for six hours in the ward, with the hope something interesting might come to make good my losses; but no--the little babies prefer to see first rather the light of an oil lamp than to view the rosy light of the sky. What was to be done? To sleep in town and let the most interesting births pass by during the night in the hospital, noticed by others, would certainly not give me much profit; to sit all the night long by the bed of the female, who would perhaps not be delivered until early in the morning, was rather too hard a task to perform for any length of time; to lay down on one of the beds not occupied by two mortal beings—one enclosed within the other—and to enjoy a pleasant dream, was against the rule of the house. But still man is not at a loss, as long as he does not lose himself.

I soon made the observation that the inspector's room was almost always vacant after eleven o'clock at night, the gentlemen preferring to be either at their lodgings in town, or in their bed in the Kaserne, if they had one. What could be more reasonable for me than to pretend every evening, at eleven o'clock, that it was my duty to stay there all night? Thus the inspector's room was mine all night, and there I slept on the sofa within my clothes, after having obtained some covering material with some unnecessary fleas in it, from the adjoining ward by stealth, to protect me from taking cold. Thus I had—not regarding the presence of the little criminals, which were trying very briskly to work themselves through my epidermial cells—the best situation I could imagine, for whenever a birth took place during the night the attending midwife rushed into my room to pull the bell, in order to arouse the occupants of the Kaserne from their sweet slumber, and I, being at the seat of war in an instant, witnessed every birth, while it sometimes was terminated so soon that when my colleagues hurried up stairs into the ward the battle was already fought, and they returned, very much disappointed, to their quarters. Every morning I arose from my comfortable (?) couch before five o'clock, and felt in my limbs as stiff as though I had been put on the rack. I then went once more into the ward, and examined the females that were in the beds; after having then washed my hands only, I went down into the town to my lodgings, where I paid the proper attention to my toilet, visited then a café to take breakfast, and found myself again, at seven o'clock, in the hospital, to attend the clinic.

We had frequently an opportunity to witness the action of the forceps. They make here slight alternate tractions from one side to another; Scanzoni, of Würzburg, does the same; but in Vienna they do not change the direction toward the sides, or they do not make any part of the forceps act as a fulcrum. After the delivery of the child that of the placenta is quietly waited for, and when received it is carefully examined, to see that all its parts have been removed from the mother. A legal prosecution was instituted just at that time against a midwife, who in private practice had not removed all of the placenta, upon which puerperal symptoms set in, and the poor woman died of pyæmic fever, in the female ward of the general hospital, to which she had been removed from the city.

It is also a very laudable rule of the Lying-in Hospital, that every one is held to soap his hands after each examination, before laying hands on another patient, for instances have occurred here in former years where syphilis was transferred from one female to another by the unclean fingers of those persons who examined them.

Thus I passed two weeks, when I thought I should try once more to become a member of the community in the Kaserne. With the higher authorities I had failed to secure any success, so I descended in the scale, and prevailed upon the female who had the immediate domestic charge of the rooms to furnish me with a bed, and, promising her a good fee, I succeeded and became one of them, and was now free from nightly bloodsuckers—was always within reach of the sound of the bell, which roused us every night several times; and only imagine with what a hurry, on such occasions, everybody would try to get into his clothes, to some extent, at least, for complete buttoning was not required, as the night was dark, and the females' attention entirely engaged with their own condition. In this bustle many a one would put a boot on one foot and a slipper on the other, and thus appear in the hall before the altar of science, where Nature was the high priest, wielding her scepter with her creative power.

Often we were paid well for thus depriving ourselves of our night's rest, for we saw a good number of different presentations, some cases of twins, some of abortion, a few where the child did not survive, and none where the mother died, except the case of the midwife, of which we only saw the result of the mal-practice. It is said that more than three thousand females are delivered annually at this hospital. A careful record is kept of each birth, containing the name and age of the mother, the weight of the child, the time when labor began, and when the delivery of the child and the placenta occurred, the presentation of the child, the mode by which the delivery was terminated, whether by the efforts of Nature or by art, and finally general remarks.

The institution derives its support from funds furnished by the Austrian government. Every woman of the land that is pregnant can gain admission. If she be poor, the town or village where she comes from is under the obligation of paying a small daily tax, about twenty cents, to the institution. If she be in better circumstances with respect to money matters, or if she has a paying lover, she can obtain more comfortable accommodations, and is not to be used in the service of science, but, on the contrary, the scientific men are at her service, and she can come into the house for admission and accommodations without her name being asked, or notice being given to her relatives.

Those who have been delivered can stay until they feel quite well enough to leave. If a mother feels herself unable to support and raise her child, she is at liberty to give it up to the foundling department of the institution, in which the poor little cosmopolitan is taken care of by appointed nurses and waiting women, until it reaches the age of some years, after which time steps are taken to find out some proper family in any part of the country that will take further charge of it.

Such a child certainly deserves to be pitied, but still this arrangement is well calculated to erase those criminal acts from history where a mother is driven by despair to kill her own issue.

And now somebody might suppose that the females delivered here belong to the outcast of society. Well, they may be to some extent a part of this unhappy class, but certainly a great many of them are even morally not quite forlorn, and as for their looks a good number of them are handsome, and once in a while you will meet with one that can pass for a model of perfect natural beauty. Seeing such poor victims of a weak moment, all sensual thoughts are driven off with the reflection that this poor creature might have rendered a good husband comfortable and happy, had it been her lot to pass into life under more favorable circumstances, for almost all of them have been betrayed by some miserable scoundrel, and did not fall into such an error with the intention of trade; and strangely was I surprised to find, from conversation with them, that, although they would strengthen their mind in the future, they would still continue to love the man who had reduced them to so miserable a condition. I had frequently an opportunity, when the time flowed tediously, and I felt no disposition to study, to sit down by the bedside of such an unhappy girl, and to enter into a conversation with her on her past history, which generally was a very melancholy one, but often showed clearly that she kept still a good stock of moral sense, and in many an instance she continued in the fostered hope of the legal union with her lover at the altar.

By treating such females as outcasts, society commits a moral crime, and drives a poor creature entirely to the low degree of misery, from which no power can deliver her; whereas, the remembrance of the proverb, that every man is liable to error, would return many a girl to the rank of virtue, not to be claimed perhaps with justice by many a one who is looked upon as a pattern of moral purity. From what I have learned of these Bohemian girls, it is not the custom of respectable families to refuse their services as housemaids, provided they hold in other things a good reputation.

Besides the course in obstetrics, which stands, I believe, unequalled for its excellent arrangements, I attended a private course of Professor Seiffert's, in diseases of females, for which plenty of cases were furnished in the female department of the general hospital. All the other departments of the hospital were closed for the vacation.

Prague is provided with extensive means for medical studies; her general hospital is very large; and a fine building for pathological anatomy is just being finished. I saw the culprits of the state prison, in chains, carry the stones and other building material to the place. Until now the post-mortem examinations were made at the hospital, several being made every day. The first and much esteemed Professor of Surgery, Pitha, has been called to the same chair in the Josephinum at Viénna, where the surgeons for the Austrian army receive their medical education. Waller is the Professor on syphilis, and is regarded by the students as an excellent teacher. So much about Prague, I will write you my next letter from Berlin.

Truly your friend,

ARNOLD STROTHOTTE.

[*Cincinnati Lancet and Observer.*

St. Louis Medical Society.—*Dr. W. M. McPheeters reported to the Society a case of* Unusual Rigidity of the Perineum.—This case, said he, was interesting in many respects, especially in reference to the obstetrical procedure which was resorted to for its relief.

At 9 o'clock, A. M., he was called to a lady then in labor with the first child. Things had proceeded on slowly but regularly, and in the evening the child's head had fairly descended into the pelvic cavity; at midnight, the head reached the perineum, which it considerably distended; the pains then were so frequent and so powerful, the distension of the perineum was such that at every moment he expected to see the issue of the child's head. However, notwithstanding the energtic action of the uterus continued without intermission, no progress was perceptible. Fearing at this juncture an imminent and extensive rupture of the perineum, he had recourse to venesection to the amount of thirty ounces, to the warm bath, to fomentations, etc.; the state of nausea in which the patient was for several hours, contra-indicated the use of antimonials. In spite of all these means, the rigidity of the perineum gave no signs of yielding, and in order to deliver the patient he feared that no alternative was then left him except to lessen the child's head. However, before resorting to this extreme measure, he thought that he would try another means. It was to incise the perineum. This he did by introducing a guarded bistoury at the margin of the extended vulva and performing an incision in the perineum in a diagonal direction, sufficient in length to procure the desired opening; the opening was hardly completed, when, with strong expulsive pains, the child's head made its appearance externally, and was soon followed by the shoulders and the rest of the body. He thought this result very encouraging, and although not aware that this operation had been performed before, he would certainly have recourse to it again under the same circumstances, and would recommend it to others.

He would further remark, that the child was born alive and that the patient recovered without any untoward symptom.

Dr. Pallen said, that in his opinion, Dr. McPheeters, under the above circumstances, had adopted a most judicious course of conduct. Not only was the operation warrantable, but it had been recommended by the highest, such as Dr. Blundell. He had himself taught this procedure and applied it in similar cases for many years. Lacerations of the perineum were, said he, very much to be deplored and should be prevented with the utmost care. This laceration may extend sometimes very far, involving the spincter ani, etc. However, in a case of that description, he had observed an admirable proviso of nature, which consisted in the establishment of another internal sphincter for the anus above the seat of laceration, thus preventing the disastrous consequences of this lesion under ordinary circumstances.

Dr. Hammer, in reference to the case related by Dr. McPheeters, said, that the procedure he adopted in this instance was a regular obstetrical operation taught and practised in Germany for many years. Naegele, whom he heard lecturing on this very subject, speaks of this operation as having been of long use in Germany. Dr. Hammer added, that in order to maket he incision fulfil its object, which was the enlargement of the vulva, the incision should be sometimes rather extensive and deep. If the incision, said he, was not sufficient, the pressure of the child's head would certainly continue it and produce thus a ragged cut instead of the clean incision which the knife would perform. He would remark, however, that it is a thing of rare occurrence that the distension of the perineum be so great as to necessitate this operation. By referring to the statistics of the large hospitals, we seldom notice this operation mentioned. In Germany, forceps are generally used in such cases. Their action is to alter the form of the child's head, by elongating it and thus facilitating its passage. Moreover, lessening the rigidity of the perineal muscles by other means; among these, chloroform by inhalations or local fomentations. Kneading the perineum, he said, would also often be of benefit. Killian, of Bonn, recommends this method.

Dr. Pallen, in reply to the above remark, said, that he thought that the application of the forceps would have here increased instead of diminishing the difficulties. The forceps, said he, are compressors of the head only to a very limited degree, as shown by Baudelocque. The forceps is mainly an instrument for extraction, a *tire-tete.* Chloroform, said he, was advisable for many reasons. It does relieve spasmodic muscular action; he had tried it many times with marked success in cases where the version was indicated, and where it would have been next to impossible without the relaxing influence of this powerful anæsthetic. The kneading, spoken of, merits also, said he, commendation; he had practised many years this method, which he once learned from Dr. Linton.

Dr. McPheeters replied, that he was well convinced that in this case an application of the forceps would inevitably have caused a rupture of the perineum. He had often applied them under similar circumstances, but not when there was that degree of rigidity; he added, that fomentations and inunction of oil had been freely used; the patient was already naturally nauseated so as to preclude the use of antimony, she had been bled to a considerable amount, and the co-aptation of the head to the perineum was so intimate that the very act of introducing the forceps would have produced laceration. Furthermore, there were expulsive pains enough without the forceps. Chloroform was not administered on account of the repugnance of the patient's friends to it. Moreover, he considered that the very pressure of the child's head was the greatest relaxant that could have been devised; and all the above means, after a fair trial, proved of no avail. But after the incision made in the perineum, the child's head issued without the slightest difficulty.

Dr. Hammer considered the variety of opinion regarding the use of the forceps in these cases, as a mere matter of discrepancy between two continental schools. In England and America, craniotomy, said he, was outrageously frequent, whilst in France and Germany forceps operations were preferred to the former more bloody procedure. The Germans consider that a certain degree of disproportion between the child's head and the pelvis or the perineum may be overcome by a judicious use of the forceps. However, the disproportion is sometimes so great that it can not be overcome. Under the circumstances of Dr. McPheeter's case, the Germans would have used the forceps; there was, it is said, an impossibility of introducing the instrument during the pains, but in the absence of a pain this might have been done. It has been said that the rigidity of the perineum is principally kept up by the presence of the child's head; but the spasmodic contractions of the perineum thus induced, alternate with those of the uterus. By the action of the forceps this alteration is annulled by the continued pressure it allows to be made on the perineum with the child's head, whilst it is embraced in the blades of the forceps.

Naegele, although of a timid temperament, in other things, used the forceps liberally.

The forceps, by compressing the child's head, elongated it. This instrument did not perhaps much diminish the size of the child's head, but by altering its shape it facilitated its passage through a narrow pelvis. Did not nature proceed in the same manner in cases of disproportion between the head and the pelvis? In those cases it was to be remarked that the head was sometimes considerably elongated. In this wise did the action of the forceps imitate the operation of nature.

Dr. McPheeters admitted the possibility of this elongation. As for using the forceps in this case, he considered it was utterly impossible either during a pain or in its absence; for the fitting of the head to perineum was so close the patient complained loudly of the mere introduction of the finger, which she compared to a cutting instrument.

As to the spasmodic contractility of the perineum referred to by Dr. Hammer, it was altogether destroyed in this case, as will occur whenever the muscular fibre has been acting powerfully for a length of time.

Dr. M. M. Pallen thought that so far as this case was concerned, the delay of the passage of the child's head through the vulva was not owing in any degree to inertia of the uterus or to a want of the *vis a tergo;* this existed to a sufficient extent to have procured the birth of the child under ordinary circumstances; but here was a most rigid perineum, against which the child battered in vain—a danger of laceration was imminent as the result of natural forces alone—the forceps, he thought, would have increased the danger of this laceration. A great deal of prudence should guide the accoucheur in deciding upon the cases where the forceps would be useful. He was himself a strong advocate of the forceps under certain circumstances, although he must say that he condemed the use of the *long* forceps under any circumstances. In this, he was aware that he differed from the opinions taught by all accoucheurs even in America and especially in Germany. He was not, however, inclined to undervalue the great and valuable progress made in the art of obstetrics by the Germans; he knew full well that Naegele had done more than any man, except Baudeloque, to advance this art. But whilst he acknowledged their eminence he did not in everything adopt the teaching of that school; he endorsed the doctrines taught in London and Dublin; he was convinced from statistics, that more women recovered after craniotomy than after the use of forceps when applied above the pelvis. The mortality of the children was also very great in those cases; frequently, also, various ruptures, and also vesico or recto-vaginal fistulæ were the result of the imprudent applications of the forceps. Siebold had, said he, applied the forceps once in every seven cases of midwifery which occurred in his practice. This preference of the Germans for the forceps, was, he said, not based upon merely scientific reasons, but resulted in a great degree from peculiar religious prejudices.

Dr. Hammer, in reply, denied that the German accoucheurs were influenced by peculiar religious opinions in this matter; their adoption of the forceps, in many cases where it was not employed by the English, was based upon purely scientific grounds. In Germany, the forceps was better studied and understood in all its applications, and selected, therefore, as a very safe and useful instrument in cases where the English or American accoucheur would, without hesitation, sacrifice the child's life. In Germany, in those cases, they tried to save the child and the mother also, and they generally succeeded. In England, one of the reasons why the forceps were not used was on account of that mock modesty so peculiarly British, which did not allow of a woman to be uncovered. Acting on this false modesty they have not encouraged the establishment of lying-in hospitals to the same extent that they have done on the continent. They do not understand the forceps for want of facilities to study its applications, hence their hesitation in applying it. The Germans, who are familiar with that instrument, are more bold in its employment.

As for the action of the forceps in the case of Dr. McPheeters, he thought that the rigidity of the perineum was owing to a spasmodic contraction of the muscles of that part, comparable to the spasmodic strictures which occur sometimes in the urethra. If a bougie is introduced when one of these spasmodic strictures exists, it will impinge against it, without penetrating through it; but if the bougie be left there for a few minutes, the parts will gradually become accustomed to it, and it will at length penetrate through the stricture. The same process will occur in spasmodic contraction of the perineum. This said he, was the rationale of the action of the forceps in those cases. If the child's head be pressed for some time by the forceps on a rigid perineum, the spasmodic contraction of that part will gradually yield. As for the ruptures and vesico or recto-vaginal fistulæ, which according to Dr. Pallen, were often the result of the use of the forceps, he thought that they took place oftener when the forceps were not used, as in those cases which have been left to nature or where craniotomy was performed. Vesico-vaginal fistulæ happen mostly in the protracted cases which have been left to nature. Statistics would prove this.

In the United States, there are, said he, as many craniotomy cases as there are forceps cases in the rest of the world; because the forceps are not used at all in some cases where it could have been beneficially applied in the beginning of the labor, and craniotomy is resorted to as a last resort. He would not blame the profession here for this state of things; it originated from the want of lying-in hospitals, as he had already remarked.

Dr. Pallen said, that he would see with the greatest satisfaction the establishment of lying-in hospitals in this country, and he expressed the hope that the Biddle Lying-in Hospital, in process of construction in this city, would some day prove a source of practical instruction to students desirous of perfecting themselves in the science of obstetrics. As for himself, he was far from condemning the use of forceps at the inferior strait; he was in the frequent practice of applying it himself—and he had taught its utility for years;—he would add, that his opinion was, that it was not applied in many cases in this country where its usefulness would be incontestible if better understood, and where craniotomy was, unfortunately, performed, especially in the country, where the practitioner is often left to himself.

Dr. Smith was one day present, said he, at a lecture on obstetric operations, delivered by Dr. Murphy, of London; in that lecture, he made the startling remark, that statistics would bear him out in the assertion that death oftener followed operations of craniotomy at the superior strait than cæsarean sections.

Dr. Smith said, that as for the rigidity of the perineum giving way under the continued pressure of the child's head, as asserted by Dr. Hammer, he would rather expect to see the rigidity increased under this prolonged pressure; he would expect to see inflammation setting in under these circumstances, and a spasmodic contraction firmly established. As for the procedure employed by Dr. McPheeters, in the case he had reported, he thought it perfectly warrantable and worthy of imitation under similar circumstances.—*St. Louis Medical and Surgical Journal.*

A NEW EDITION OF THE SIAMESE TWINS.—*Recorded by William B. Ball, M. D., Chesterfield County, Va.*—I encountered a case on Thursday night last, which seems to me worthy to be recorded.

Was called to see Sarah, a slave, in her twenty-sixth year, and third confinement—once of a healthy child, now two years old, next a miscarriage at about the fifth month, and lastly of a pair of boys, the exact counterpart, I believe, of the "Siamese Twins."

The two children are about the same size, both males, seventeen and a half inches in length, perfectly formed, and weigh eleven pounds twelve ounces. The point of union begins at the ensiform cartilage, and continues down the middle line of the body to the umbilicus, in which it terminates. The thoracic cavities are distinct, and so I think are the abdominal, in their upper portion. The lower part, however, seems to be without any interposing tissue, although from outward appearances, two complete sets of organs exist therein. I was somewhat inclined to satisfy my mind upon this point by dissection, but concluded to keep them unmutilated, until our medical brethren of the city (whither I have sent them this evening) could have an opportunity of inspecting them.

I did not reach the case until after the birth was accomplished, and am informed of the following particulars by the attending midwife:

Labor pains commenced about night on the 18th instant, and at 11 o'clock the head of one was ushered into the world. The pains then ceased for about three hours, when they resumed, and the shoulder of the same child was pushed forward, quickly followed by the head of the second, and the *rapid* completion of labor, *without any further difficulty whatever*, by 4½ o'clock, A M. There was but one cord, very short (about eight inches,) coming out directly *underneath* the abdominal union, and one placenta, in no way peculiar.

The midwife says neither child ever breathed. The mother affirms that she distinctly, and unmistakably felt them move, just before the head of the first one was born. My own impression is, that the first born *was* alive for a time at least, because its neck is quite swollen, from the efforts (I suppose) to deliver it.

They have been carried to the full term evidently, with nothing unusual during gestation.

Such is the history of these monstrosities, and I wish you to inspect them for yourself.

The mother is doing well so far.

Feb. 20, 1858. [*Virginia Medical Journal.*

ON THE LONG-CONTINUED USE OF BELLADONNA.—A class of cases in which these solutions are exceedingly useful, are those in which either a central opacity of the cornea exists, or the iris is tied down by adhesions, and the pupil partially obliterated. Under either of these circumstances, artificial dilatations of the pupil often suffices to afford to the patient vastly improved vision. These objects the solution of belladonna or of atropine will accomplish for any length of time. The fact is well vouched for by writers, and is frequently confirmed in practice, that habit does not diminish its influence. We have known patients continue its employment for ten years or more, its activity being just as apparent at the end of the time as at first, and no increase in the strength of the solution having been required. It may, perhaps, be deemed rather clumsy practice to set a man to put drops into his eye every day for the rest of his life, when a simple operation for artificial pupil might at once secure the object desired; but still, there are circumstances, and those not of unfrequent occurrence, in which the former practice is preferable to the latter. The one requires a skilled operator, whilst the other is devoid of the slightest risk, and open to the adoption of any one. At any rate, it is to be regretted that the efficiency of the plan by the permanent use of belladonna is not more widely appreciated than it is. The cases scattered up and down the country are probably not a few in which great benefit would be derived from its adoption.—*The Medical News and Library.*

EVILS OF THE MODERN SYSTEM OF FATTENING CATTLE.—Mr. Gant has done good service to the public in exposing some of the evils of the modern system of fattening cattle. Mr. G. has inspected the prize animals at the last exhibition of the Smithfield Cattle Club, has followed them to their destinations, and inspected them after death. He found exactly what any one would expect to find who had thought upon the physiological aspect of the question: fatty degeneration of the muscular tissue, and excessive deposit of fat in those situations where it is normally present. He also found numerous filariæ in the lungs, tubercles in various stages, congested livers, and all the signs of general venous engorgement. It is quite clear that the nutritive value of the flesh of such animals is most materially diminished by the conversion into fat. A certain admixture of fat and muscle is desirable, but the conversion of muscle into fat is a change which seriously impairs the quality of meat. So far from these over-fed animals being deemed worthy of a prize, the breeders should be condemned to a penalty. The panting breathlessness from the congested lungs and weakened heart—the dull, stupid expression from the congested brain—the inability to move—are positively disgraceful to agriculturists, and should certainly attract the attention of the Society for the Prevention of Cruelty to Animals.—*Med. Times and Gaz.*, Jan. 16, 1858.

OPERATION FOR MERCURIAL TRISMUS OF THE JAW, OF FOUR YEARS' STANDING. *By Smith Warner, M. D., of Lumpkin, Georgia.*—Mr. C. C., a promising young man, aged seventeen years, applied to me in February, 1855, to be cured of immobility of the lower jaw, which had existed for four years, and which was the result of severe ptyalism.

Upon examination, I found that there was slight lateral motion of the jaw, showing that the pterygoid muscles were still under the control of the will, and that no osseous union had taken place at the articulations, but there was no downward motion whatever. This perfect closure of the jaw interfered very much with his enunciation, and also with his eating: he subsisted entirely upon a fluid and semi-fluid diet, and such fine particles of solid food as he could insinuate between the canine and bi-cuspid teeth on the left side, where a little irregularity in their arrangement had left a gap.

On the right side alone strong tendinous contractions existed, from the front of the first back to the last molar tooth, (as I supposed,)—for no exploration could then be made in that direction.

I placed my patient on an elevated chair, facing a strong, clear light, and stood before him, while an assistant held his hand steady, and by means of a common-sized stiff spatula held open and forced out the angle of the mouth on the right side. With a straight bistoury I soon divided the tendinous bands, determining by sight and touch when the last was cut. Very little force was needed to open the jaw, as my patient easily opened it of his own accord, when the operation had been completed.

After Treatment.—A wedge of soft white pine, of sufficient size to keep the mouth well opened, was inserted, and I required him to wear or keep this wedge between his teeth during the healing process, to be removed for a short time when any sense of fatigue was experienced. I also required of him the introduction and retention, between the teeth and the cheek of the side operated on, of an oval ball as large as a hen's egg, made of soft cotton thread, or sponge, well oiled, to be worn either with or without the wedge, as an aid in preventing contraction.

My patient was soon well, having as perfect use of his jaw as though nothing had ever happened.

I have noticed (since performing this operation) the report of sixteen cases by Dr. Valentine Mott, in the third volume of Velpeau's Operative Surgery, which he styles "a new chapter in Surgery."—*Charleston Medical Journal and Review.*

VERATRUM VIRIDE.—*Messrs. Editors:*—Having noticed in the Journal of the 11th inst. some remarks made by Dr. Coale, on the effects of the veratrum viride in controlling arterial excitement, I thought I would send you an outline of my experience with the same remedy; which, although it has not been very extensive, has been sufficient to convince me that the veratrum is *one* of the most, if not the most valuable of the arterial sedatives we possess. My first experience with this article was in April last. Since then I have treated five severe cases of pneumonia with the veratrum as the chief remedy.

Case.—J. K., aged fourteen years, after having a bad cough for several days, was seized, on the 10th of May last, with a severe chill, followed by fever, severe pain in the right side of the chest, and hurried and painful respiration. I saw him on the 11th, and found him with the above symptoms much aggravated; skin intensely hot; tongue covered with a white fur; pain in right side, very severe, and much aggravated by coughing; dullness on percussion, and crepitation throughout both lobes of right lung; sputa rusty and perfectly characteristic of pneumonia; pulse 115. I ordered a mustard poultice to the side, and left a solution of tart. antimon. et potass. to be given in nauseating doses every two hours; the patient to partake freely of mucilaginous drinks.

12th, 9 A. M.—Patient much worse. Pain in side unmitigated; sputa very rusty and tenacious; bowels moved a dozen or more times during the night; tongue dry and red; pulse 142, and weak. Discontinue the emet. tart. and substitute Norwood's tinct. verat. vir., to be given every three hours, beginning with three drops, and increasing each dose by one drop until vomiting ensue, then dimishing the dose one-half, and continuing every three hours. Mustard poultice, and mucilaginous drinks as before. 8, P. M.—Patient vomited after the third dose (five drops) of tinct., when all the symptoms were much relieved. Expectorates more easily; sputa less tenacious, but still very rusty; skin cooler; tongue moist; pulse 130. Continue the tinct. in two-drop doses every three hours.

13th, 9 A. M.—Rested well last night; skin quite cool and moist; very little pain is complained of; expectoration easy; sputa less rusty and tenacious; pulse 94. Continue the tinct. 8, P. M.—Side rather more painful than in the morning; pulse 72; slight hepatization in lower lobe; large blister to side; calomel one-third of a grain every three hours; diminish dose of tinct. to one drop.

14th, 9, A. M.—The blister drew in five hours. No pain complained of this morning; expectorates freely; rusty appearance of sputa nearly gone; pulse 56; signs of hepatization disappearing; patient asks for some meat. Omit tinct. and calomel, and give nothing but decoction of senega every four hours. Some mutton and beef-tea allowed.

From this time his improvement was rapid and uninterrupted, and he was at work in two weeks from the time treatment was commenced.

The other four were as well-marked cases of pneumonia; were treated in the same manner, and with equally satisfactory results. I have recently treated two severe cases of simple pleuritis with the veratrum, in both of which it reduced the pulse, very promptly, from 125 beats in the minute to its natural standard. I have also used it in scarlatina this winter, with excellent effects. In one case, where the pulse was 125 in the minute, and the skin excessively hot on the second day of the eruption, the pulse was reduced to 82 in the minute, and the skin lost its peculiar pungency and became quite cool in twelve hours from the exhibition of the first dose of the veratrum. It seems to be well adapted to all cases in which an arterial sedative is indicated; but from the experience I have had with it, I am inclined to believe that its effects are more certain in pneumonic affections, than in any others. The great advantage that it possesses over antimony in those cases, is, that it controls the pulse effectually, with very little, if any, tendency to irritate the gastro-intestinal mucous membrane.

In order to get the full benefit of the medicine in any case, it must not be discontinued as soon as the pulse is brought to the required standard, but must be continued, in diminished doses, if necessary, until the "*irritability*" of the heart is completely subdued. G. W. SPALSBURY.

Three Rivers, Mich., Feb. 1858.

[*Boston Medical and Surgical Journal.*

UNEXPECTED TURNING BY EXTERNAL MANIPULATION.—*By Dr. Favenue, of Leschelles.*—This case is very curious, as showing the value of manipulation through the walls of the abdomen in the process of turning, for if this mode of treatment is effectual under circumstances such as are here described, what may not be expected where the membranes are not ruptured, and where proper promptness is employed.

Case.—Madame Jamart, of Benzoufolie, aged forty-six, tall, strong, well-made, and already the mother of seven children. Dr. Favenue was called in consultation on the night of the 6th of March, 1855, and found that the patient had been in labor for forty-eight hours. He found also the left arm of the child protruding from the vulva, ecchymosed, deprived of epidermis, and limp like that of a dead child. The cord, also, was broken, and evidently great efforts had been made to procure delivery. The child appeared to have been dead for about twenty-four hours.

Under these circumstances, Dr. Favenue attempted to turn in the ordinary way, but the swollen and painful state of the vulva, and the contracted and ever-contracting state of the uterus, rendered the attempts altogether unsuccessful. All this time the patient was greatly exhausted, and the uterine pains were violent and incessant. Perforation seemed to be inevitable, but before having recourse to this severe measure, another attempt was made at turning, and in doing this the unoccupied hand of the operator was pressed upon the hypogastric region over the uterine tumor, when to Dr. Favenue's great surprise, a movement was felt within the uterus, and the protruded arm receded out of reach. Immediately afterwards, there followed a violent pain, and the lumbar region of the child was forced down into the vulva. This presentation, partly by manipulation with one hand through the walls of the abdomen, and partly by manipulation in the ordinary way, changed to that of the nates, and in the end the child, which was very large, was delivered without any great difficulty. Since this time the mother has been delivered of another child.—[*L'Abeille Méd.*, July 5, 1857, *Gaz. Hébdom. de Med. et Chir.* July 31,1857.

Substitute for Cod-Liver Oil.—Dr. G. P. Cammann, in a letter to the New York Journal of Medicine, (January, 1857,) proposes the oily substance taken from the cavities in the head of the spermaceti whale—known in commerce as "head matter"—as a substitute for cod-liver oil. It should be used in its fresh state, as it becomes rancid by keeping. It may be obtained from the manufacturers of sperm candles. Kane says spermaceti consists of two atoms of margaric acid, one of oleic acid, and three of ethal. Dr. C. proposes, as an appropriate name for the substance proposed, Ol. Ethal, as being composed mostly of oils and ethal.

"It is preferable to cod-liver oil on account of being more agreeable to the taste, leaving a pleasant flavor in the mouth, and also being more nutritive and soothing. It is also less apt to disagree with the stomach, and does not cause offensive eructations. The patient may take it either pure, in coffee, or with bread, boiled rice, potatoes, etc. When required, tinct. opii camph. or liq. ferri iod. may be added."—*Medical and Surgical Reporter.*

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CURIOUS CASE OF HALLUCINATION, IN THE TEMPORARY LUNATIC ASYLUM, NEW ORLEANS.

By C. Delery, M. D.

WITH REMARKS ON THE NATURE AND CAUSES OF MENTAL HALLUCINATIONS.

By Anthony Peniston, M. D., Professor of Physiology, New Orleans School of Medicine.

Case.—Mary Welch, a young Irish girl, aged 20, entered the Temporary Lunatic Asylum on the 27th day of last January. She is of rather small stature, but well and stoutly made, red haired, with grey eyes and ruddy complexion. She blushes very easily. While speaking, she winks her eyes, and her hands have an almost imperceptible tremulousness. Her husband died of yellow fever during the epidemic of 1853, after having been married but a very few weeks, and she declares that her life has been chaste since that period, notwithstanding her courses have stopped for two months.

Upon being questioned as to her usual habits, she acknowledges that she drinks, but moderately; never to a degree to impair her senses, nor to prevent her from working. She thus describes the cause of her presence in the Asylum.

"I was employed at Mr. B.'s, on Orange street, between Race and Tchoupitoulas. I had been six months in the house, when my mother insisted that I should leave. I did not like to do so, as the situation suited me; I therefore, refused to comply with her desire. On the evening of the 25th or 26th of January, about half-past 9, my mother came to Mr. B.'s house, accompanied by a Mr. T. O. I first heard her speak in the yard, and afterwards go up into the second story.

"On reaching the balcony of the second story, she had an altercation with Mr. B. I was in bed with Mr. B.'s two children. On hearing the quarrel, I went on the lower gallery, where I could see all that was passing, without being seen. I could see Mr. B. very distinctly; he had a revolver in his hand, and was threatening my mother. His wife was standing beside him, holding his arm, and begging him not to fire, but to send for a police officer to put that woman out of the house. Her remonstrances were vain, as Mr. B. fired and my mother fell, her head pierced by two bullets. Mr. T. O. ran to her assistance, but also fell, wounded by two shots. I saw him fall, but cannot say positively if his death was instantaneous. 'They are both dead,' said Mr. B. 'Make a hole in the yard,' said Mrs. B., 'bury them, and say nothing more about it.' 'But, said Mr. B., 'the girl, who is sleeping down stairs, must have heard me; she will denounce me; I must kill her also.' 'She has heard nothing, she is sleeping,' said Mrs. B.; 'you must not kill her, she is innocent.' At this moment I went into the room, awoke the children, and told them that I was going to run away, as I was afraid of their father. They wished to prevent me, but I escaped. While I was going down stairs, Mr. B. said to his wife, 'you see she is going to escape; I must follow her.' I ran into the street, crying murder, closely followed by Mr. B., who kept on saying, 'it is useless to run; I'll finish you also.' After running two or three hundred steps, I came in front of a house which was in process of construction; fortunately for me, Mr. B. stumbled over some bricks which were in the way, and his revolver fell from his hands. While he was endeavoring to get up and look for his weapon, I got some distance in advance of him, and arrived at the Magazine Street Market, which was then lighted The light frightened the assassin, who went back, and I then related what had passed to a police officer who was standing in the market. I reproached him for not having come when I had cried 'murder.' He said he had not heard me, but begged of me to conduct him to the place, which I did. On the way he called on two other officers, who stationed themselves at Mr. B.'s gate. When the door was opened, we saw Mr. B. walking in the yard, very much agitated. He came towards the police officer and said, 'Here I am, sir.' Mrs. B. was close behind him. I then showed the officer the room in which my mother had been murdered, and told him to look out of the window and he would see her body. He went to the widow and said, 'It is true, they are killed.' I then asked him to go into my room and bring me my clothes, as I wanted to dress myself. He went into the room, followed by Mr. B., and brought my clothes. I dressed, and the officer conducted me to the Station-House."

"Mary," said I, "this is a strange story you are relating; are you certain that your senses have not deceived you?" "The facts that I have related are real." "Were you placed in such a manner as to see distinctly what was passing over your head?" "Perfectly." "How can you explain why the four shots did not attract the attention of the neighbors?" "It was raining very hard at the time, and the scene passed at the back of the yard. The pistol shots did not make much more noise than the shutting of a door.". "But I cannot imagine how the officer whom you called to your assistance, and who certified the double murder, should have allowed the murderer to escape, and seize you, who are the daughter of one of the victims." "I think, sir," said she, "that he was bribed by Mr. B., who followed him when he went into the room to get my clothes." "But the next morning, when you appeared before the Recorder, how did it happen that your narration did not decide them to make an inquest?" "When I appeared before the Recorder, it was some hours after the event and I was too much excited to explain myself very clearly. Before my declaration was entirely finished, a person sent by Mr. B., came and touched me under the chin, and in order to deceive the judge, said 'well, Mary, how is your head now?' He then made affidavit that I was crazy. It is on his simple declaration that I am here."

It was astounding to see with what assurance Mary related this history; how precise she was in all the details, and with what promptitude she replied to all the objections that were made. I questioned her several days without discovering the slightest contradiction in her recital. I brought several persons to see her, and among others a very learned physician of this city, Dr. Alfred Mercier, and every person was struck with the tone of conviction with which she related her history. I resolved at last to make an investigation, in order to assure myself of the real facts of the case.

"Mary," said I, "can you give me a proof of the truth of this strange story that you have been relating during a week?" "Very easily; go to St. Mary street, between Rousseau and Religious, in the middle of this street, near the Public School, you will see a lattice-work which looks into a yard; at the end of this yard is a small house, where my mother resided. Her name was Catherine Welch. My brother, Denis, aged 16, and my sister Bridget, aged 12, are, without doubt, still residing there. Ask for them, and find out what has become of my mother. They will tell you she has disappeared."

I went to the place and had no difficulty in finding it, she had given such plain directions. The lower part of the house was unoccupied; I went up stairs, where I found an old woman, sitting at a table with a young man of about 16, and a young girl of 12. "Are you Catherine Welch?" I asked of the old woman, who had risen to receive me. "Yes, sir." "Have you a daughter named Mary Welch?" The old woman turned very pale, and replied, trembling, "Yes, sir." "Do you know where she is?" "No, sir; she disappeared several days ago." "Did she drink?" "A great deal, sir." I then related to her the strange story that her daughter had told us. She burst into tears, and said, "My poor daughter is crazy; there is no truth in what she says." "Has she worked for Mr. B.?" "Yes; she was employed as servant in his house; but I had no manner of difficulty with him." "Madam," said I, "your daughter is in the Lunatic Asylum; here is the written address. You can come there to-morrow, between nine and ten. If you should arrive before me, wait till I can be present at the interview between you and her." She promised to do so, and I left.

The next morning, as I was about to leave, a celebrated lawyer begged of me to allow him to visit my patient. When we arrived the mother was already waiting for me. I begged of her to remain some time longer at the door, and we, Mr. S., Dr. Alfred Mercier, and myself, went in. Mary repeated, for the tenth time, the particulars we have already related. "Are you aware, said Mr. S., rather roughly, "that Mr. B. is to be hung to-day?" She cast down her eyes and appeared very much embarrassed. "It seems to trouble you," said Mr. S. "It does, sir," said she, "because Mr. B. is the father of a family, and I am very much attached to his children." "But he killed your mother." "True, sir; but his death will not restore her." "Mary," said I, "what would you say if I made your mother appear before you." "I should say that I am crazy." "Tell her to come in," I said in French to the Keeper of the Asylum. She came in and stood before her daughter. It is difficult to describe the rapid changes that Mary's countenance underwent. She looked at her mother from head to foot, her eyes twinkled, her face became red, her bosom heaved, and most probably she would have fainted had not a torrent of tears relieved her bursting heart. She arose immediately and endeavored to leave the room. I ran after her, and said "How is this, Mary; yesterday you were terribly afflicted because you thought your mother was dead, to-day I bring her to see you, and instead of rejoicing, you weep and repulse her?" "No! no!" she replied, in an agitated voice, "this is not my mother; I saw her killed." "Come," said I, "embrace her and weep; it will do you good." She followed me mechanically, but did not wish to go near her mother. She conversed with her sister, Bridget, and begged of her to bring her some clothes when she returned to the Asylum. I asked her the next day if she wished to be set at liberty. "No, she replied, I am too much afraid of Mr. B.; I am the only witness of his murder; it is his interest to make away with me."

"You still insist, then, that this is not your mother who comes to see you?" "There are moments," said she, "when it appears as if it must be she, but then again the image of my murdered mother rises to my imagination and convinces me that this cannot be." "If this woman," said I, "be not your mother, how is it that you are so strangely moved at seeing her?" "Because," said she, "she always wears my mother's bonnet and cloak."

As soon as the weather became favorable, I gave Mary alternate shower baths and pugatives. As a last resource, I placed a large blister on her arm, and caused it to suppurate. After following this mode of treatment for some days, her reason was completely restored. She recognized her mother, with whom she left the Asylum, laughing herself at the strange hallucination, which she attributes to certain drops that a lady of her acquaintance had given her in order to cure a cough. It seems to me that her folly is attributable rather to the constant and immoderate use of spirituous liquors. It is a kind of folly not uncommon in the Lunatic Asylum of this city. Very recently a worthy Irishman, not of the most abstemious habits, received the visit of a sorcerer every evening at 8 o'clock. "He raps at my door," said he, "ordering me to open, and as I do not choose to obey, he comes in by the key-hole and seats himself on my bolster, where he passes the night, grinning at me and spitting in my face." After three days of total abstinence, the sorcerer disappeared. "I feel very well now," said he, "I drank too much; I want to go back to my work." "Go," I said, "and do not return again to the bottle, it is in it that all sorcerers are lodged." "They may remain there, said he, "as far as I am concerned; I shall not dislodge them again in a hurry."

New Orleans, March 3, 1858.

REMARKS.

The organization of man is a marvelous union of mind and matter; and this has given rise to the beautiful definition: Man is an intelligence, with organs subservient to it. The physician can never lose sight of this compound structure; for what is more trite, more true, than the influence of the mind upon the body, even in the ordinary run of diseases. In those whose entire seat is in some part of the nervous system, the connection is more readily traced; for the brain is now admitted to be the sensorium commune, the great centre whence radiate all impressions, whether conducted by the sensitive nerves to the brain, or from the brain to the extremities by the motor or efferent nerves. In this admirable and complex machinery, is it astonishing that some of its parts may at times be deranged? Can we not comprehend that cultivation may develop some parts at the expense of others, just as the muscles of the boxer, or of the pedestrian, become almost hypertrophied, and, correspondingly, certain organs or faculties may be atrophied from disuse or inaction? We thus enter the threshold of one of the most interesting branches of medical inquiry, the problems of which must inevitably compel the physician to undertake those metaphysical questions which have heretofore been considered a distinct branch of science. It is evident we cannot adopt the purely material position, and consider thought as the result of certain chemical or molecular changes, going on in the substance of the brain, although such changes are evidenced by alterations in the excretions of the body, those outward manifestations of internal waste. It is undoubtedly true, that as we advance in chemical and physical science, many physiological phenomena, now apparently complex and inexplicable, will be made simple and easy; and the processes of thought are not without analogy in chemical and physical agencies. Take, for instance, a plate of zinc, another of copper, plunged into dilute sulphuric acid, and you obtain an electric current, which may travel along a wire with almost inconceivable rapidity. Well! are not the nerves a set of conductors, and the brain the registering and generating battery, to which impressions are incessantly conducted from without, elaborated and again transmitted outwards, where they make their appearance, either in the movement of some muscular apparatus, or by the voice, that inimitable mechanism, by which man communicates to his fellow beings the results of his intellection. The analogy between the galvanic force and the nervous current is made still greater by considering the intimate structure of the brain. For the dissimilar metals and the interposed fluids, which we have seen are necessary for the production of the galvanic force, have, as analogies, to determine the polarity of nerves, the two kinds of nervous matter, viz: the vesicular, and the fibrous; while the blood is the interposed liquid. While we suggest these striking similarities between the structure of the brain and an electrical battery, we would not argue to their absolute identity; no more than we could define thought to be a secretion of the brain. There can be no doubt, on the other hand, that the thermo-electrical forces will explain to us ultimately many of the properties of nerves. One of the great difficulties, for instance, in the electrical theory is the nature of the impressions transmitted by centripetal nerves, from the extremities to the centre. There the analogy of the mode of development of the current is not so obvious. But when we remember how easily thermo-electric currents may be excited by disturbing the equilibrium of heat in a wire of even a single metal, it seems not unreasonable to refer this excitability of nerve to some similar proneness to change in it.

But, go as far as we may in explaining the wonderful combination of the etherial essence with the grosser materials of the body, there seems to be a point beyond which we cannot go; and we must, perhaps, be forced to admit that ideas are different from sensations. "Therefore," say Todd and Bowman, "although the workings of the mind are doubtless independent of the body, experience teaches us that in those combinations of thought which take place in the exercise of the intellect, the nervous force is called into play in many a devious track throughout the intricate structure of the brain. How else can we explain the bodily exhaustion which mental labor induces? The brain often gives away like an overwrought machine, under the long sustained exercise of a vigorous intellectual effort; and many a master mind of the present or a former age has, from this cause, ended his days a driveller and a show." As light is indispensable for vision,—as sound is indispensable for hearing,—as odorous particles are indispensable for smelling, notwithstanding the different arrangements by which the senses are adapted to receive their special exciting agents, so may the brain be the instrument, the seat, of intellectual operations, though not their creator and originator. It is true that it has been asserted, that the weight of the brain is in direct proportion to the intellectual power of the individual; and in proof of this, the brains of Byron, Cuvier, and Dupuytren, and others, are cited as examples of enormous development of matter, proportionate to the genius and talent of their possessors. But, in thus asserting a correspondence between the development of the cerebrum and intellectual capability, we may still assert the instrumental nature of that organ. So that imperfections in it may produce corresponding inferiority in the exercise of its powers, though no ways affecting the essence of the intellectual principle itself. Psychology, or the science of mind, has heretofore been left to the schoolmen, and the so-called philosophers, who have written learned and abstruse treatises on the intellectual powers, though entirely ignorant of the composition and structure of the organ which is, at least, the seat, if not the source, of those very faculties. From the physician, from the physiologist and anatomist alone can come those data which will eventually build up the science of mind on a solid foundation. Among the most fruitful sources of knowledge in this direction, we may look to the physiology of insanity, if I may so speak, as a rich mine of psycological investigation. There are all grades of insanity; from the slightest deviation of a well balanced standard, to the wild delirium of a maniac. Passing through all these grades, the operations of the mind may all be consecutively studied; and, as in other departments of physiology we remove an organ or a nerve to study its properties or functions by its very absence, knowing the harmonious working of the whole, so may the nature and properties of the intellectual principle be elucidated by the dreams of the extatic, the visions of the hallucinated, or the ravings of the maniac. As the traveler, who surveys the ruins of cities famous in history, seeks on their marble tablets the confirmation of legendary records,—as the remains of Nineveh, and Babylon, and Egypt, bear witness of the glories of the past, so the very ruin of the intellectual faculties may be urged in proof of an immortal essence, distinct from its tabernacle and dwelling place. In fact, the subject of hallucinations, considered in a medical view, offers the most serious subjects of meditation, not only to the physician and jurist, but to the moralist, the historian, and the theologian. For, if all hallucinations are to be classed among the products of a disordered imagination, many passages of the sacred writings may be attacked as unsound; and many popular beliefs, under the same tests, would scarcely stand the scrutiny of scientific investigation. It is evident, therefore, that we must at once draw a line of demarcation between the vagaries of mind which are the province of the physician, and those mysteries of another nature, whose origin is coeval with man; whose belief is implanted in his nature, and on whose foundations many of the institutions of society raise their proud superstructure.

In proof of the importance of the subject in an historical point of view, I cannot resist quoting the following beautiful lines from a great American physiologist, Professor Draper, of New York: "It has been said that a belief in apparitions is natural to every man. However much we may dissent from the correctness of such a general assertion, there can be no doubt that it has a foundation in truth. The faith of a child in this particular is only gradually sapped as he grows up to be a man. Nay, even in mature life there may always be found those who have an unwavering confidence in the reality of these illusions; and many of these are characterized by their moral courage and love of truth. I have just remarked that few things have exerted a greater influence on the career of the human race than a firm belief in these spiritual visitations. The visions of the Arabian Prophet have ended in tincturing the daily life of half the people of Asia and Africa for a thousand years. A spectre that came into the camp at Sardis, unnerved the heart of Brutus, and thereby put an end to the political system that had made the great republic the arbiter of the world. Another, that appeared to Constantine, strengthened his hand to the accomplishment of that most difficult of all tasks of a statesman, the destruction of an ancient faith."

What, then, are hallucinations? what is their physiological explanation?

In the first place, we must premise by establishing two great classes of hallucinations: those which are compatible with reason,—in other words, the patient is aware of the disorder of his senses; and that other great division, comprehending many varieties, but accompanied by more or less mental disorder, in which the patient is not aware of his state, or the unreality of his visions.

Hallucinations may be defined to be an error of the mind in which ideas are taken for realities, or real objects are misrepresented by the cerebral ganglia, without any general derangement of the intellectual powers.

The perception of external objects is through the medium of the apparatus of vision; for the eye, like the *camera obscura*, receives the images of surrounding objects and repeats them upon the retina; from the latter, which is the expansion of the optic nerve, the sensitive impressions are conducted to the brain and there registered in the optic thalami, the sensitive ganglia of the brain. As to the nature of these impressions, thus transmitted to the registering ganglia, we know but little, except that in the normal state they convey to the mind an exact idea of surrounding objects; for, after all, what we really see is not the objects themselves, or even their shadow, upon the retina, but the impression, which is transmitted through the optic nerves to both hemispheres of the brain. These impressions, perhaps, do not at all resemble the objects from which they are derived, no more than a word, which is a conventional sign of an idea, resembles the object for which it stands; and yet, why should not each sensation of the brain be a microscopical Daguerreotype of the reality! Then, memory would be the power of calling forth old shadows from their resting place of years to assume the distinctness and vividness of actuality; and when in dreams we live over the scenes of the past, and call around us with vivid intensity the faces of departed friends, it is as if, when reason is away, the imagination in her mad career pulls down from the picture galleries of the mind the Daguerreotypes of friends and foes, and revels in wild conceits of her own at the memories of auld lang syne. Now, this emergence of old impressions so common in dreams, may take place during our waking moments. It would seem as if, from some morbid condition of these ganglia, certain impressions are incessantly resuscitated with as much or even greater distinctness than actual sensations; and in this way a subjective state of the brain may be mistaken for the reality of a visual impression. In some cases the shadowy apparition may be projected among real forms, and, in many instances on record, the patient could discriminate between the reality and the visionary, even with the eyes open; but, as the appearance still continues even when the eyes are shut, it becomes a convincing proof that these hallucinations are dependent upon the phenomena of inverse vision. And, though in general, by an effort of reason, it is thus possible to convince ourselves of the unreality of these impressions, and, by starting from these waking dreams, chase the phantoms away, yet, how many give themselves up to the belief in their supernatural origin! And such is the constitution of man, that the bravest and wisest encounter these fictions of their own organization with awe and trembling. This explains to us why, at all times in the history of man, in the most different latitudes, under opposite kinds of government, with various religions, we always find the same belief in spirits and apparitions. The source of this belief must evidently be sought in the psychical nature of man himself, predisposed as he is, by an irresistible fondness for the marvellous, to accept as real the morbid manifestations of his brain. The savage, who dreams of the Great Spirit and the incomprehensible mysteries of another life,—he who in the middle ages knelt before the shrine of Saint Patrick, and sought a remission of purgatory,—the Arab, who wanders in the enchanted palaces of Alladin,—the Indian, absorbed in the incarnation of Brahma, and the inhabitant of our civilized communities, who, in public, proclaims his universal skepticism, but who in private demands from the fortune-teller or the magnetiser the revelation of secrets beyond their ken—what is this more than a manifestation of this same propensity, this yearning for the wonderful and unknown ?

The various phenomena which may be considered under the term of hallucination, are exceedingly numerous, and susceptible of different classifications, according as they are or not complicated with insanity, and according as these aberrations of the sensitive ganglia affect one or other of the organs of sense.

In the first place, hallucinations must be distinguished from what are called illusions. The latter include certain perversions of the senses, through which real sensations are distorted in nature and form; whereas the term hallucination implies that state of the cerebrum according to which the patient is under the belief of the actual existence of certain impressions of the senses, although entirely false and unreal.

These disorders are of various kinds. Thus, we have hallucinations of the sense of sight; and they are the most common; of the sense of hearing, smell, and taste. In all of these cases the disease evidently includes those portions of the brain in which the nerves presiding over the organs of special sense send their terminal branches. On the other hand, it is a well known fact that certain substances, such as opium, stramonium, Indian hemp, alcohol, and others, have the property of bringing on certain states of the mind which are akin to hallucinations, only they disappear as the influence of the drug ceases. Yet, so pleasing are these fantastical visions while they last, that many have sought in the poisoned cup that oblivion from all cares, and that incomparable bliss, greater than all pleasures of earth, which are said to be thus enjoyed as a foretaste of Paradise.

Sir Humphrey Davy, who first recognized the physiological properties of protoxide of nitrogen, or laughing gas, has given us a graphic description of these effects: "A thrilling, extending from the chest to the extremities, was almost immediately produced. I felt a sense of tangible extension, highly pleasurable, in every limb. My visible impressions were dazzling, and apparently magnified. I heard distinctly every sound in the room, and was perfectly aware of my situation. By degrees, as the pleasurable sensation increased, I lost all connection with external things; trains of vivid, visible images rapidly passed through my mind, and were connected with words in such a manner as to produce sensations perfectly novel. I existed in a world of newly connected and newly modified ideas. When I was awakened from this semi-delirious trance, by Dr. Kinglake, who took the bag from my mouth, indignation and pride were the first feelings produced by the sight of the persons about me. My emotions were enthusiastic and sublime, and for a moment I walked around the room perfectly regardless of what was said to me. As I recovered my former state of mind, I felt an inclination to communicate the discoveries I had made during the experiment. I endeavored to recall the ideas; they were feeble and indistinct. One recollection of terms, however, presented itself, and with the most intense belief and prophetic manner I exclaimed to Dr. Kinglake, "Nothing exists but thoughts; the universe is composed of impressions, ideas, pleasures, and pains."

Somewhat akin to the above, though different in intensity, are the effects of the famous Haschisch, or the oriental extract of Cannabis Indica. It appears that even in the middle ages this preparation was known, and that the Old Man of the Mountain, who exerted such wonderful influence on his emissaries of vengeance, prepared them for their mission by presenting to their astonished senses a paradise of rapturous delight, by means of this very substance.

In modern times this drug has been experimented upon by numerous persons, and its effects appear to be extremely uncertain, at least in the shape in which we commonly find it in the shops. Travellers, however, who have imported it, or tried it in the East, speak of its effects in the most wonderful terms. Theophile Gauthier, a celebrated French writer, speaks of its effects upon himself, in the following terms, which we attempt to translate:

"About half an hour afterwards, I was under the full influence of the Haschisch. My visions became complicated and wonderful. Through the air, brilliant with phosphoric light, flew millions of butterflies whose crystal wings went crepitating through space. On every side gigantic flowers, with diamond calices, enormous roses, with petals like rubies, and lilies of gold and silver, were blooming around me, emitting scintilations as bright and dazzling as the wonders of the pyrotechnist. My hearing was marvellously developed; I heard the noise of colors in harmonious notes. Sounds which were green, red, blue and yellow came to my ear in distinct undulations. The least noise, like the moving of a tumbler, the creaking of a chair, or the whispering of a word, resounded in my ear like the rolling of thunder. My own voice appeared so loud that I feared to speak, lest the blast would rend the walls asunder, or the detonation shiver me to pieces like a shell from gunpowder. More than five hundred clocks appeared to chime the peal of passing time with tones the most silvery and soft. Each object which I touched gave forth sounds like an æolian harp, or the low notes of the harmonica. Thus, methought I floated in an ocean of sound, through which were wafted the masterpieces, the gems, of the most celebrated operas. Never had such delightful sensations pervaded my senses. My spirti seemed detached from the body, and lost in indefinite space. Thus, disenthralled from the egotism of self, I could understand the nature of celestial spirits, of angels, and the independence of the soul from its earthy mould. Methought that, like a sponge, I floated in a sea of delight; I was pervaded at every pore by waves of voluptuous sensations; my whole system, to the minutest capillary, seemed loaded with liquid pleasure, and no words can express the fantastic ideas which followed in rapid succession through my brain. Light, sounds and perfumes came to me through myriads of channels, and magnetic currents seemed to animate each nervous filament. According to my calculation, this state lasted about three hundred years. Nor can I estimate, by the ordinary measure of time, the sensations, ideas and emotions which succeeded each other with thrilling intensity through my whole body. When I awoke from my dream I found it had only lasted a quarter of an hour."

If such indeed be the delights which the Eastern opium eater finds in his unceasing dream, can we wonder that, having once tasted its enrapturing draughts, the world of reality appears so dull and tedious, that he ever seeks emancipation from its thraldom?

In the same way, the explanation of the drunkard's devotion to his bottle is to be found in that cerebral excitement which is the consequence of alcoholic intoxication in the first periods. Thus the miserable slave to this vice, for the sake of a few fleeting moments of happiness, incurs all the misery which it inevitably entails on its besotted votaries.

We have stated that hallucinations are to be distinguished from insanity; but it is evident that as colors insensibly blend with each other, as the shadows of evening gradually melt into night, so the various forms of mental disorder connected with hallucinations insensibly merge the one into the other, and the line of demarcation can scarcely be drawn between neighboring but different varieties of mental disease. One of the most distinguished French writers on insanity says: "I have often tried to establish some standard by which to discriminate between rational ideas and those which may be called nonsensical, and when I sought among the inmates of the Lunatic Asylums of Paris, the most absurd conception, apparently, in itself, and afterwards compared it to many ideas of the same kind which passed current in the world, I was forced and humbled to admit that there were no real fundamental differences between them." Mr. Lelut, in his interesting work entitled "The Demon of Socrates," says: "Will you call that man crazy who, under the dominion of some exclusive thought, acts in a manner which appears strange and unaccountable to others? But how many celebrated painters, great musicians, and even learned men, have been remarkable for their eccentricities in relation to their favorite pursuits? In our own normal state have we not in our moral organization some strange peculiarity, some weakness which we cannot explain, and still less overcome? Thus, with the same ideas, according to the time in which he lived, a man may be considered a sage or a lunatic. Man judges others according to his own standard; our own reason is our measure of the folly of others. These ideas, though apparently abstract in their nature, are as much the province of the physician as of the moralist; he must study man as he is, and not as he should be; nor does he know at what time he may be called upon as an expert, to decide legally upon the sanity or insanity of a person, to prove the culpability or irresponsibility of acts which in one is mania, in another guilt and crime.

ON THE CONSTITUTIONAL EFFECTS PRODUCED BY SUBSTANCES INTRODUCED INTO THE URETHRA.

By I. L. Crawcour, M. D., F. S. A., Professor of Chemistry and Medical Jurisprudence, New Orleans School of Medicine.

In the summer of 1857 I noticed that when bougies, tipped with an ointment containing morphine, were introduced into the urethra, rapid and intense narcotism was produced; and so intense and constant is the phenomenon, and so out of all proportion to the quantity of sedative employed, that I deem it my duty to lay before the profession this remarkable circumstance, especially as I can find no mention of it in books, and as even in some, the absorbing property of the mucous lining of the urethra and bladder is denied. In April, 1857, having a patient afflicted with chronic inflammation of the urethra, and who suffered intense agony whenever an instrument was passed, I thought relief might be obtained if, instead of using oil, I smeared the instrument with morphine cerate. Accordingly I prepared an ointment containing twenty grains of morphine in every drachm of spermaceti ointment, and as the inflammation was confined to the membranous and prostatic portions of the canal, I smeared the last three inches of the instrument (a silver sound) only, with ointment, passed it and allowed it to remain about five minutes. The patient complained of feeling very sleepy, and the next day stated that he had hardly been able to walk home and as soon as he arrived there, fell into a profound slumber. Thinking this was simply a coincidence, I again applied the ointment in the same manner and a similar result ensued. Desirous of testing whether this was not an idiosyncrasy on the part of my patient, I passed a bougie, prepared in a similar manner, on myself, and to my surprise it had not been in my urethra three minutes before all the symptoms ordinarily produced by opium developed themselves.

On the twenty-third of May another opportunity presented itself for testing this singular property of the urethral membrane in a most striking manner, on a patient whom I will call A. I passed a bougie tipped with morphine ointment, consisting of a drachm of morphine to an equal quantity of cerate, and immediately after passed another smeared with an ointment consisting of ten grains of atropine to the drachm of cerate. The patient almost immediately, that is in less than a minute, became incapable of locomotion. I left him asleep in my office at nine o'clock, and on returning at two I found him still asleep and vomiting had taken place. It was not until four o'clock that he was capable of walking home, and then he staggered like a drunken man and complained of a numb sensation over the whole body. Two days after, he called on me and said that as he went home he heard many persons say he was drunk, but was unable to speak--was unable to answer any questions when he reached his boarding-house; as soon as he arrived there, went to bed and did not awake until twelve o'clock the next day. He says he still feels under the influence of the drug.

A few days after this another patient presented himself—a strong, robust, healthy man, with a remarkable muscular development. I passed a catheter tipped with a minute quantity of the atropine ointment, certainly not as much as would be equal in size to a pin head; the instrument remained in about five minutes; absorption was very rapid; even while it was in, I noticed that the pupils dilated and the voice became husky; in ten minutes, when about leaving my office, he complained of feeling drunk and numb, and walked with a staggering gait. He called on me the next day and said that when he got home he felt perfectly drunk, was unable to sleep, had constant confusion, sense of giddiness, inability to speak, his throat felt dry and parched, and was unable to see. This morning he feels perfectly recovered.

On the 28th of May I passed a silver sound on Mr. H., tipped with the morphine cerate. Drowsiness was produced in five minutes. He called on me the next morning, at half-past nine, to evacuate his bladder—paralysis of the bladder having occurred and retention since half-past three of the previous day, the period when the instrument was passed.

During the past winter one of our students, who suffered from stricture of the urethra, came to me for relief. As he was a very intelligent young man I considered this a very fair case for experiment, and having explained to him the nature of what I was about to do, obtained his consent to pass a bougie tipped with atropine, with the promise that he would analyze his sensations and furnish me with a written account of what he experienced. I append in his own words the result of the experiment: "On arising from the couch on which I was resting I experienced a sensation of giddiness in the head, with impaired vision, which passed off in a few minutes. These symptoms were accompanied by dilatation of the pupil. After walking a short distance these sensations returned and my gait became very unsteady. Dryness of the mouth and fauces now occurred, the secretion of saliva became almost entirely suspended, so much so that the use of tobacco would not reproduce it. These phenomena were followed by an intense desire to urinate with inability to do so. In this condition I remained six or eight hours, when I succeeded in getting to sleep. On awaking the following morning these effects had very nearly subsided, a little dryness of the fauces being all that remained, and this gradually passed off during the day." Such were the sensations produced on this gentleman by an almost infinitessimal quantity of the alkaloid applied in the manner I have indicated, and the phenomena commenced in five minutes.

Since this I have employed bougies smeared with morphine and atropine ointment in at least fifty cases, and in every instance the result has been the same; but as I was desirous that this singular phenomenon should be witnessed by some of the profession, I obtained permission from Dr. Choppin to pass a bougie, similarly prepared, on a patient in his ward, in the Charity Hospital. The patient was a man not suffering from any disease of the urethra, and I passed a No. 7 silver sound, smeared at its extremity with atropine ointment, into his urethra, in the presence of Dr. Choppin. Within three minutes from its introduction, as witnessed by Dr. Choppin and several students, the pupils dilated completely; in five minutes the phantasmal delirium, so characteristic of belladonna, was developed to such an intense degree as to necessitate mechanical restraint. All these effects were produced by a quantity of atropine so minute as to be almost inappreciable. The strength of the ointment was ten grains to the drachm the quantity smeared on the bougie did not weigh half a grain, equaling the twelfth of a grain of atropine. On withdrawing the instrument, apparently the whole of the ointment was adhering to it, and it is probable that not the fiftieth of a grain of the ointment had been removed or absorbed. This would reduce the quantity of atropine to an almost infinitessimal quantity, and I have not the slightest doubt that certainly less than the three-hundredth of a grain entered the system. But allowing even that the whole quantity was absorbed, it would still be inexplicable how absorption should be so rapid and the effects produced so powerful. The usual dose of atropine is about the twentieth of a grain, of morphine from the sixth to one grain; yet in no instances, administered either by the stomach or endermically, would their effects be comparable to those I have seen and just described, as when applied to the urethral mucous membrane.

There is also another circumstance I have observed, viz.: that this absorbing power is not common to the whole urinary tract. The bladder and the spongy portion of the urethra seem incapable of absorbing. I have injected three drachms of Battley's sedative into the bladder and allowed it to remain there, without perceiving the slightest general influence. At the same time, if we pass a prepared bougie only four or five inches into the urethra, we shall not perceive any narcotic influence however long it may remain there; pass it an inch, or inch and half further, and rapid narcotism ensues. It seems as if the prostatic portion of the urethra and the neck of the bladder were the only portions possessed of this absorbent property, and that the rest of the canal was inert. In order to test this, I injected into my own urethra a saturated solution of morphine, and held it for some ten minutes—of course it could not penetrate beyond the membranous portion—not the slightest narcotic effect was produced. The next day I passed also, on myself, a bougie, smeared at its distal extremity with morphine, to the neck of the bladder, and in three minutes absorption took place, accompanied by all the effects of a full dose of opium. If we take a bougie with its last two inches only, smeared with morphine, and pass it *rapidly* into the bladder, allowing its extremity to rest against its walls, but its coated portion being beyond the prostate and neck, no absorption takes place, however long it may remain there; withdraw it two inches, and now allow the morphine to come in contact with the neck, absorption again rapidly ensues. These circumstances, therefore, conclusively point to the fact, that this elective power resides only in a small portion of the urinary membrane—a portion bounded by the prostate and neck of the bladder—there is a possibility that the trigone may have some absorbing power, although this is doubtful.

With regard to the rationale of this curious phenomenon, I am at a loss—it is true that the part where I have located absorption, is the one most richly endowed with nerves and blood vessels—the hypogastric plexus of the sympathetic ramifying over the prostatic urethra and neck of the bladder. Here they are very numerous and of a large size, and, according to Valentin and Muller, have ganglionic swellings on various points. We have also a large congeries of blood vessels at this point, but the absorption and intensity of effects are so out of all proportion to what we observe when sedatives are applied to other organs, that I cannot hazard a theory. I have simply stated the facts as I have observed them, and shall leave it to others to frame a hypothesis to account for them. I would suggest that this property, which I have indicated might be found useful in certain cases of mania and delirium tremens, where we wish to get patients under the influence of a narcotic, and where they refuse to swallow. In these cases we can always pass a bougie smeared with atropine or morphine into the urethra, and I am convinced that speedy and full narcotism would be the result.

TREATMENT OF A CASE OF GUN-SHOT WOUND OF THE HAND, ILLUSTRATIVE OF THE EFFICACY OF COLD-WATER DRESSINGS, WITH THE INTERNAL USE OF LAUDANUM AND TART. EMETIC.

By G. E. Elmer, M. D., of Marksville, La.

Not long ago, a youth, between twelve and fourteen years of age, had the whole of the left hand, from the carpus to the fingers' ends, most violently injured, in fact, horribly lacerated and contused, by the accidental discharge of a large pocket pistol loaded with buck shot. The skin, palmar aponeurosis, muscles and their tendons, were all dreadfully mangled, exposing and leaving quite bare all the metacarpo-phalangeal bones and articulations. In a word, it was a most shocking gun-shot wound of the wrist and hand, such as is not frequently encountered in country practice.

Called to the case three hours after the accident, I immediately, and without much hesitation, suggested amputation, to which both patient and parents peremptorily objected. The only alternative left, was to endeavor to save the part by such local and general treatment as the exigencies of the case seemed to authorize or require. After clipping off what was "dangling," the ends of tendons and portions of skin and lacerated muscles, and applying a few sutures—no other applications were made to the wound, save pledgets of lint kept constantly saturated with cold cistern water, through a fine cotton roller carefully adjusted to the parts. On the second or third day after the first dressing, I removed the sutures, as tetanic symptoms were supervening, but continued the cold water applications to the wound. Nervous symptoms becoming more and more alarming, and traumatic fever well developed, I prescribed Graves' tart. emetic solution, and laudanum in camphor mixture in large doses. From the moment the patient commenced feeling the influence of the medicine, every alarming symptom ceased as if by a charm—the wound suppurated kindly and freely; healthy granulations sprang up; re-union of the divided parts took place, and in twenty days the patient completely recovered his health, with the use of his hand. This case exhibits too plainly the superior efficacy of cold applications to tissues laboring under traumatic inflammation, particularly from gun-shot wounds or accidents. The patient was of feeble constitution and of nervous temperament and habit, which gave a preponderance to nervous or irritative over arterial excitement; hence the good effect of opium in the case, modified in its action by a combination with tart. emetic, counteracting a state or condition of the brain and nervous system, which otherwise would have resulted in tetanus and death.

MARKSVILLE, LA., March 25, 1858.

REPORT ON CHEMISTRY AND TOXICOLOGY.

By I. L. CRAWCOUR, M. D., F. S. A., Professor of Chemistry and Medical Jurisprudence, New Orleans School of Medicine.

Poisoning by digitalis is so extremely rare, that the following account of an attempt at suicide by means of digitaline is interesting, not only from the great clearness with which the symptoms are detailed, as from the large quantity ingested, and which failed to produce death.

Madame X., a native of Paris, aged twenty-three years, and of a strong, robust habit of body, resolved to commit suicide, and on the 26th of June, at 7, A. M., swallowed six granules of digitaline; immediately after, she drank a cup of coffee, and then throwing herself upon a couch, waited the result. In two hours she was seized with a violent chill, accompanied by vertigo and incapability of moving. During the whole day these symptoms were present, and alternated with singular hallucinations. About 6 o'clock in the evening she concealed her indisposition, and forced herself to swallow nourishment offered to her, and among other things she drank another cup of coffee. Immediately after this, she was again seized with fresh chills, cold sweats and dyspnœa; warmth gradually returned, but there was great restlessness, insomnia and dyspnœa. About 1, A. M., being almost incapable of respiring, she rose, was seized with vertigo, and dragged herself with difficulty to an open window, where she remained until 3 o'clock. She then retired to bed; soon all the symptoms ceased, and she slept. She did not awake until 7 o'clock; was then free from any indisposition. Seeing that she had failed, she resolved to make another attempt at self-destruction, and swallowed forty more granules, and again laid down on the bed. In an hour the following symptoms supervened: hallucinations, vertigo, constant chills, cold perspirations, nausea, and frequent vomiting and colics. She became more and more feeble, and felt in vain for any pulsation at the wrist. In the evening the vomitings were more frequent, and intense debility succeeded them; she was unable to pass urine or stool; she was totally incapable of motion, or of uttering the slightest sound; it seemed to her as if her eyes were becoming too large, and were about to start from their sockets. This condition lasted all night and during the whole of the next day (the 28th) until 5 o'clock in the evening; about this time the vomitings ceased, and she was unable to speak above a whisper. During the night the debility increased; the chills, the hallucinations and the cold sweats still continued; every minute she experienced a deathly sickness, and expected to die; she was incapable of the slightest movement; with all this she did not lose consciousness, suffered a burning thirst, and was still incapable of passing water or of going to stool. The night passed in this manner; in the morning she experienced a desire to live, and when a friend came to see her, sent for a physician. Dr. Heer saw her on Monday the 29th. He found her stretched at full length on the bed, and almost motionless. The face was very pale, the eyes projecting enormously, the eyelids wide open, the complexion of a lemon-yellow color, and the pupil much dilated. The voice was almost extinct, the skin was cold and covered with perspiration, the pulse small, feeble, intermittent, ranging from 46 to 48 in the minute, and at times was unappreciable. Tho tongue was dry, white in the centre, red at the apex; much thirst and nausea and no appetite. The epigastric region was very painful, especially on pressure. For two days she had passed neither stools nor urine; the bladder was not distended. Dr. H. prescribed two laxative enemata, sinapisms to the extremities, frictions with camphor liniment and lemonade. In the evening the pulse was the same, the skin less cold, she was still incapable of moving, hallucinations constantly present, thirst intense, neither urine nor stools, the injections having been passed alone. Ordered to continue the frictions, the sinapisms, and every half hour pills, containing 3 grains each of calomel, jalap and aloes, and weak coffee for a drink. The next day the pulse was a little more frequent; she had been in a state of extreme agitation all night; complete insomnia and pain in the epigastrium. She had four stools in the morning. For three days no urine had passed, and the bladder did not seem distended. From this time she gradually improved, and in four days was completely recovered, still suffering, however, from slight debility.

This case is one of great interest, inasmuch as it gives a clear insight into the phenomena produced by excessive doses of digitalis, and also as showing us that recovery may take place even from enormous quantities, provided treatment is perseveringly kept up. Assuming that the granules were those usually dispensed, and which contain 1-50 of a grain of digitaline in each, this woman took, in the fifty-six she swallowed, over one grain of digitaline, equal in amount to about 130 grains of the powder of the fresh leaves. I believe there is no instance on record in which recovery has taken place after such a large dose. Taylor relates instances where much smaller quantities produced death, and we learn in our every-day experience how rapidly depression of the heart's action follows even a moderate dose of the drug. The dilatation of the pupils and the pain in the epigastrium is a constant sign. The suppression of the urine is remarkable as continuing so long without producing poisoning, although Christison relates a similar instance, where a woman swallowed by mistake ten ounces of a decoction made by infusing a handful of the leaves in a quart of water. In this case also there was suppression of urine for three days. Salivation, which is a very usual symptom, does not appear to have been present in this case of Dr. Heer's.

☞ Böttger has given us a new re-agent for manganese. He states that the minutest quantity may be detected by the chlorate of potash. In order to detect it, throw a small quantity of the material suspected to contain manganese into a test tube, which already contains the chlorate of potash in a state of fusion. After the combustion has entirely ceased and the tube is cold, a peach blossom residue will be left if there has existed the smallest quantity of manganese. By means of this re-action Böttger has discovered manganese in boxwood, beech, cork, in the iodine of commerce, tea leaves, and several articles of food.

SPECIAL SELECTIONS.

CLINICAL LECTURE ON INFLUENZA.

By W. T. Gairdner, M. D., Physician to the Royal Infirmary, and Lecturer on Clinical Medicine, Edinburgh.

I invite your attention to-day to a subject of great importance, and very directly suggested, not perhaps by any one case now in the wards, but by a combination of circumstances which you have witnessed during the last fortnight. It is to the prevalence of certain diseases in our hospital wards, which, taken collectively, amount to the proof of an epidemic morbid tendency; that is to say, which show, by the extent and manner of their diffusion, the existence of a morbid influence operating temporarily upon the population at large. I cannot, indeed, show you in the wards a single typical case of this epidemic disorder, as it is seen so frequently outside, unless it be that of the woman just admitted into the fever ward. But, although I cannot place before you the ordinary forms of the epidemic (because these are commonly too mild to be admitted into hospital,) I can show you its accidents and complications in sufficient number to furnish a text for some remarks on its nature and prevalence.

You may recollect that, at the beginning of the month, we had very few acute cases of disease, though there were many interesting chronic cases, chiefly of abdominal affections, and almost all of organic diseases. The few acute cases that we had were fevers, and these almost all of one kind, viz., enteric typhus, about which I may have more to say at another time. Now, on the other hand, the wards are crowded with more or less acute cases of disease; and most of these diseases are of the chest. Let me enumerate a few of them.

There is the case of the woman already noticed as having been admitted to the fever ward. She is a healthy-looking young woman, who has been occupied as a domestic servant. She was seized, a few days ago, with shivering, succeeded by headache, pains in the limbs, sickness. Along with these there was a certain amount of catarrh, which has now settled (not very severely, however) upon the chest. The fever is now intense, and very much out of proportion to the severity of the catarrh. Headache persists, the skin is hot, the tongue loaded, the color dingy, and the general aspect of the patient certainly goes far to justify her being sent to a fever ward. Nevertheless, I believe it will turn out not to be a case of fever, in the ordinary acceptation of the term; but of the current epidemic—which I will take the liberty of calling, if it has not already been called—INFLUENZA.

Had this been the first case of influenza presented to my notice, it might have passed for one of continued fever or typhus. But even then I should have remarked its singularly abrupt invasion, the great amount of prostration in this early stage, the extreme severity of the headache and articular pains, as being rather out of character in any fever to which we have lately been accustomed. Knowing what I do of other cases, I have no doubt these symptoms are owing to influenza. The only question is, whether this woman may have influenza and fever combined. This question must remain open for the present.

Now, by considering this case of catarrhal fever, or of feverish cold, (if you like to call it so,) in relation with the other facts to which I shall allude presently, you will draw for yourselves the picture of the epidemic, as we have it.

The first indication we had of anything out of the usual course was, perhaps, that downward tendency of several of our cases of phthisis, which, you will recollect, I remarked to you more than a fortnight ago. It does not always happen that cases of phthisis are the first to show a tendency to influenza, and in this instance, it may have been a coincidence; but it is a curious coincidence, that, when we had picked out four cases of phthisis as fit subjects for trying the new remedies—the hypophosphites of lime and soda—and had noted them carefully for that object, three out of the four should have been seized with acute symptoms, within a short period of our commencing the novel treatment. I told you at the time, that I had no reason to blame the remedy for this result, and that it was probably a mere coincidence; I am now disposed to believe that it was one of the first manifestations of the morbid influence of which we have since seen so much.

[Two of these patients have since died; one went out relieved; another survives, considerably enfeebled, but without acute symptoms.]

On the 11th of November, we saw together a case in the female general ward, of very old-standing chest disease, apparently emphysema of the lungs, in which acute symptoms had supervened, and the patient appeared to be in extreme danger from respiratory oppression, with feverishness and bronchitis. Under a very simple treatment, this woman is now improving; but her case is, no doubt, one of the epidemic in a debilitated subject.

Shortly before this case was admitted, a boy was brought to the waiting-room screaming with pain, which he referred to his left side. He was also very feverish. He had not much catarrh, but auscultation left us in no doubt that there was a degree of dry pleurisy on the left side, and also a friction sound, not so well marked, over the pericardium. Under moderate leeching and opiates, he was soon convalescent; but the respiratory friction sound continued loud and characteristic, and we have detained him in the ward mainly for your benefit. I had some doubts, at first, whether this boy had not suffered perforation of the lung; but it was not so. I do not say it was a well-marked case of influenza, but I mention it by the way.

The next case was that of a boy from the Industrial School, who had gone through a distinct attack of feverish catarrh before we saw him. The traces remained in the form of bronchitis of the smaller tubes, or rather, I suspect, a tubercular condition of the lung, with bronchitic signs. This boy has probably had an unsound chest for some time. He is better, however; indeed, nearly well.

About this time, I thought it right to pay a visit to the Industrial School, as I had seen several cases of feverish disorders from thence, which the head-master sent up for my inspection. I found thirteen or fourteen boys smartly ill with cold of the head or chest, and several of them plainly very feverish. Coughs resounded on every side; and squill mixture, with paregoric and ipecacuanha, were greatly in demand. None of the cases were, however, dangerous.

On November 14th, I directed your attention to a very acute case of bronchitis, or broncho-pneumonia, admitted two days before. The fever was very intense on admission, but had quite subsided before you saw the patient, under the treatment, by considerable doses of tartar emetic, employed by Dr. Yellowlee's from the commencement. The patient, a young girl of seemingly sound constitution, recovered rapidly—the large doses of tartar emetic being replaced by a simple cough mixture, with small doses of antimonial wine, after the lapse of about 48 hours; as soon, indeed, as the fever shows signs of retreating. No other medicine was required in this case.

Very different was the result of treatment, or rather of the neglect of treatment, in another case in the same ward. A young woman, the mother of a family, was seized with acute bronchitis, and lay many days neglected. She was then seen by Dr. Watson, who, after blistering the chest and administering some internal remedies, sent her into the hospital. In this case, seen by us only at an advanced stage, the fever had assumed a hectic character. Occasional flushes overspread the face; there was marked dyspnœa and lividity; sweating was very severe every night, and sometimes in the day; and prostration very considerable. She has since had acidulous drinks, antispasmodics and opiates, and is better; but her convalescence is very slow, fever is not subdued, and I greatly fear that the seeds of tubercular disease have been laid in this case. She flushes whenever she is spoken to, and is very nervous. [This patient was lately dismissed, as she felt it necessary to go home to her family; but she is very unfit for household duties, and will probably be so for some time.]

Two other cases of chronic catarrh, with acute exacerbation, were admitted into the male ward, and were seen by you on November 18th. Both of these were street-porters, and men above 60 years of age—by no means temperate in their habits. I will not, however, dwell upon them.

The same day, November 18th, brought under your notice, for the first time, two extremely interesting cases of acute disease, having the imprint of the epidemic tendency.

One of these was a case of acute pneumonia, or pleuro-pneumonia, in a previously healthy man of 28 years of age. The disease had run a course of many days previous to admission, having begun in symptoms altogether like simple influenza, succeeded, at the end of a week, by pain in the right side of the chest and difficulty of breathing, with shivering fits. We found the whole lower lobe on the right side more or less consolidated, the sputum rusty, and the fever considerable. The night of admission, before treatment had been well begun, pain occurred on the opposite (left) side, at the lower part; and this aggravation was attended with a pulse of nearly 140 in the minute, at one period, and with respirations between 50 and 60 in the minute. So soon, however, as the tartar emetic began to take effect, these symptoms subsided; and next day we noticed the pulse at 78, and the respirations at 32, the skin cool and moist, and the general state quite satisfactory; though a certain amount of dull percussion, with some consonating râle, existed at the lower part of the left lung, and the physical signs on the right side were unchanged. In another day, the line of dull percussion in the right lateral region was lower by an inch and three quarters, and from this period the convalescence may be said to have begun. The operation of the grain-doses of tartar emetic here was most prompt and satisfactory; and as the fever and dyspnœa have entirely subsided, the pulse being 72 and the respirations 26 in the minute, I am of opinion that we may now suspend the remedy, and leave the cure to be completed by nature. [The convalescence was uninterrupted. The patient left on 2d December, perfectly well.]

The other case which we saw for the first time on the 18th, was that of a woman in the fever ward. This patient, a domestic servant, aged 28, of rather corpulent habit, always enjoyed good health till a fortnight ago. At that time she was seized with pains in the head, back and limbs, with a feeling of lassitude and exhaustion, which confined her to bed for three or four days; but at the end of that time she was somewhat better, and tried to resume her ordinary work—to very little purpose, as in a day or two more she took to bed again, and has been feverish ever since.

I believe that this case is one of enteric typhus, or typhoid fever, as it is often called. I make this diagnosis, however, chiefly on the ground that certain rose-colored spots, which you saw me mark on the skin yesterday, bear a strong resemblance to the characteristic eruption of that fever. Should these spots continue to appear, we shall feel sure of our diagnosis; although there is at present *not a trace of abdominal complication, and all the more prominent symptoms are thoracic*, so that there is little doubt the epidemic tendency is showing itself strongly in this woman. There are, in fact, the following very formidable symptoms: great acceleration of the respiration; dingy lividity of countenance, with flushed cheeks; small and very frequent pulse; considerable pain in the chest, not localized; some delirium; and I have little doubt that some peculiar form of broncho-pneumonia is present, as there is scanty sputum, deeply tinged with purple blood, and we find, on examination, limited dullness on percussion, together with consonating respiration and râle in both backs, at the lower part of the lungs. It is easy to see in this case enteric fever, complicated with influenza, and with very serious, though ill-defined, acute diseases of the lungs—a very ominous conjunction, and all the more so as the debility of the patient forbids the employment of active remedies, and we must confine the treatment to regulated stimulation. I must say, that the state of this woman appears to me perilous in the extreme.

[The sequel of this case justified our fears. On the 21st she was visited, on account of my unavoidable absence from town, by Dr. W. Begbie, who marked out additional rose-spots, thus removing all doubts as to the diagnosis. The chest symptoms, however, still predominated; there had only been one stool, and that a natural one, since admission; and there was no pain or tenderness of the abdomen to any appreciable extent. On the night of the 21st a very loose stool was passed, containing blood. Another followed next day, and another the succeeding night, the blood being in large quantity, notwithstanding the application of ice to the abdomen, and the administration of acetate of lead, with opium, internally. On the 23d, at visit, she was manifestly sinking. She had been very restless and delirious, and had three other stools, largely composed of blood. The tongue was dry and brown, and the pulse almost imperceptible. There was no additional embarrassment of breathing, and I did not examine the back; but over the right front there was marked dullness on percussion, with feeble tubular respiration and consonating râle. She died on the morning of the 24th.

Post-mortem examination showed numerous enlarged and congested patches of Peyer in the ileum, in a state of ulceration and sloughing; with enlarged, congested and softened mesenteric glands. The lungs were in an extremely curious and almost indescribable condition; the right lung almost entirely devoid of air, flaccid, evidently collapsed, but showing throughout, on section, much conjestion, and here and there patches of hæmorrhagic condensation; the bronchi loaded with mucus deeply stained with blood. In the left lung there was a good deal of collapse at the base and root; but on the whole, not much disease. In neither lung was there anything like ordinary hepatization, and the pleuræ were quite smooth, and free from exudation. The spleen, as usual, was large and soft.

It is worth noticing, that the nurse of the ward, a most careful and attentive person, was under the impression that this patient was menstruating two or three days before death, and that the patient herself had a similar impression. The examination of the uterus and ovaries showed that this impression was erroneous. The mucous membrane was pale throughout; a gelatinous mass of mucus occupied the cervix uteri, and there was no recent corpus luteum. It is evident that the stains of blood from the bowels had led to a mistake in this particular.]

The only other case worth mentioning in illustration of the epidemic tendency, is that of a little girl, admitted on the 19th, as she had been several times before, on account of disease of the heart. She has, I think, a contracted mitral orifice; and with this there is associated, at present, a great deal of lividity, with feverishness, and marked prostration of strength; the consequences, no doubt, of influenza acting upon organs predisposed to disease. I should think badly of this case if I had not seen it before; but this girl has repeatedly got over attacks considerably worse than the present in a very short time. She had all that elasticity of constitution which appears to be the exclusive endowment of youth; and she is in every respect a very good and hopeful little patient. [She recovered in a few days.]

Let me now review these facts. Here, within the space of less than a fortnight, you have seen admitted into our wards (with an average population under 40) no fewer than 11 cases of febrile disease, associated with pulmonary symptoms of one kind or other. Most of these, no doubt, were complicated cases, and only one of them could be called simple influenza. But this is because simple influenza is usually too rapid and too mild a disease to be admitted to an hospital. We see here, not the disease, but the consequences and complications of the disease. In private and in dispensary practice we see the disease itself.

[Of these 11 cases of chest affection, 1 was double pleuro-pneumonia; 1 was pleurisy and pericarditis; 2 were very acute bronchitis, or broncho-pneumonia, in one with a probable tubercular complication; 1 was sub-acute bronchitis, certainly with tubercular antecedents; 3 were sub-acute bronchitis, supervening an old emphysema of the lungs; 1 was sub-acute bronchitis, supervening upon old valvular disease of the heart; 1 was enteric typhus, with very acute pulmonary complication; and 1 was influenza, pure and simple.]

While we have been watching these cases together, I have seen many and heard of many more cases of the simple and ordinary form of the disease. Not a few of yourselves have had it, and two or three have been seriously ill. Most of the cases that I have seen, however, have been remarkable for the sharpness and suddenness of the attack, and not less so for the rapidity of the passage from a state of feverish prostration to convalescence. I have found a man with a pulse of 130 at night, and next day he has been up and about. This, of course, only happens with sound constitutions. In one or two instances, it has appeared to me that an emetic, given in time, has anticipated or cut short the attack. Certainly it has been followed by great relief. For the rest, the bed, or, in mild cases, the sofa, restricted diet, laxatives where required, and liberal doses of opium where there is much restlessness and exhaustion, seem to me to comprise all the necessary treatment of ordinary cases of influenza, even when severe. In the complicated cases no rule can be laid down. Some are very amenable to remedies, others run their course in spite of treatment. You have seen illustrations of both kinds in these wards.

The most characteristic symptoms of influenza are intense feverishness, usually with great tendency to chilliness or shivering, until the patient takes to bed, and reaction is fairly established. Then come racking headache, with pains in the back and limbs, which sometimes constitute the principal source of suffering; extreme sensation of debility; total prostration of appetite, with less of thirst than is usual in fever; and with these, coryza or mild catarrh, bronchitis, broncho-pneumonia, as the case may be. But though catarrh is frequent, and may be severe, the disease is essentially a fever, not a catarrh. Nay, the catarrh may be absent, or insignificant; not infrequently it is so. In one of the cases I saw among yourselves, there was absolutely no catarrh; in another it was very slight. And I saw two very curious cases a few days since, which enable me to put this point yet more strongly. The catarrh may, in fact, be absent in the very case in which you would *a priori*, expect its occurrence. A gentleman, who has been long afflicted with spasmodic asthma, with intervals, however, of fair good health, and with no appreciable organic disease of the chest, came to me after he had been struggling for several days with debility and prostration, with chilliness and feverish sensations. These were with him the only manifestations of influenza. [He afterward, at an interval of ten days, had a slight cold in the head, without fever; in the meantime his whole family sickened with fevorish colds, some of them with chest affection, from which he himself remained exempt throughout.] In another case, a gentleman, who also suffers from habitual asthma and bronchitis, and in whom I suspect a morbidly enfeebled heart, sent for me in a great hurry on account of the alarming prostration, produced by this strange and inexplicable "influence." He was, however, more frightened than hurt; in a couple of days he was convalescent, and the amount of bronchitis in his case never gave me the slightest uneasiness.

Even the complications in influenza are not always of a catarrhal kind, nor even confined to the chest. Ten years ago, in connection with a great and general epidemic of influenza, I witnessed in this hospital a succession of cases such as I have never seen since that time. In the course of a few weeks there occurred, I forget exactly how many, but upward of half a dozen cases of inflammation of all the great serous membranes conjointly—double pleurisy, pericarditis, peritonitis. Most of them were fatal; indeed, they seemed to come into the house only to die; so rapid, so incontrollable were the symptoms, that no time was given for the application of remedies, even had remedies been clearly indicated.

It is somewhat remarkable, that the great epidemic influenza of 1847–8 began at the same time of the year with the present one, almost to a week. You will find an account of it in the excellent monography of Dr. Peacock, of London.* That epidemic, however, came upon a population wasted by typhus and other forms of fever, and not yet recovered from the famine and destitution caused by the blight of the potato, and the high price of grain in 1845–6. Scurvy, dysentery and fever preceded the influenza on that occasion, and cholera followed not very long after. Notwithstanding the recent money-crisis, and the distress likely to follow among certain classes of the working population, we may hope that we are at present more favorably situated than we were ten years ago. A short time will show whether the present epidemic is to bear comparison with the last or not. Hitherto it has been of a very mild character, comparatively speaking. I have myself seen only one fatal case—a man who had been for some time in poor health, and who died of a chest complication, not very unlike that of our case of enteric fever. I do not know, indeed, that this can fairly be called a death from influenza, though I believe influenza to have been mixed up with the fatal result.

(*From a Clinical Lecture on Friday, November* 27, 1857.)

Since I spoke to you about influenza a week ago, there have been only two additions to the list of acute diseases which appear to have had their origin in it—one, a case of pleuro-pneumonia, admitted only ten days ago, treated both before and after admission by calomel and opium, and already in process of resolution; the other a case of genuine influenza, with all the usual symptoms, and which, like the former one, was sent up to the fever ward, as lying under suspicion. I have directed her to be put in the closet, apart from the other patients; and we shall make a point of parting with her as soon as possible. So far as the wards are concerned, the epidemic does not appear to have made rapid progress this week.

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* On the Influenza, or Epidemic Catarrhal Fever, of 1847–8. London, 1848.

I have received the Registrar-General's report of mortality in London for the week ending November 21st. It is worth while to compare the indications in this report with those derived from our own observation as regards Edinburgh. For this purpose I have drawn up a table of those diseases whose mortality appears to be notably above the average of the season, and have calculated the existing mortality as against the corrected average of ten years. The correction I speak of is made thus: The Registrar's table gives the mortality of each disease during the forty-seventh week of the present year, and during the corresponding week of ten previous years; from these he deduces an average, which occupies a separate column. But before you can use this average as against the number of the present year, you must in every case raise it by one-tenth, to make allowance for the increase of population, which, it is calculated, increases by one-tenth in five years.

Now, the past week has, in London, been one of unusual mortality *for the season;* seeing that the corrected average for ten years makes the total mortality of the forty-seventh week of the year, 1211; while during the past week it has been 1382. This very considerable extra mortality appears to be due chiefly to bronchitis, pneumonia and phthisis, to which may be added whooping-cough. All of these are 20 or more in excess of the average mortality of the season; and bronchitis is in excess by the very large number of 123, showing a mortality much more than double the corrected average of the ten years. These four diseases together have a mortality 188 in excess of the average; while the entire excess of deaths for the week is only 171; the difference being, of course, made by diseases which are below the average, especially typhus, scarlatina and smallpox, which have at present a low mortality. The other diseases which, though to a smaller extent, have contributed notably to raise the mortality of the past week above the corrected average, are croup, (with which I have included laryngitis,) scrofula, (the disease of the young,) and apoplexy, with paralysis, the diseases of the aged; to which we may add that somewhat vague condition called atrophy (mostly infantile,) and that still more vague cause of death called age. Both of these are considerably in excess; and these, with the other causes stated, go to show that the mortality of the past week in London has fallen heavily on the two extremes of life. This, indeed, is always the case with influenza.

But are we justified in assuming the existence of influenza as a cause of death in these cases, especially when we look to the fact that not more than 9 deaths are recorded in all London, during the past week, as having occurred from influenza? I think we are; because we may be sure that an epidemic condition which raises the whole mortality by one-seventh, which more than doubles the deaths from bronchitis, and largely increases those from other acute diseases of the chest, while the aged and the young, the apoplectic, paralytic and consumptive suffer out of proportion to the rest of the population—such an epidemic condition, I say, has essentially the characters attributed to influenza, by whatever name it may be called. The small number of deaths under the special head of influenza, therefore, is only one proof out of many that the Registrar-General need not have been at the trouble of making a separate class of what he calls zymotic or epidemic diseases. The epidemic tendencies of a given period must be sought, not in any particular class, but in an intelligent consideration of the whole mortality list. Medical men are slow to report a death from influenza when it can be properly placed under any other title. It is, however, the fact (as I know from other sources,) that influenza has been unusually prevalent in London.

Table deduced from the Registrar-General's Returns (London) for the week ending November 21, 1857; *showing the rate of mortality in the forty-seventh week of the year* 1857, *in regard to those diseases which are above the corrected average of the same week for ten years:*

	Average Mortality.	Actual Mortality.	Excess.	Excess per cent.
Whooping cough	33.5	53	20	58
Croup and Laryngitis	13.4	26	13	94
Influenza	3	9	6	...
Scrofula	6.5	13	7	...
Phthisis	137.6	159	21	15
Apoplexy	25.6	33	7	29
Paralysis	22.7	31	8	36
Bronchitis	103.6	227	123	118
Pleurisy	2.6	7	4	...
Pneumonia	104.2	127	23	22
Atrophy	30.6	38	7	24
Age	49.6	57	7	15
All causes	1211.4	1382	171	14

Additional Remarks, Dec. 19.—The epidemic mortality in London appears to have attained its culminating point in the week ending December 6, in which the mortality from all causes was 1428; from bronchitis 242, from pneumonia 129, and from phthisis 168. Considered with reference to the season, however, this mortality is by no means so much in excess as that indicated in the above table; and we may therefore possibly conclude that the epidemic has passed its maximum in London. The next week shows a considerable decline. It is worthy of remark, that all the gentle hints and solicitations of the Registrar-General in the Weekly Reports, have not succeeded in raising the cypher of influenza above 22. In the year 1847, the stated deaths from influenza for the corresponding week were 198; those from bronchitis 343, from pneumonia 306, and from all causes 2,454. The epidemic of 1847–8 was, therefore, immensely more fatal than the present one, so far, at least, as we have hitherto gone.

It appears from the returns of the Registrar-General (London) for the quarter ending September, 1857, that the mortality from acute diseases of the chest was considerably below the average during the past autumn. It began to exceed the decennial average, however, in the month of October; and during the latter week of that month and the beginning of November, the increase was considerable, although not such as to give a decidedly epidemic character to the mortality. It was only in the second week of November that the total mortality began to be decidedly in excess of the decennial average.

In Scotland, the Registrar-General's returns show a very large advance on the mortality from pulmonary diseases during the month of November, 1857, as compared with the preceding month. Thus, in October, the deaths from bronchitis in the eight principal towns of Scotland were only 76, while in November they were 151, or *almost exactly double.* Pneumonia, in the same period, increased from 53 to 76; while phthisis has only advanced from 212 to 228. The increase, as regards bronchitis, is most marked in Aberdeen, next in Greenock, next in Dundee, next in Glasgow, and next in Edinburgh. Influenza scarcely appears in the returns, numbering only 3 in October and 7 in November.

The weather was, on the whole, fine in November, and not very dissimilar from that of the preceding month. The barometric pressure was somewhat higher than in October, viz: 30.143 inches against 29.817 inches. The mean temperature was nearly six degrees less, viz: 45°.1′ against 51°.0′. The rainfall was somewhat greater, and there was somewhat more of easterly winds. It is worthy of remark, that the mean development of ozone, as tested at Greenock, was decidedly less in November than in October.

It would be interesting to know to what extent the inland districts of Scotland have been affected with influenza, and whether its manifestations have been simultaneous with those in the cities or not. From circumstances which have incidentally come to my knowledge, I am inclined to believe, that in some places in the neighborhood of Edinburgh the appearance of influenza, in an epidemic form, was considerably later than in the city itself.—*Edinburgh Med. Jour. Boston Med. and Surg. Jour.*

EDITORIAL AND MISCELLANEOUS.

The Free Dispensary of the New Orleans School of Medicine.—In our March (1858) number we gave a summary of the medical, surgical and obstetrical statistics of the Charity Hospital, for the year 1857. We then announced that a sum total of 8,897 patients had been treated in this great institution in the year, and at a cost to the State of $84,803 11. Our readers know that the location of the New Orleans School of Medicine is such, that this unlimited field for clinical observation is at the very door of the institution, and both Faculty and pupils have the freest access. To the casual observer it would seem almost superfluous to furnish a student of medicine any more clinical material than can be found every day in this vast sick-house ; yet, there is a long list of ailments to which poor mortality is subject, which give infinite suffering, but which the poor laborer must, from necessity, endeavor to endure and resist as he toils for his daily bread. In other words, while one-half the sick world can afford, through the means of money or hospitals, to lie down quietly and let nature and medicines restore them to health, the other half are forced to fight disease and starvation at the same time, by applying remedies while they toil for food and raiment for themselves and their dependent families. In all large cities in this and other countries, experience has clearly proven to the benevolent that the best, and the only way of providing for this latter half is by the establishment of Dispensaries, at the doors of which the poor sick receive both advice and medicines gratuitously. All over our Northern cities free dispensaries are thickly scattered, and they are liberally supported by private subscription; and the rich man has long since found out that, while hospitals are indispensable for the accommodation of a vast number of poor sick, who, from the nature of the circumstances which surround them, either can or must lie down during treatment, still, far the greater portion of the indigent sick would not only bitterly suffer for aid, but the aid which is requisite can be far more cheaply furnished them in this way. As evidence of the truth of this proposition, we have only to adduce a few statistical facts, taken from the reports of some of these Northern institutions.

In the year 1853 there were treated by the "New York Dispensary" 46,338 patients, of which number 7,188 *were treated at their homes*, and the remainder under the roof of the institution. This was done at a cost of $4,953 63, or less than 10 cents each—of course, granting that 3,202 were vaccinated, and $6,674 were cases of minor surgery.

In the year 1856 there were treated by the "Northern Dispensary," of the City of New York, 11,797 patients, at a cost of $4,461 80, or about 37 cents apiece. In this institution there are two District Physicians, (who are paid $600 per annum each,) one Vaccine Physician, ($200 per annum,) one Apothecary, ($600,) and one Assistant Apothecary, ($200); making in all $2,200 paid to these officers, against $2,894 45 paid same officers in New York Dispensary, together with four additional District Physicians and two House Physicians.

In the year 1853, the "Demilt Dispensary," of the City of New York, treated 9,006 patients, at a cost of $4,907 37—the institution paying its officers, as above-mentioned, $2,625 77.

Now, we have here an aggregate of 67,140 patients treated, at an aggregate cost of $14,322 80, or about 20 cents the patient—and this in institutions paying, not only their apothecaries, but their physicians. Let us compare this with the cost of treating patients in an hospital, as shown by our data in relation to the Charity Hospital of this city.

In the Charity Hospital, 8,897 patients were treated during the year 1857, at a total cost of $84,803 11, or *over* $10 *apiece*. Is not the difference remarkable? Yet we do not mean to infer that there should be no hospitals: on the contrary, they are a positive necessity in every large city. But we do say, that there are a vast number of patients who enter hospitals, (where no dispensaries are to be found,) and are treated at a cost of $10 apiece, the nature of whose ailments is such that they could readily be treated at dispensaries at the reduced cost of 20, or even 50 cents each.

But we took up our pen for the purpose of directing attention to the vast importance of the Free Dispensary of the New Orleans School of Medicine, both as a field for clinical instruction and as an eleemosynary institution. On the 21st of January, 1857, the doors of the Dispensary were thrown open to the indigent poor, without regard to residence, nativity, or reputation, and up to the present date (April, 1858,) 4,500 sick persons have partaken of its benefits—and all those who render professional services, do so without pecuniary remuneration, the Apothecary alone being paid for his services. In all other cities, dispensaries are supported, as we have said, by private contributions amongst the citizens generally; this institution is not only conducted gratuitously, by the Faculty of the School of which it is an integral part, but is also entirely supported by them.

As a field for clinical observation, it needs only to be seen to be appreciated. Every variety and shade of disease is to be found around the prescribing tables, and in the persons of men, women and children. It is in such an atmosphere as this that the real student learns to recognize disease, and it is this tact of recognition of disease that the young physician requires. A man may readily know all about a disease as described in the books, but it is another thing to be able to diagnosticate it when thrown in contact with it. This tact is a part of the *art* of our profession, and it can only be acquired by being thrown in contact with disease—and every practicing physician knows, that the great difficulty we all have to contend with, is not how to *treat* a disease, but how to establish its true nature.

The Free Dispensary of the New Orleans School of Medicine is not only a fertile field for clinical observation, but it is one of the noblest charitable institutions to be found in our land.

The Board of Health Controversy.—In our April number we acknowledged the receipt, through the courtesy of Dr. A. Mercier, the author, of a pamphlet entitled "Review of the Annual Report of the Board of Health to the Legislature of the State of Louisiana, January, 1858; being a Memorial addressed to both Houses of the Assembly, now in session at Baton Rouge." We have now to acknowledge the receipt of a pamphlet entitled "Reply of the President of the Board of Health to the 'Memorial to the Legislature,' published in the New Orleans Medical and Surgical Journal, of March, 1858."

Having no desire whatever to take part in the unpleasant controversy discovered in the leaves of these two pamphlets, we shall only notice them to that extent which the interest of our readers would seem to demand.

The author of the Memorial makes the following points: 1st. Yellow Fever originates in New Orleans. 2d. Yellow Fever is not contagious. 3d. New Orleans cannot be quarantined. 4th. The Board of Health, in their anxiety to establish the propriety of quarantine, have suppressed facts in relation to the origin of Yellow Fever in our city last summer, which, if promulgated, "would have upset the edifice it was trying to build."

As to the first point, we believe, with the author, that Yellow Fever can and does originate here, and that it did originate in this city last summer. This is a very interesting question indeed, and one which merits the very closest attention. The author seems, however, to have introduced it merely as a weapon of attack on the Board of Health. But it is certainly a harmless weapon, as not only is the proposition undenied by the Board, but the fact of a disease being known to originate, even frequently, in a place, is no reason why, if it is believed to be contagious or infectious, efforts should not be made to prevent its importation from abroad. All the exanthemata have a beginning somewhere; so far as we know, they can originate anywhere; and yet self-preservation prompts us to bar them out of our houses, because they are believed to be contagious, or infectious, or whatever we may choose to call it.

We are surprised at the peremptory style of the author on the second point. He says: "Notwithstanding what the Report says, the opinions of physicians, North and South, did not change with regard to the non-contagion of yellow fever, and there is not a single member of the profession (?) whose convictions have been altered by the epidemics of 1853–4 and 1855."

Like humanity in general, we believe ourself to be fallible in judgment, and perhaps we do not know how to translate the many strange freaks of yellow fever in 1853–4, outside the city of New Orleans. Previous to those years, we had seen yellow fever in New Orleans, and we had endeavored to familiarize ourself, to some extent, with the literature of the disease. All that we saw and read of it tended to make us a *non-contagionist;* but the evidence furnished by the two years above named, forces us to believe firmly, that yellow fever is, under certain circumstances, strictly communicable from one person to another, and that the introduction of a case into a healthy community, may be followed by an epidemic of yellow fever—the case introduced being the exciting cause, the epidemic the effect. We are *one* physician, then, whose views have been totally changed by the occurrences of 1853–4 ; and if the author will but sally forth from amid the brick and mortar of this great city, and interchange ideas with the medical men of the country, from New Orleans to Memphis, he will find hundreds who have undergone the same change as ourself.

But the author is good at special pleading. He would ignore the more positive evidence of an epidemic following quickly on the introduction of a case into a healthy community, and lays great stress on the altogether negative circumstance of the introduction of a case being followed by no such epidemic. We all acknowledge measles and scarlet fever to be communicable, and because the human testimony on the subject is ample; and yet, who dares to say that the introduction of a case of either of these diseases into a healthy community will surely be followed by an epidemic? On the contrary, how often does the reverse occur? And do we not frequently know individuals to sleep with these diseases, for the purpose of contracting them, and yet go unscathed? And yet, what man will dare argue from such premises that measles and scarlet fever are not communicable?

The third point is; that New Orleans cannot be quarantined. Well, we believe that, in all human probability, New Orleans cannot be effectually quarantined; but here is only an opinion—a shadow. Will as many such opinions as could be expressed on paper in the next year, be half as satisfactory evidence to the mass of the people, as absolute experiment itself? Is it not ten times better, then, to exert our energies in the other direction, and try to have the quarantine experiment as complete as possible? Talk about the expenditure of money! Why, what is money made for, if it is not to buy knowledge, experience? And what experience so highly appreciated as that for which we pay the highest price in money? If the State did not pay out money for the quarantine, she would do something worse with it. Again, it will not do to merely *assert* that the quarantine tax, etc., acts injuriously on our commerce: if it be true, it can readily be proved by reference to statistical data, and nothing short of this sort of proof can be satisfactory.

Our own private opinion is, that New Orleans has never been, and cannot be, properly quarantined; but we earnestly desire to see the fairest efforts made to effect that which others believe to be difficult, yet altogether practicable.

As to the fourth point, we think that the author fails to make out a clear case against the Board of Health, and we think the defence of the President of the Board of Health very satisfactory. To our mind, the three cases occurring in June, July and August—there being an interval of forty days between the first and second, and then twenty days between the second and third—do not appear, from the record furnished, as genuine yellow fever, and if we had been President of the Board of Health, although we should certainly have mentioned them in the report, we should have said so. The very case on which the author of the Memorial lays greatest stress—it being the case he himself treated and examined after death—excited doubts in the mind of Dr. Chaillé, the associate proprietor of the author in the hospital where the man died; for the President of the Board of Health says Dr. C. told him more than once, "that he did not consider it a case of genuine yellow fever, but if it were to occur during an epidemic, it would unhesitatingly be called a case." Then, again, the President himself saw the case, and did not think it yellow fever. In the name of common sense, then, why should he report it as such, in spite of his conviction to the contrary? Certainly, as the matter stands, the Board are exonerated from the charge of willful suppression of facts in relation to the history of the appearance of yellow fever last summer. As an editor of a medical journal, we know full well how difficult it is to get at a true history of yellow fever cases here, and we do not envy the President his arm-chair. It is an easy matter any day to hear of cases in all parts of the city, but when you visit them it is not so easy to recognize yellow fever. Who has not witnessed the annual farce at the Charity Hospital? Here lies a case of jaundice. The attending physician thinks it is yellow fever season, and does not care particularly whether yellow fever is here or not. The case is marked down, "yellow fever." In the opposite ward lies a case of genuine *vomito*—the patient is yellow, he has suppression of urine, he is throwing up the "coffee-grounds" by the pint—even his gums are bleeding freely—he dies. "But it is too early for the yellow fever," says his physician.

Or, "it can't be yellow fever, as there are no other cases." Or, worse than all, "it is not yellow fever; the community should not be frightened with such a report."

Truly, who shall decide, when doctors thus disagree?—B.

SILVER SUTURES.—We notice elsewhere the receipt of this interesting pamphlet. Dr. Sims has at last published to the world the history of his success in operating for vesico-vaginal fistula. But, better than this, he at last, not only acknowledges, but positively urges on the profession the fact, that his success is not to be attributed to clamps, shot, or buttons, but to the silver wire used as a ligature. It is a very interesting and instructive fact, that Dr. Sims should have attained the highest eminence in the estimation of the profession, and they all the time awarding him his distinction on a wrong basis. Several years ago we said to a friend, that all the success in these operations was to be attributed, primarily, to the silver wire used as a ligature, and all experience in the treatment of these cases since, has tended to corroborate our opinion. We think too much praise cannot be awarded Dr. Sims for his great triumph in surgery, and we are surprised to find in his pamphlet that he has found it necessary to defend his discovery against those he thinks disposed to rob him of his fair fame. For the honor of the profession, we trust that his suspicions may prove groundless. Let what will be done towards perfection of this important operation by others, as long as silver wires are used as ligatures, (and more especially when the perforated shot is used in conjunction,) other things amount to but *modifications,* and the operation should still be called Sims'.

The reader is both instructed and amused by this pamphlet. Dr. Sims defends himself with unusual vigor, and, as if at last in very desperation, he proves that his own clamps and his own shot are totally worthless, in order to prove that the "button" is the same. He strips clamp and button naked, and then scourges them without mercy; while he demonstrates clearly, that the whole principle and secret of success lies in the little silver thread.

NEW EXCHANGES.—We are at last in receipt of Brown-Sequard's new journal of physiology, under the title: "Journal de la Physiologie de l'Homme et des Animaux. Publie sous la direction du Docteur E. Brown-

Sequard." If we are to judge from the number before us, it will certainly be a great acquisition to the periodical medical literature of the age. Physiology, of all branches of medicine, is least cultivated by the mass of the profession, and yet it must strike the thinking man as the very hinge on which the door to successful practice hangs.

The number before us is replete with valuable matter, and we shall make it a point to give our readers translations of all we may think will particularly interest them.

We are, also, in receipt of the first number, (April, 1858,) of the Oglethorpe Medical and Surgical Journal, published under the auspices of the Faculty of the Oglethorpe Medical College, at Savannah, Georgia, and edited by H. L. Byrd, M. D., and Holmes Steele, M. D., Professors in the institution. The mechanical execution of the number is excellent, and if all future numbers equal this one in the value of their contents, the enterprise will succeed—that is, if the proprietors can prevail on Southern physicians to pay their Southern dues as promptly as they pay Northern publishers of medical journals. We exchange with pleasure.

BOOKS AND PAMPHLETS RECEIVED.

"Outlines of a Course of Lectures on the Principles and Practice of Surgery, delivered by E. Geddings, M. D., from notes taken during the course. Published with the consent of, and revised by Professor Geddings. Charleston: S. G. Courtenay & Co., Publishers, No. 9 Broad street, 1858." We are under great obligations for a copy of this valuable book. Ever since we first read Watson's Lectures on the Practice of Physic, we have had a decided taste for medical works of this class. The published lectures of any teacher of eminence are more valuable than any regular text book he may undertake to issue, and if these lectures are delivered in an informal, (even extemporaneous) style, they are all the more valuable, as the author is then most apt to give all that is original with himself. We have not had time to peruse the work before us at all carefully, but just where we dash into it we find it most interesting, and no doubt it is a valuable contribution to surgical literature. Professor Geddings is widely and favorably known, and his opinions are valuable to us.

"Lectures on Sulphate of Quinia; delivered in the regular course of the Medical Department of the University of Michigan, by A. B. Palmer,

A. M., M. D., Professor of Materia Medica, Therapeutics and Diseases of Women and Children. Published by the Class." A very interesting series of lectures.

"Silver Sutures in Surgery. The Anniversary Discourse before the New York Academy of Medicine. Delivered in the new building of the Historical Society, on the 18th November, 1857. By J. Marion Sims, M. D., Surgeon to the Women's Hospital. (Published by order of the Academy.) New York: Samuel S. & William Wood, 1858." From the Author.

"Annual Report of the Board of Managers of Philadelphia Lying-in Charity and Nurse Society, for the year 1857, with a list of officers for 1858." To our esteemed friend, Dr. Ellwood Wilson, of Philadelphia, we are indebted for a copy of this interesting Report.

"Dysentery: its Pathology and Treatment. Clinical Lectures delivered at Jackson Street Hospital, by Robert Campbell, A. M., M. D., Demonstrator of Anatomy in the Medical College of Georgia, etc." From the Author.

"Diseases of the Teeth: a paper read before the Cook County Medical Society at Chicago, and published at their request. By W. W. Allport, Dentist."

"Proceedings of the Pathological Society of Philadelphia, 1858." From our friend, Dr. E. Wilson.

"The Transactions of the New York Academy of Medicine. Instituted, 1847; Vol. 2, Part 2. Printed for the Academy. New York: 1858."

REPORT OF DEATHS IN THE CITY OF NEW ORLEANS, FROM DEC. 20TH, 1857, TO APRIL 18TH, 1858.—*From Dec. 20th to Jan. 24th.*—Men, 113; women, 67; boys, 77; girls, 84. Adults, 180; children, 161; males, 190; females, 151; colored persons, 46. Total, 341.

Under one year, 98; one to two years, 17; over 60 years, 23.

Consumption, 42; Stillborn, 26; Yellow Fever, 3.

From Jan. 24th to Feb. 21st.—Men, 120; women, 64; boys, 93; girls, 85. Adults, 103; males, 213; females, 149; children, 178; colored persons, 71. Total, 361.

Under one year, 104; one to two years, 12; over 60 years, 22.

Consumption, 42; Stillborn, 30.

From Feb. 21st to March 21st.—Men, 146; women, 66; boys, 116; girls, 95. Adults, 212; children, 211; males, 262; females, 161; colored persons, ——. Total, 423.

Under one year, 134; one to two years, 13; over 60 years, 17.

Consumption, 55; Stillborn, 23.

From March 21st to April 18th.—Men, 107; women, 67; boys, 110; girls, 118. Adults, 174; children, 228; males, 217; females, 185; Total, 402.

Under one year, 133; one to two years, 27; over 60 years, 22.

Consumption, 64; Stillborn, 24.

LIST OF PAYMENTS TO APRIL 28TH, 1858.—Drs. G. E. Elmer, $5, vol. 5; W. Breedlove, $5, vol. 3; C. Langenbecker, $5, vol. 5; Richard Fowler, $5, vol. 5; Mr. Thos. Teare, $5, vol. 5; Drs. John Carter, $10, vols. 3 and 4; S. H. Rushing, $5, vol. 5; C. Johnson, $5, vol. 4; F. M. Phillips, $5, vol. 4; N. C. Smith, $10, vols. 3 and 4; T. T. Russel, $5, vol. 4; J. M. Baylis, $5, vol. 3; John Morris, $10, vols. 3 and 4; Mr. Samuel Matthews, $10, vols. 3 and 4; Drs. L. W. Chamberlain, $5, vol. 5; McCormick, U. S. A., $13, vols. 2, 3 and 4; C. R. Smith, $5, vol. 5; D. R. Cole, $10, vols. 4 and 5; C. T. Chamberlin, $10, vols. 4 and 5; A. Holt, $10, vols. 4 and 5; M. C. Boyse, $5, vol. 4; Wm. C. McGehee, $10, vols. 4 and 5; E. Doumeing, $5, vol. 5; Hinckley, $10, vols. 3 and 4; P. C. Boyer, $5, vol. 5; W. L. Shelby, $3, balance on vol. 4; Mr. C. F. Hamer, $10, vols. 4 and 5; Drs. M. Hebert, $10, vols. 3 and 4; J. Blevins, $21, vols. 1, 2, 3, 4 and 5; F. A. Bates, $8, in full, vols. 4 and 5; W. T. & J. Webb, $5, vol. 4; J. H. Jones, $5, on account; B. F. Barry, $10, vols. 3 and 4; J. G. Forster, $5, vol. 4; A. Donald, $10, on account; G. A. Ketchum, $10, vols. 3 and 4; F. B. Coleman, $5, vol. 5; S. R. Oliphant, $5, vol. 5; Thomas M. Matthews, $5, for vol. 5.

LAWS RELATIVE TO THE DYNAMICAL PHENOMENA IN MAN AND ANIMALS.—*By Dr. Brown-Sequard, of Paris.* Translated from the Journal de Physiologie, for N. O. Med. News and Hospital Gazette. By A. Peniston, M. D., Prof., etc.

The following laws are based upon an immense number of facts, observed either by myself, or collected from many different works, pamphlets or journals. These laws, throwing light, as they do, on many of the functions of animated beings, are here collected together for more convenient reference; especially as being the foundation of a system of general physiology. I am free to confess that many physiologists, and among others Fontana, Delaroche, Reil, Adamucci, Broussais, J. Müller, J. Paget, and W. Carpenter, have either proved or hinted at the existenceof these laws which I now formulate :

First Law.—Nervous actions, muscular contractility; the electric shock of certain fishes, the galvanic currents of certain organs, the galvanic discharge which accompanies muscular contraction, and, *probably* also the phosphorescence of certain animals, the ciliary motion, are all phenomena connected with certain organic changes dependent upon the nutrition of tissues, their waste and repair.

Second Law.—The faculty of producing these phenomena is in direct proportion to the rapidity of the circulation, the abundance of blood, and the amount of its entire nutritive materials.

Third Law.—During *rest*—that is to say, during the intermisssion of these functions—each faculty has a tendency to increased development.

Fourth Law.—This development is much more rapid after the exercise of organic functions than after prolonged rest; provided the requisite nutrition be maintained.

Fifth Law.—The faculty of producing the above-mentioned phenomena depends upon the peculiar structure of the tissues in which they manifest themselves; so that if these tissues remain long inactive their nutrition, and consequently their structure, become altered; the result of which is, that the faculty may either be impaired or entirely lost.

Sixth Law.—The faculty of producing these phenomena increases in proportion to the amount of rest, up to a certain limit, after which it decreases in proportion to the duration of this inactivity.

Seventh Law.—Many of the tissues which produce these phenomena are never absolutely at rest, and the phenomena manifest themselves as if spontaneously; their energy being in proportion to the elevation of temperature.

Eighth Law.—The faculty of producing these phenomena diminishes while they are taking place in inverse proportion to the rapidity of their manifestations and the length of time during which they have already lasted, but in direct ratio to the energy of nutritive process.

Ninth Law.—As nutrition is incessantly repairing the waste of functional activity; these phenomena may never completely disappear so long as circulation and respiration continue, for they furnish the materials and stimulus to renewed action.

Tenth Law.—As the loss arising from functional activity is followed by increased energy of nutritive repair, it results that if the action be frequently renewed there may be an excess of nutrition, and a considerable increase of functional activity.

Eleventh Law.—As nutrition may take in many tissues, without the circulation of nutritive materials, provided they already contain a certain quantity in reserve, the activity of these organs may still continue, even in the absence of the circulation.

Twelfth Law.—Although circulation, and consequently repair, are more active in summer than in winter, the functional activity of organs may be greater in winter than in summer, because there are then fewer spontaneous losses, and fewer also dependent either upon the will or external stimuli; and this is especially the cases with cold-blooded and hibernating animals.

The foregoing laws may all be resumed in the following general law: The intensity of the faculty which animal tissues possess of producing vital phenomena, seems to be in direct ratio to the intensity and duration of nutritive repair, but in inverse ratio to the intensity and duration of the exercise of these faculties.

EXCERPTA.

A Poisoning Case from Haschish, with Notes. *By William Wright, M. D., L.R.C.S.E., Professor of Materia Medica, McGill University, Physician to the Montreal General Hospital, etc.*—From the rarity of similar occurrences, at least in Canada, I have been induced to record the following instance of poisoning by Haschish. It happened during the last month, (March,) to a much-esteemed friend, whose only fault was in the love he bore to science. He has increased its interest by having kindly favored me with the narrative in his own words.

"I was much interested some time since in the extraordinary tales related of the Cannabis Indica, and felt curious to experience some of its effects myself. With this intention I took, at various times, several doses of the Churrus or resin, and also of the extract or Haschish, commencing with one or two grains, and increasing the dose gradually to ten or more, without producing, however, the slightest perceptible result.

"As there could be little doubt of the purity of the preparation—it being obtained from the well-known and highly respectable firm of Morson & Co., London—I concluded that it must either have lost its virtues by keeping, or that the power of the plant itself must be much exaggerated. Conversing with a physician, a few days ago, on the subject, who entertained a different opinion, I swallowed, in his presence, about 15 grains of the extract, to convince him of the sincerity of my belief.* About two hours and a half afterwards I was reminded of the almost forgotten circumstance by a strange and sudden sensation of warmth at the pit of the stomach, which quickly extended over the whole body. At the same time I became considerably excited and talkative, and with difficulty restrained a strong inclination to violence. These symptoms quickly and completely subsided. After the lapse of a few seconds, without any warning, the blood seemed to rush violently to my head, the heart beat rapidly, my mouth and throat became perfectly dry, and I entertained a painful sense of suffocation. In a few minutes, after a succession of such attacks, the interval of recovery growing shorter with each, I grew very faint; the power of moving my limbs seemed to be deserting me; I could scarcely speak, and felt certain that death would quickly terminate my rash experiment. Up to this time I had been able to control myself, and to resist the wild suggestions which thronged through my mind. But now, completely overpowered, I seemed like a cork, floating up and down, now gently, now swiftly, through space. Then I was a balloon, gradually expanding as I filled with gas, till, becoming more and more buoyant, I suddenly, with a feeling of ecstasy, shot high up through the air. All at once I began to feel cold, and lo! I was an immense iceberg floating about on a calm sea. These illusions, however extravagant, were of a far more real and substantial character than is usual in ordinary dreams, and up to this period were rather of a pleasant nature than otherwise; there were short intervals of partial recovery, but the sense of buoyancy or floating prevailed throughout. These, however, were now replaced by others of a character so different, that for some days after my recovery I could not shake off the painful sense of dejection they left behind; I was whirling through an ever-changing scene, sometimes along the course of rivers, sometimes through thick dark forests, sometimes in well-remembered houses. At first I struggled hard to resist the power which seemed hurrying me on; but at last became calmer, and began to inquire *where* and *what* I was. Was I dead? I had no body or form, and could touch nothing. I concluded I was dead, and vainly tried to remember how and when I died; and as I experienced a very distressing sense of isolation and unrest, with a burning pain which I could not refer to any part of my being, (caused, I suppose, by a blister which was applied to the back of my neck,) I was convinced that I was entering on a state of suffering, which, though at the time not insupportable, would be ever becoming more painful. Ages passed away, through which I was still rushing on with a feeling of helpless despair.

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* The same extract had been the subject of our talk, from having appeared to evidence, in the person of another of my patients, a potent action in a single dose of two grains. To decide the improbability of this opinion, the *experimentum crucis* was voluntarily made.—W. W.

"At length I knew I was lying in bed ill. In vain I tried, again and again, to recollect the cause of my illness; my imagination suggested many explanations, but the true cause never once crossed my mind. At last the pain in my neck arrested my attention, and all at once I seemed to recollect that I had been shot in the back of my head, and thought I must be now in a state of delirium. It appeared to have happened a long while ago—many years. I seemed to fall asleep occasionally, and was much puzzled that I always awoke in the day-time, apparently about the same hour. These intervals of sleep or complete unconsciousness could not in reality have lasted many minutes, but to me they seemed days. I cannot describe the painful earnestness with which I struggled to recover my recollection of what had really happened. About three hours from the time I first became affected, I began dimly and imperfectly to recognize objects around me; they became gradually more and more distinct, and at length I was myself again, but with bewildered feelings, as if I had just risen from the dead.

"So far as my experience goes, the sensations arising from the use of

Cannabis Indica are most frightful, and nothing should induce me to repeat the experiment."

Notes.—The length of time that intervened between the ingestion of the dose and the manifestation of the symptoms is important to observe. The circumstances were not such as to explain it. The substance was taken upon an empty stomach, four hours or more after breakfast, and the symptoms did not supervene till sitting down to dinner, two hours or more afterwards. In the meanwhile he had continued his usual avocation, and had engaged his mind with its pursuits. Indeed, he had, so to speak, forgotten his recent experiment till reminded of it by the dangerous consequences that suddenly, though remotely, appeared. Generally the symptoms are late in occurrence in poisoning by Cannabis, seldom succeeding before an hour has elapsed from its administration. But the retardation here was unusually protracted. In consequence, when called upon to treat him, the ordinary indication of provoking emesis—which it is desirable to fulfil in most cases of toxication—did not suggest itself for practice. The substance I conceived had already been removed from the stomach, and was then more likely to be in process of excretion than absorption. Accordingly an opposite line of treatment was pursued. Effervescing sodaic draughts, with small doses of antimonial wine, were exhibited from time to time, and appeared decidedly beneficial by promoting the restoration of secretion, and thus aiding in the depuration of the blood of the deadly narcotic which was circulating with its living elements. At first the mucous membrane of the tongue and mouth generally, as well as the skin, was dry; but both moistened as recovery progressed. The urinary fluid was also rather deficient. Another advantage which the medicinal combination above-mentioned seemed to afford, was in keeping under a salutary level a tendency to inordinate vascular excitement of the arterial system generally, which was shown not so much in the entailment of increased frequency in the rate of its circulation, as in the super-addition of an augmented degree of relative power; but of this I shall speak again. The only other remedial expedient had recourse to was the application of a sinapism to the nucha, and its subsequent repetition. No true antidote has yet been discovered for Cannabis, and, admitting the possession of such an agent, its use was precluded in the present, and I believe would also be in the majority of cases, in consequence of the long time that passes away before the letheon brings out its effects; for whatever may be the utility of an antidote when opposed to its bane in the gastric cavity, it becomes powerless once absorption has been effected, and even though it might be supposed capable of overtaking the poison, while in transit through the system, its virtues would be rendered nugatory by the controlling influence of vital functions, which are directly averse to all chemical combinations or decompositions ensuing within the vessels, during life, between inorganic substances, introduced from without, that are not amenable to assimilation. Nevertheless the contrary has been vainly supposed by some writers.

Few facts are more remarkable than the astonishing influence which apparently indifferent substances, in trifling quantities, are capable of exercising over the intellect and feelings of man: like the creature of a passing fancy, his happiness may owe its existence to the most improbable sources. "It may be purchased for a penny and carried in the waistcoat pocket." The wretch, whose hopeless life is a burden, may procure from his deceptive stimulus a joy that the dull realities of existence never could have afforded. Nor is the example of this kind of sustenance confined to a few—the outcasts of despair—as it might be supposed. Indian hemp forms but one of the five great narcotics which have become articles of national consumption, and it is far from being the most extensively used, yet on it alone depend between two and three hundred millions of human beings for their pleasure. The condition of mind it produces is characterized by several peculiarities. As attentively studied from the present case, they seemed to be:

1. An exaltation of the impulsive propensities, creating a preparation to perpetrate violence upon provocation, the excitation being vastly disproportionate to the act, so that under no circumstance could the latter be extenuated by the former. The will is, to a certain extent, under the subjugation of impressions communicated from without, and, the reasoning also being impaired, true occurrences are distorted and falsely interpreted, with an invariable tendency to amplification, magnifying under increasing examination like pictures in the field of a stereoscope. The Haschish-eaters of the East betray this moral obliquity in a very decided manner. The shocking atrocities committed not many months ago by the Delhi rebels had probably a foundation in Cannabis. Of the notorious Old Man of the Mountain it is thus recorded: Taking the Count (Henry of Champagne) to the top of a high tower where were stationed guards in white robes, "I doubt," said he, "whether you have any subjects so obedient as mine," and, making a sign to two of the sentinels, they precipitated

themselves from the height and were dashed to pieces. Summoned at another time, by an envoy from a powerful enemy, to submit himself, the sheik called a soldier and ordered him to kill himself, which the man, unquestioning, did. "Tell your master," said the old man to the wondering envoy, "that I have sixty thousand men who will do the same." In fine, Haschish induces for a time a partial moral insanity, in which the impulses developed have a murderous tendency—either homicidal or suicidal, though generally the former.

2. A loss of individuality. The free spirit is no longer confined or trammeled by its corporeal tenement. Body, for the time, has no personality, and the existence solely experienced, is immaterial or etherial. The nerve substance of the brain seems paralyzed, and does not contribute its accustomed support to the substantiation or ratification of objects received through the special senses; or when these are unexercised, as in dreaming, the false creations of the struggling mind are not corrected by an application of consciousness. There is a remarkable buoyancy of feeling, and an exemption from the perception of ordinary physical clogs, so that, as in the above case, the idea that takes strong hold of the individual is that he is a spirit. The same hallucination occasionally manifests itself under another form, in which still the predominant feature is a belief in the possession of capabilities inconsistent with material organization. A gentleman while under Haschish thought himself a locomotive, and of whom it is said, that he, for the space of two or three hours, paced to and fro in his room with measured stride, exhaling his breath in violent jets, and when he spoke dividing his words into syllables, each of which he brought out with a jerk, at the same time turning his hands at his sides as though they were the cranks of imaginary wheels; and who, arising to taste water out from a pitcher, set it down with a yell of laughter, crying out, "How can I take water into my boiler when I am letting off steam!"

3. Closely connected with the preceding illusion is another, as regards the computation of time. The mind under Haschish forms no correct estimate of time; seconds may extend themselves into years, and minutes into centuries. While watching the progress of his case, my friend woke up after a short reverie of a few minutes' duration, and, without any reference to antecedent associations, for they had probably rolled far away into an interminable vista of oblivion, he gravely assured me that since then, the period when he had taken up his last wondering tale, two

thousand five hundred years had come and gone; while in verity his watch had scarcely sounded one-tenth the number of ticks!

4. A proclivity to extreme hilarity or laughter is established, rendering the patient ridiculously susceptible of the least humor, and causing him to exhibit his unwarrantable appreciations in an outrageously absurd or silly manner. The desire for laughter is often irresistible, and, even when not incited, peal upon peal of baseless merriment may astound the ear of the astonished beholder; at other times the exhiliration may give itself vent in a jovial song, loud shouting, or energetic dancing, or some other equally extravagant action. About the time of the occurrence of the above case, I had prescribed extract Cannabis to two other patients. One, a gentleman, who took a pill containing two grains three times a day. A short time after the first he said he got into a sort of histerical state, ready to laugh at anything, and with a constant inclination to giggle and behave unseemly. At the same time his eyes felt as if too large, and altogether strangely curious. He persevered in the use of them for over a week, as he derived great benefit from the symptoms for which they were ordered; but he assured me, that although the effects first experienced never recurred, yet he had always, more or less, a feeling of slight intoxication till he discontinued taking the remedy. The second was a lady, to whom it was prescribed in half-grain doses; and I considered that I could subsequently detect, in the slight veil of unusual cheerfulness, overspreading a naturally amiable countenance, the delusive power of Haschish. In the original case, or that first narrated, this proclivity was not so marked as might have been expected. I saw but slight traces, and these only occasionally: on the contrary, there was rather a painful character attached to the delusions, which is altogether unusual. As with other narcotics, the exact effects will be dependent upon modifying causes, as idiosyncrasies, national peculiarities, etc. Furthermore, his mind appeared to be in an alternating condition between excitement and sadness, which states invariably followed each other. The first was of short duration, not lasting more than a few minutes, while the latter would persist for at least a quarter of an hour before being broken in upon by its less sombre successor. These two phases were accompanied by corresponding changes in the pulse, it being always quicker and stronger under the first specified.

5. The other observations made upon his psychical disorder were, that for about the first three hours after its setting in, he was in a dozing kind

of state, easily awakened by any noise, such as that of ordinary conversation. Frequently he woke himself up. It was upon being thus aroused he declared his phantasms. He rapidly after fell into a sort of sleep, and never continued insensible beyond ten or fifteen minutes. It appeared natural, and was not accompanied by any stertor of respiration nor turgescence of the features. The pupils were, however, dilated, and the retina had lost its natural susceptibility to the stimulus of light. After the end of these three hours his sleep became more normal, and he gradually exhibited symptoms of decreasing disturbance. Afterwards I did not see him till about as many more hours had expired. He was then awake and rational, and had emerged from his Haschish-dream.

The dose had exercised a decided interference with common sensibility; ordinary perception was in a measure deadened, and there was a partial anæsthesia of the cutaneous surface, and yet sensation was positively acute, so much so as to be accompanied with feelings of actual pain. A slight pick or quick pinch gave no response; but a more extensive or prolonged irritation, as from continued pressure, was felt, and subsequently remembered. He told me afterwards that he experienced great pain from touches, etc., which at the time he did not complain of, or, as might be imagined, he was incapacitated from objecting to. Pain existed, then, although it could not be resisted. Its endurance was borne by the patient while the observer presumed he was unmindful of it.

The heart and arteries appeared to be specifically affected. The pulse possessed a strong, full tense character, the vessel conveying to the finger a pseudo-cartilaginous feel, as if the tonicity of its organic contractile coat was powerfully stimulated. Upon recovery he complained much of a feeling of painful constriction about the heart, and of pain in his wrist which was brought on by slight compression of the pulse. There was no capillary engorgement nor over-action—no texture nor organ evinced symptoms of determination of blood, nor were there signs of venous congestion discerned in any part. The action of Cannabis on the vascular system is, probably, one which, if better established, might be turned to great practical benefit. It is in conformity with the experience of Dr. O'Shaughnessy, of India, to esteem this remedy as an arterial stimulant well adapted for reviving the failing energies of the circulation, and for conducing to the establishment of re-action in cases of collapse. In recording his investigations with it in cholera, he states that "he has known the pulse and heat return, and the purging checked, by a single dose.

In one case of collapse, the patient revived immediately upon taking the remedy.''

The above case is also interesting in a negative way. It was not marked by several phenomena that have been witnessed in others, such as catalepsy, aphrodisia, etc.

And lastly, the reader is reminded that the word *Haschish* is employed in this communication to designate an extract of Indian hemp, prepared by Morson of London, and procurable in this city at Messrs. S. J. Lyman & Co., Place d'Armes. The distinction is the more necessary, for the same name is given in some places to an extract from Gunjah, obtained by boiling it in butter; and has likewise been applied to the tops and tender shoots and pistils of the flowers of the plant, in contrast to the larger leaves and capsules, which are known orientally as bang, or, in the language of Dr. Wood, (Pharmacology) *bung*. While a nearly identical term, *hushish,* is assigned, according to Avicenna, by the Arabians, to an inebriating substance prepared from the bruised leaves.—*Montreal Monthly Journal of Medicine and Surgery.*

ON THE ACTION OF PANCREATIC JUICE ON FAT—*By* ARTHUR LEARED, M.B. L.R.C.P. M.R.I.A. etc.—*Physician to the Great Northern Hospital, and Lecturer on Materia Medica at the Grosvenor-place School of Medicine.*—I published some years since an account of a case in which singular microscopic bodies were contained in the fluids habitually vomited. I ascertained that these bodies were composed of fatty elements. The presence of pancreatic juice in the fluids as a result of obstruction in the duodenum was inferred. I proved by experiment with pancreatic juice elsewhere obtained, that it possessed the remarkable property of forming similar bodies by its action on fat. Conversely it was thus inferred that this secretion had really been regurgitated into the stomach of my patient. Circumstances prevented my then investigating the subject further.

In M. Bernard's " Mémoire sur le Pancreas et sur le Rôle du Suc Pancréatique dans les Phénomènes digestives, particulièrement dans la Digestion des Matières grasses neutres," Paris, 1856—I find the following paragraph, page 58:

" Je dois encore signaler ici une autre particularité intéressante qui survient dans le suc pancréatique normal qui s'altère spontanément; je veux parler de la formation spontanée de cristaux particuliers que j'ai signalés pour la première fois dans le suc pancréatique du chien, en 1848. Ces cristaux vus au microscope, forment des houppes soyeuses magnifiques, et m'en rapportant alors uniquement à la forme, je les avais pris pour des cristaux de stéarine ou de margarine, parce que je les avais vus se former quelquefois très rapidement dans des émulsions de graisses que j'avais produites avec du suc pancréatique. Dupuis que j'ai parlé de ces cristaux, beaucoup d'autres observateurs les ont vus. Ils ont été représentés par MM. Robin et Verdeil dans leur bel atlas de chimie anatomique, et ces auteurs regardent ces cristaux comme formés de sulfate de chaux. J'y ai bien trouvé la chaux, mais l'acide paraîtrait être un acide organique."

It may be supposed from this that M. C. Bernard may possibly have seen my observations. But if he had seen them it would also appear that he concluded them to be erroneous. I believe I have met with the crystals described by him and figured by MM. Robin and Verdeil as those of sulphate of lime, and I affirm that they are not identical with the bodies I have figured and described. I own to some surprise, therefore, that the existence of the latter kind should be ignored by so eminent an experimentalist as M. Bernard.

A glance at the figures of MM. Robin and Verdeil,* and at the wood-cuts given by myself,† will convince any one that the things represented are essentially different. I am aware it may be objected that the pancreatic juice or infusion used in my experiments was abnormal, having been obtained some time after death occurring by disease. I reply, that fluid so obtained possesses properties sufficiently curious to merit attention.

The correctness of my statements may be tested as follows: Take a pancreas from the human subject as soon after death as possible; let it be cut into pieces and pounded in a glass mortar, with about ℥ss. of distilled water at 98 degrees Fah. Having allowed it to stand a few hours, let the pancreatic tissue be well squeezed in a piece of linen; let a couple of drachms of the expressed fluid be put into a test-tube, with a few grains of mutton fat in a divided state. Place the test-tube in a water-bath, just warm enough to melt the fat, and when melted let the contents of the tube be well shaken up. Replace the tube in the water-bath, and allow it to cool gradually. An abundance of the bodies I have described may soon be discovered by the microscope in the fluid from the upper part of the tube. They are not, however, in every instance developed in the same degree. Out of two experiments I performed very recently, in one the echinus-like bodies were perfectly developed, in the other they were neither so numerous nor so perfect, but still present. Not only is decomposition of the fluid unnecessary for their production, but they disappear when decomposition occurs. There is a marked difference in this respect between the crystals of M. Bernard and these bodies.

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*Op. cit. plate 6.

†Medical Times and Gazette, June 3, 1854.

That they are derived from fat by a peculiar action of the pancreatic fluid, I offer the following proofs:

They are not found in this fluid without the addition of fat; their microscopic characters; their melting at a moderate temperature; their complete solubility in boiling ether; and lastly, from the frequent appearance of oil (oleine ?) attached to the bodies that had evidently issued from their interiors. In some the spinous envelopes were even seen to be collapsed. In these instances it was evident that the crystalline portions of the fat had originally encased the non-crystalline.

I suggested in my former paper that the chief action of pancreatic fluid in the digestion of fat may consist in the separation of it into its proximate elements. This, as well as that margarine and stearine were not assimilated, occurred to me from reasons detailed. These views were strengthened by experiments I subsequently made on the effects of oleine obtained from cod-liver oil on patients, as compared with those of the residuary margarine.‡ Since then the same views have been advocated, chiefly as the result of experiments identical with mine, but without allusion to them.§ I do not speak confidently on a part of my subject only. To this subject I shall return in a future paper.

It is well known that the essential action of pancreatic juice in the digestion of fat is held by M. C. Bernard to be that it forms an emulsion with the latter. He believes that in the minute subdivision of the fat thus effected, absorption of the whole occurs. Another action discovered by him is, that this emulsion, which is at first alkaline, soon becomes acid. This appears to me of especial import. It argues that decomposition occurs simply a step farther than I suggested. The acid reaction is due to the liberation of the acid principles of the fat, and this goes to prove that the essential action of the pancreatic fluid consists in decomposition. There is reason to believe, too, that the degree of decomposition has relation to the time of contact between that fluid and the fat, within the province of the professed chemical physiologist. It is the prerogative of the physician to study physiology by aids more within his range. I have availed myself of a hint derived from pathology, and I submit that, having acted on that hint, the evidence derived from therapeutics tends materially to strengthen my views.

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‡Medical Times and Gazette, July 21, 1855.

§Dr. Garrod in British and Foreign Medical and Chirurgical Review.

48 Finsbury-square. [*Med. Times and Gaz.*

POST-PARTUM HÆMORRHAGE.—*Sir:* In some of your recent numbers I have read with interest letters from correspondents on a subject of the greatest possible practical importance, viz: post-partum hæmorrhage. In the year 1840, I adopted a mode of treatment which in seconds, rather than minutes, answered every indication, and more than realized my most sanguine expectations. As this has been tested now by upwards of seventeen years' experience, not confined to my own practice, but shared by the majority of practitioners with whom I have come in contact during that time, I give it to the profession with confidence, as an unfailing remedy.

It consists of an injection into the rectum of 4 oz. of turpentine, 4 oz. of cold water, and a handful of common salt, forcibly retained by means of a folded napkin firmly pressed against the anus, until violent bearing down or tenesmus is induced. The object is, *first,* to produce continuous reflex action; an action that will last sufficiently long to prevent all risk from relaxation of the womb, since the bearing down of the gut will be necessarily accompanied by the contraction of the womb. *Secondly,* as turpentine, by its stimulating properties, acts as a powerful restorative to the whole system, it becomes a matter of the greatest importance in the selection of the agent chosen for the purpose of inducing reflex action, to make use of one possessing such properties. *Thirdly,* the anti-hæmorrhagic properties of turpentine may possibly also prove useful. Want of time prevents my entering into the subject as fully as I should have wished to do; but if you will oblige me by the immediate insertion of this, I shall take an early opportunity of supplying you with the particulars of my first case, in which all the ordinary known remedies had failed, and my reasons for adopting the practice mentioned. I am, etc.

THOMAS ELLIOT, M.R.C.S.L.

Carlisle, Feb. 2d, 1858.—*Med. Times and Gaz.*

DIFFICULT DETECTION OF CALCULUS.—Mr. Solly has at present a case of much interest under his care in St. Thomas's, in which very different opinions have been given as to the existence of a stone in the bladder. The patient, a man of thirty, has three times been brought into the operating theatre, and sent back on account of those present not being able to satisfy themselves. Mr. Solly himself has, we believe, on each occasion entertained a confident opinion that he has struck the stone, but no sound perceptible to those standing by has been elicited, and in other hands the sound has forwarded no satisfactory evidence. It has been suggested that there is a calculus embedded in the prostate, or possibly encysted in the left side of the base of the bladder, in which positions the grating sensation has always been felt. In support of the diagnosis of an encysted stone is the fact that the man's sufferings are not severe. Some of our readers may possibly recollect a very instructive case recorded in our hospital reports about five years ago, in which a man under Mr. Erichsen's care in University College Hospital died of abscess in the liver, having a large calculus encysted in the walls of his bladder. He had been under the care, first of Mr. Arnott, and latterly of Mr. Erichsen, for a long series of years, and the stone had repeatedly been felt. On several occasions lithotomy had been decided on, and the man placed on the table for its performance, but as no one had ever succeeded in detecting the stone at the time, it had always been deferred. The result proved the wisdom of the caution which had been exercised, for the stone was so almost completely encysted that its removal could not have been safely accomplished.—*Med. Times and Gaz.*

ON THE TREATMENT OF INTERMITTENT FEVER—BY M. NONAT.—In a recent clinical lecture at La Charité, M. Nonat made some observations upon the treatment of intermittent fever. He observed that in judging of the value of febrifuges the real point to observe is not the relief of the febrile manifestation but of the splenic engorgement, for as long as the latter remains the cure is only temporary. Now, as the result of his trial of the numerous febrifuges which have been proposed as substitutes for cinchona, he finds that they all fail in influencing this condition of the spleen. Even arsenic itself must be placed in the same category of imperfect remedies, the sulphate of quinine being the only complete and reliable one, though the sulphate of cinchonine and the cinchona itself are so in a lesser degree only. To obtain all the effects from the quinine, however, its administration must be methodical. When paroxysms are present it should be given at the commencement of the apyrexia, beginning with a dose large in proportion to the splenic complication, and only gradually diminishing this. The continuance of the medicine, too, must be regulated by the diminution of the spleen. When the fever is simple, and without splenic engorgement, five or six grains in the day are sufficient. Supposing that the disease is of old standing, and that the spleen is much increased in size, then, during seven or eight days, 22 grains in two doses, are given daily, and afterwards for other five days 15 grains in one dose daily. The quantity is then gradually diminished to 7, 6 and 5 grains, the patient being instructed to continue doses of 1 or 2 grains for some time after this. When there is great splenic swelling, and especially when accompanied by pain in the hypochondrium, M. Nonat orders leeches or cupping, bringing the economy first under the influence of the quinine by employing it for three days prior to the depletion. Since this combined treatment has been put into force he has not found old cases of two, three or four years' standing offer resistance. The grand point is to employ the quinine in large enough doses, and to continue it until the engorgement of the spleen has become completely dissipated.—*Union Med.* 1857, *No.* 109.

RAPID DELIVERY.—*Sir:* Should you think the following case of any value in a medico-legal point of view, perhaps you will find a place for its insertion in your columns.

On the 1st ult. I was requested to attend a labor, some four miles distant. On arriving at my destination, I found that the case was a primapara, and that labor had been progressing for some three hours; but about half an hour previous to my arrival the membranes had ruptured, followed by an ordinary pain, and that no further pains had been felt. I waited in the room some ten minutes, but finding no pains present, I set it down as one of those cases in which the membranes rupture early, and a lingering labor ensues; and finding that an examination per vaginam was objected to at present, I directed that I should be called as soon as labor again commenced, and advising, if she found it practicable, that she should walk gently about, supported by the nurse.

I had hardly made myself comfortable in the parlor, before I was hastily summoned up stairs, and was informed by the nurse that "baby was born."

It appears that at her last pain, now some forty minutes ago, she "felt something come away," and the nurse fancied she "heard a baby cry," but no notice was taken of either circumstance, the completion of the labor not being expected so soon. The child was to all appearance dead, but I deemed it advisable to try every measure for its restoration, and did so for half an hour, but without avail.

I may add that the person was a respectable farmer's wife, and great was her grief when she found the position of affairs.

Should the like case have happened to one of the class of "unfortunates," I will leave your readers to draw their own conclusions as to the apparently "suspicious circumstances of the case." I am, etc.

Arthur Wiglesworth.

Blakeney, Gloucestershire, Feb. 1, 1858.—*Med. Times and Gaz.*

[This is a glaring instance of the folly of yielding to the prejudices of patients, against physical exploration. If Dr. Wiglesworth had done his duty to his patient and himself, he would have insisted on making the examination when he first saw the patient; then he would have found the child at its mother's breech, and all the probabilities are that he would have succeeded in resuscitating it.—Eds.]

The Attempt to Assassinate the Emperor.—The explosive machines employed were very much the shape and size of a large fir-cone, the base, which by reason of its greater weight, (being at least three centimetres in thickness,) must receive the shock on falling, being connected by chimneys with fulminating capsules. The cavity of the bomb was filled with fulminate of mercury, the explosive power of which is said to be fifty times greater than that of gunpowder. The wounds inflicted by these agents presented some peculiarities, upon which M. Larrey recently commented. Most of them are small and superficial, some being so narrow that a common probe could not pass. Some are confined to the skin, while others reach as far as the muscles, being usually limited to the superficial layers of these. There is almost always only one aperture, but sometimes there are two, usually only five or six centimetres from each other. The communication between these is usually direct, though sometimes tortuous. The apertures are usually irregular and jagged. The projectiles extracted from the wounds, and those collected in large numbers in the clothes, were generally very small, consisting of angular, irregular toothed fragments of the shell of the bomb. The number of wounds received by the same person is remarkable, twenty-seven being counted in one case—the lower extremities being usually the parts struck. Of little account at first, these wounds after some hours became more painful than their size would lead us to expect, owing probably to the narrowness of the track. There has been little hæmorrhage; and the inflammation and fever have been very slight, while there is an absence of the nervous stupor or agitation seen in ordinary gun-shot wounds. In a few cases large pieces have produced far more serious effects, either causing death by penetrating important viscera, or producing the ordinary effects of gun-shot wounds. The great number and relative mildness of most of the wounds received, is due in great measure to the anxiety of the assassins to make sure of their aim. The fulminate, by the violence of its action, so to say, crumbled up the machine in place of splitting it into splinters.—*Med. Times and Gaz.*

COLD WATER IN DYSENTERY.—Dr. Hiard states that for several years, during which he has pursued the following treatment, he has hardly ever met with a fatal termination in even very bad cases of dysentery, the patient being usually completely restored within two or three days, and sometimes sooner. On the first visit he is directed to drink glass after glass of cold water until three or four quarts have been taken. The pain soon ceases, and the stools become rare. Next day, the water is continued, and as a precaution against reaction, venesection is performed, the dysentery usually ceasing. To encourage a slight transpiration the cold water is replaced by tepid rice water, sharpened with vinegar. In some rare cases the disease resists till the sixth or seventh day, and then on the eighth a blister is applied to the leg, and sometimes another bleeding is employed. Enemata and opiates are both forbidden.—*Revue Med., December, p.* 753.

CITRATE OF IRON AND STRYCHNIA A NEW THERAPEUTIC AGENT.—For a long time back a therapeutic agent of very efficient properties has been used with considerable success at the Royal Free Hospital in cases of dyspepsia of an atonic character, by Dr. O'Connor. He has also found it productive of great benefit in similar conditions depending on functional derangement of the uterus. In these cases it acts as an emmenagogue when all other remedies have failed, and it has a powerful effect in tranquilizing the excitement of the nervous system. This preparation is a citrate of iron and strychnia, the dose of which is about three grains three times a day, to be taken immediately after a meal. There is now a case of chorea in the Hospital, under the care of Dr. O'Connor, immediately brought on by the patient being frightened by a thunderstorm in August last, since which time she has, without any intermission of the symptoms, been a sufferer. In this case the citrate of iron and strychnia has been only used for a few days, and already with marked benefit. The case is one of interest, and we purpose at a future time giving it in detail. The preparation which Dr. O'Connor uses is made by Mr. Bastick, of Brook-street, Grosvenor-square.—*Med. Times and Gaz.*

EFFECT OF LOCAL INFLUENCES ON SPASMODIC ASTHMA.—Dr. Hyde Salter read a paper on this subject before the Medical Society of London, (Dec. 5th, 1857.) The purport of it was to show that, in a very large proportion of cases of asthma, in which it has been fairly tried, change of locality effects an instantaneous cure, which is permanent as long as the asthmatic continues his residence in the place that has cured him. The author remarked that, although the subject of his paper was a single method of cure of a single disease, yet that the efficacy and completeness of the cure, and the painfulness and intractability of the complaint, vindicated it from unimportance; and, indeed, that in so distressing and unmanageable a disease, any remedy that offered even a small per centage of cures might be considered the greatest possible boon. The paper was illustrated by nearly thirty original cases, and the points that the author considered to be established were as follows:

a. That residence in one locality will radically and permanently cure asthma resisting all treatment in another locality.

b. That the localities which are the most beneficial to the largest number of cases are large, populous and smoky cities.

c. That this effect of locality depends, probably, on the air.

d. That the air that would be imagined to be the worst for the general health is, as a rule, the best for asthma; thus the worst parts of cities are the best, and conversely.

e. That this is not always the case, the very reverse being sometimes so—a city air not being tolerated, and an open, pure air effecting a cure.

f. That there is no end of the apparent caprice of asthma in this respect, the most varying and opposite airs unaccountably curing.

g. That, consequently, it is impossible to predict what will be the effect of any given air, but that probably the most opposite to that in which the asthma seems worst will cure.

h. That some of these differences, determining the presence or cure of asthma, appear to be of the slightest possible kind, arbitrary and inscrutable.

i. That the mere conditions of locality appear to be adequate to the production of asthma in a person whose disposition to it was never before suspected, and who probably never would have had it. had he not gone to such a locality.

k. That, consequently, probably many healthy persons who never have had asthma, and never may, would have been asthmatics if their lot had been cast in other localities.

l. That possibly there is no case of asthma that might not be cured if the right air could only be found.

m. That the disposition is not eradicated, but merely suspended, and immediately shows itself on a recurrence to the original injurious air.

n. That change of air, as change, is prejudicial.

o. That, from the caprice of asthma, the constancy of the result in any given case is often deranged.

The cases that were cited were very illustrative of these several positions, and some of them were of great interest. In reference to the frequency with which London air is beneficial to asthma, the author remarked that he was in the habit of putting to country asthmatics the two questions: "Have you ever been in London? have you ever had asthma there?" and that, if an affirmative answer was given to the first question, a negative one was pretty sure to be given to the second. In his own experience he had found hardly an exception to this rule.—*Am. Journal of Med. Sciences.*

SAN FRANCISCO MEDICAL SOCIETY—Annual Address delivered before the Society, pursuant to appointment, January 27, 1857: *By Henry Gibbons, M. D.* 8vo. pp. 24.—From the annual address of Dr. Henry Gibbons, delivered before the San Francisco County Medical Society, January 27, 1857, a production replete with correct principles and liberal views, well and boldly expressed, we select the following paragraphs, as presenting in brief outline, a general view of the climate and diseases of California:—

"The climate of the Pacific coast, in its etiological relations, opens an immense field of observation and study. In the winter season, if winter it can be called, from November to April inclusive, there is but little difference of temperature on the seaboard, in a range of fifteen degrees, from the mouth of the Columbia River on the north, to San Diego on the south. Nor is there much difference inland, except that the more elevated regions are liable to colder weather. Thus whilst snow and ice are rarely seen on the coast, the mountains of the Sierra Nevada range are covered with snow nearly all the year.

"In the summer season, a similar uniformity of climate presents itself on the seaboard; but during this period, from May to October, a wave of cold air flows daily from the ocean, often loaded with falling mist. So little difference is there in temperature between winter and summer in this wide range of coast, that flannel garments are constantly worn, and no one thinks of changing the dress from winter to summer. But beyond the mountain barrier, which skirts the ocean, and walls out the sea breeze from the interior, the heat of summer is often intense, the mercury rising frequently to 100 degrees or upwards. At the same time, the air is generally very dry. Almost invariably however, the nights are pleasantly cool, so that sleeping is well done everywhere.

"A few years ago it was supposed that the climate of California was almost proof against pulmonary disease. In 1850, if an individual happened to cough in church, all eyes were turned on him with curiosity and amazement. The native population, it was said, were entirely exempt from disorders of the lungs. But time has dispelled the delusion. Pulmonary consumption and the kindred affections have become the great enemy of human life, as in the Atlantic States. Our entire climate everywhere is less injurious, it is true, to pectoral disorders than the corresponding latitudes on the Atlantic. But, the cold and searching winds of summer on the seaboard, while they often build up the strength by their bracing and tonic power, are in general unfavorable to patients suffering from the class of maladies under consideration; and the extreme heat of the interior is equally noxious, from its debilitating influence. The relation of our climate to this class of diseases may be summed up in a few words. Persons afflicted with bronchial or pulmonary disorders, in the incipient stage, are almost invariably benefitted, and oft times cured by traversing a tropical climate, and taking up their abode in California. On the other hand, such diseases are developed *ab initio* in this country, about in the same degree as in the Atlantic States. As the female population increases, the bills of mortality exhibit a corresponding increase in the number of victims.

" Some years ago, it was a general practice to send pulmonary cases to the Sandwich Islands. But experience has shown its futility. We stand in need of some other sanitarium. In many cases, change of climate is the only remedy; and a genial climate, not liable to sudden or material fluctuations, and exempt from strong winds are requisite conditions. In the summer season, the region bordering on the Bay, at its northern and southern extremities, may serve the purpose, holding, as it does, a medium place between the damp and chilly ocean climate of San Francisco and the arid and scorching heat of the interior. In the winter we must turn our attention to the south. Los Angelos and San Diego, in the southern section of the State, are still too far north. The table land of Mexico will probably supply the desideratum. But even in Mexico, proximity to the ocean must be avoided. Twelve months ago, in a brief stay at Manzanella, which is on the western coast, in latitude 19 degrees, I observed among the native population an extraordinary prevalence of pulmonary disease, caused in all probability, by their sleeping on the damp ground, exposed, more or less, to the cool night wind. Sixty or seventy miles inland, in the vicinity of Cotima, is a different climate, said to be much more salubrious. With all the knowledge I now possess on the subject, this spot appears preferable to any other, and, accordingly, I have latterly recommended it to my patients instead of the Sandwich Islands. This subject, however, deserves much more consideration than it has yet received.

" Epidemics are of rare occurrence in this State. At an early period, when the comforts of life were greatly wanting diarrhœa and dysentery were prevalent, and extremely fatal. In those days men dwelt in tents, and slept on the earth ; they used a diet of animal food, almost exclusively, undiluted with vegetables and fruit, and composed of beef run down before being killed, and they employed that *blessed* prophylactic, brandy. With improved habits of life, and wholesome diet, and a diminished antipathy to the internal use of cold water, these diseases have greatly diminished, and death from dysentery or diarrhœa is comparatively rare.

" In the winter of 1850–51, the malignant cholera was epidemic in various localities, visiting our territory in its regular course of march on the western coast of North America. In this city the mortality was light, but Sacramento was nearly depopulated. Since that time, sporadic cases have occasionally appeared, and patients laboring under the malady, and dying with it, have been frequently landed from steamships from Nicaragua and Panama, and taken to the county hospital or otherwise disposed of. The disease has, in no well authenticated instance, extended to others. Several cases were said to have appeared in the neighborhood where the clothing from an infected vessel was sent to be cleansed, giving support to the theory which attributes the spread of malignant cholera to exhalations from the feces of its victims. But as the statements to that effect were connected with some pecuniary interests or speculations, they are not entitled to credit.

"In June and July, 1851, bronchial affections first made their appearance in San Francisco, in the form of an influenza. Since that date, catarrhal disorders have prevailed nearly every summer, in a sufficient degree to be pronounced epidemic.

"Croup is a disease from which the infantile population of our State suffer very much. It is apt to be violent and intractable, terminating fatally in many cases, perhaps as much for want of timely aid and proper nursing as from the intrinsic violence of the malady.

"Scarlatina first presented itself to the notice of the American inhabitants in the spring of 1851, on the heels of the cholera. There were few cases, however, and they were of a mild grade. Since that time it has occurred sporadically, and with the same tractable character. A form of disease bordering on malignant scarlatina has committed sad havoc in certain localities in the past two or three years. It commences with inflammation, and mostly ulceration of the fauces and tonsils, exhibiting no peculiar features, and exciting no alarm. Sometimes a cough is present from the beginning. Suddenly the patient is seized with croupy symptoms, and in a brief period, from six to twenty-four hours, life is extinguished. The muscular strength and the faculties of the mind are generally retained to a late period. The pulse is that of scarlatina, but there is no eruption. Delirium and convulsions seldom occur. Children are its ordinary victims, though adults are not always exempt.

"A striking feature of this malady is its fatality in certain families. It may visit only a few houses in a village or neighborhood, but in them it is apt to carry off all the young children, in rapid succession. In San Francisco, it has not prevailed so much as in Oakland, Sonoma, and some other towns. In the only instances within my knowledge, in this city, where all, or nearly all the children of the household were swept away, the victims were trophies of homœopathic practice. I have had no fatal, and no very malignant cases, in my own charge exclusively, but have visited a number in this city, and at a distance, in consultation, where no opportunity was afforded for autopsic examination. In these cases the fatal result appeared to arise from the rapid extension of the inflammation to the air-passages and lungs, without a full development of the proper symptoms.

"The treatment that has proved most salutary, under my observation, consists mainly of the free internal use of quinia, and the application of nitrate of silver, in solution, to the fauces, with a large blister to the chest in case of pneumonia.

"Diseases of the urinary passages are frequent and troublesome, especially chronic affections of the urethra, which often defy all treatment, yielding only to time. In fact, it may safely be said, that an unusual tendency exists on this coast to disorders of the mucous membranes in general.

"Insanity, as might be expected, is fearfully prevalent in California. It grows directly out of the excited mental condition of our population, to which the common use of alcoholic drinks is a powerful adjunct. Other cerebral disorders do not abound, excepting meningitis in infants. It has been remarked that, notwithstanding the constant exposure of a large portion of the male population to the extreme heats of the interior, 'sun-stroke' is scarcely ever heard of.

"There has been observed in the diseases of this coast an extraordinary tendency to the paroxysmal form, or to exacerbations, requiring the use of quinia. This occurs in dysenteries, in pneumonia, in puerperal women—in short, it is traced in almost every affection attended with febrile action. Fevers to which the dubious term malarious is conveniently applied are scattered every where, in city and country, and are often endemic in certain districts. In the intermittent phase, they are often excessively annoying by their repeated returns after apparent cure. Typhoid, or rather ataxic, fevers are frequently met with.

"Acute inflammations, requiring the lancet, are not common. It is truly singular to what extent venesection is discarded in California. I am apprehensive that we allow this potent remedy to be too much neglected.' *Amer. Jour. Med. Sciences.*

ON THE MEMBRANA DECIDUA WHICH SURROUNDS THE OVUM IN CASES OF TUBAL GESTATION.—Dr. Robert Lee read a paper on the subject before the Royal Medical and Chirurgical Society, January 26, 1858. He commenced by citing the various authorities to prove that the common opinion from the days of William Hunter to the present time was that the decidua or outer surface of the secundines, belong to the uterus, and not to the ovary, or that part of the conception that is brought down from the ovary; and that in cases of extra-uterine pregnancy, a deciduous membrane was still to be found in the cavity of the uterus. But a case is recorded in the seventh volume of the Medico-Chirurgical Transactions, in which the uterus is stated to have been "considerably larger than we observe that organ to be in the unimpregnated state, even in women who have borne several children. On laying it open, the uterine vessels were observed to be very large but empty, and there was a great quantity of gelatinous matter in the cavity and neck of the uterus. When this was washed off, the internal surface of the viscus looked very vascular, having been highly injected; but there was not the least appearance of a decidua." A second case is recorded in the seventeenth volume of the Medico-Chirurgical Transactions, in which the author himself was surprised at finding that "no organized deciduous membrane lined the cavity of the uterus, but the whole of it was coated with a thin layer of albumen." This case occurred in 1829, and the preparation was placed on the table of the Society in 1832; but until 1836, when another example of tubal gestation came under his observation, it does not appear that he made any attempt to determine whether the ovum in the Fallopian tube was surrounded by decidua. The two preparations were then examined together, and a description of the appearances observed was published in the Medical Gazette for 1839–40. These cases, with four others which have subsequently come under his observation, are the subject of the present communication. In Case 1, on opening the tube, and examining the different parts of the ovum, he found a deciduous membrane everywhere surrounding the chorion, and closely adhering to the inner surface of the tube, as the decidua usually does to the lining membrane of the uterus. Within the decidua, the chorion, amnion and embryo were distinctly seen. The uterus was larger than natural, and there was no appearance of decidua lining its internal membrane. In Case 2, on carefully examining the ovum contained in the right Fallopian tube, it was evident that a deciduous membrane everywhere surrounded the chorion, and adhered to the inner surface of the tube. The placenta, which was situated at the extremity of the ovum nearest the uterus, was seen covered with the decidua, and coagula of the fibrine of the blood were traced from the interstices of the placenta through the decidua into veins in the thickened muscular coat of the tube. The uterus was considerably enlarged, and its inner surface was coated with a very thick layer of yellowish white soft substance. There was no trace of any arterial or venous canal in this cavity. In the third case, the fœtus had reached the age of six months, and was contained in a cyst which adhered to the omentum and intestines and to the surface of the uterus, and contained a great quantity of thin, fluid-like pus. The walls of the uterus were healthy, and the cavity empty. There was no decidua or substance of any kind coating its inner surface, but a decidua was attached to the placenta in the usual manner. In Case 4, the greater part of the ovum had been removed, but still the decidua reflexa could be very distinctly seen covering a considerable portion of the villi of the chorion. In the uterus was found a coating of considerable thickness, and of a yellowish-white color, in which neither arteries nor veins could be traced. Case 5 is also one in which only a portion of the decidua was found in the tube. In Case 6, the substance lining the cavity of the uterus resembled the fibrine of the blood, and was of a red color at its upper part; where it had been detached from the surface of the uterus, the lining membrane presented a perfectly natural appearance. The embryo was enclosed in the amnion, with the vesicula umbilicalis remarkably large. The cells of the placenta and the villi of the chorion were distended with coagulated blood, and surrounded by deciduous membrane. By a careful dissection subsequently made, the decidua was found to consist of placental decidua, decidua vera and reflexa, with a decidual cavity. An enumeration of the preparations in St. Bartholomew's and Guy's Hospital museums is appended, illustrating the pathology of Fallopian tube conception. In the former there are five examples, and in one of these the museum catalogue states that "the outermost membrane inclosing the fœtus has all the character of decidua. Besides this membrane, the amnion and chorion are distinct; the fœtus and umbilical cord are also perfect." In the opinion of the author, there can be no doubt that a decidua surrounds the ovum; and though none of the other preparations have been dissected with the view of ascertaining the point, yet in all the separation of the ovum from the inner surface of the tube has been carried to an extent sufficient to enable us to demonstrate the fact. There are thirteen preparations in

Guy's Hospital museum. The author was permitted by Dr. Wilks, the conservator of the museum, to make a minute examination of the ovum in one of these, which had escaped entire through a rent in the Fallopian tube. He had the satisfaction not only to discover the vesicula umbilicalis, but to see the chorion completely surrounded by decidua, as in the case described at the early part of this communication. There can be no doubt that if the ova in these preparations were submitted to a similar examination, a decidua would be found in all, surrounding the ovum in the Fallopian tube. He did not wish to express an opinion on the nature of the membrane or substance found coating the inner surface of the uterus in the greater number of these preparations, which has been almost universally considered to be decidua since the days of Dr. William Hunter, although no blood vessels in it have been discovered. His great object was to demonstrate the existence of a decidua around the ovum in cases of tubal gestation.

Dr. John Clarke called the attention of the Society to one of the preparations of Fallopian tube gestation laid upon the table. The uterus, he said, was laid open, exhibiting, as he thought, a true deciduous membrane; not the membrane that was thought formerly to be formed by lymph poured out, but (as understood of late years) the inner membrane lining the uterus, and presenting a peculiar tubular appearance. In the neck of the uterus there was the usual secretion in the glands. It was quite clear that in the Fallopian tube there was no appearance of a strictly speaking deciduous membrane; there was nothing but the fœtus, the amnion and chorion lying against the inner coat of the tube. He did not venture to state, however, that in no case was a deciduous membrane formed round the ovum in Fallopian tube gestation; but he regarded the cases in which there was such a membrane as indicating an effort of nature to accommodate the part to a new function, which it was not originally intended to perform. The ovum had escaped from the ovarium, and lodged in the tube. Nature did not set up the inflammation, as in cases where foreign bodies existed; for this was not, strictly speaking, a foreign body, but a living body, intended by nature to be matured and brought into the world, and she accommodated, as far as possible, the tube to the purposes of the uterus; and accordingly there was a certain membrane thrown out, somewhat resembling a deciduous membrane, but, as appeared upon close microscopical observation, totally differing from the decidua in the uterus, having, however, certainly the function for a short time of sustaining the life of the ovum. The preparations exhibited did not appear to him (Dr. Clarke) to support the views of the author, since in almost all of them there was a deciduous membrane in the uterus, whether called lymph or shreds; and in those cases where it was not found the uterus was large, and there was some statement of uterine action, followed by the discharge of a membrane or fluid; and it was but reasonable to suppose that in those cases the deciduous membrane might have escaped.—*Med. Times and Gazette, Feb.* 6, 1858.

Paris Hospitals.—The care of the sick and the alleviation of their sufferings are duties of which no great society can be unmindful. Poverty and disease always work beneath the skirts of prosperity in large cities. Humanity suggests attention to their claims; security demands it, if only in the interests of public health. We have lately afforded an appreciation of the numerous institutions, the liberal funds, and the devoted services which private generosity places at the disposal of the pauper sick of London. Documents recently published afford the means of a like analysis of the charitable resources of Paris. This we shall attempt, from time to time. Sixteen hospitals permit the reception of 7,000 diseased persons at the same time in that city, while they employ the energies of 67 physicians and 38 surgeons, the most illustrious members of the medical body. Fifteen dispensers, 179 house surgeons, and an army of dressers, are here engaged in the same sacred duties with their seniors. In addition to these, there are employed in these hospitals 22 chaplains, 774 nurses, and 409 general servants. The annual expense amounts to 5,500,000 francs, and more than 100,000 patients are annually admitted. The impressive vastness of these figures enforces thought. The splendid munificence of such a system must be admired. The mercy which makes so vast a concession to the cure of the wounded is of a quality not strained:

> "It droppeth as the gentle rain from heaven
> Upon the place beneath: it is twice blessed—
> It blesseth him that gives and him that takes."

The devotion of the most illustrious of our brethren to this holy labor earns for the class whom they represent a proud title to esteem, whilst it brings with it the enviable consciousness of a high duty well performed. Such charity is nobility's true badge. But the philanthropist is here also a patriot. No one can say how much of the longevity in great cities is due to the separation of the sick from the healthy. A little leaven leaveneth the whole. An infinitely smaller number of sick may well become centres of wide-spreading circles of disease. The state, too, suffers, wherever disease becomes endemic, or when its ravages are feebly or unskillfully stayed. The deterioration of race—the degeneracy of physical power—the enfeeblement of body, at least, which necessarily result, inflict an economic loss. Every case of preventible disease presents a public loss. So also does each case of neglected illness or prolonged convalescence in the laboring man; for

> "Infirmity doth still neglect all office
> Whereto our health is bound."

Thus medicine claims a position in the calculations of the political economist, and the physician rises to the highest rank amongst practical statesmen.—*London Lancet—Virginia Medical Journal.*

UTERINE POLYPUS—*Great Hæmorrhage—Successful Use of the Ecraseur.*—This case occurred in the private practice of Mr. Worthington, of Lowestoft, and is described by that gentleman as follows:

Mrs. ——, a widow lady, aged 50, naturally of robust habit, from the town of Leicester, became fearfully blanched and prostrated by frequent and sudden discharges of blood from the uterus.

It was during a fit of syncope, arising from a sudden gush of blood from the uterus, that I was hastily called to see her.

I was informed she had been under medical treatment for the last two or three years, but no uterine examination had ever taken place. On introducing my hand into the vagina, I at once detected, as the source of hæmorrhage, a large fleshy preduneulated polypus, of pyriform shape, growing from just within the cervix, on its anterior part, and descending into the vagina.

At the end of a few days the patient had sufficiently recruited to warrant me in proposing its removal, which I conceived, after consultation with Dr. Webb and Mr. Spencer Wells, might be advantageously accomplished by means of the écraseur. No difficulty was encountered in passing the loop of the chain over the tumor, so as to encircle its neck. This having been accomplished, the chain was gradually tightened, and at the end of half an hour, pausing from half to one minute, the tumor was lying loose in the vagina.

Nothing could be more satisfactory in its result. By the combined agency of chloroform the operation was rendered painless, and what was of still greater importance, owing to the anæmic condition of the patient, bloodless. Upon the most careful examination, not a teaspoonful of blood escaped during the process.

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A CASE OF IMPREGNATION, WITHOUT RUPTURE OF HYMEN; GREAT SENSIBILITY OF PARTS.

By HOWARD SMITH, M. D., Professor Materia Medica, New Orleans School of Medicine.

Last winter a gentleman requested me to attend his wife in her confinement, which was expected early in the spring. He informed me at the same time, that, owing to some obstruction in the vagina, he had never been able to penetrate more than half an inch, and that at the consummation of his marriage he had been satisfied that the hymen had been ruptured. I suspected that there might be some narrowing of the passage, either by ligamentous bands or by the presence of a tumor. I requested him to pass his finger into the vagina as far as possible, and to describe to me as well as he could the nature of the obstruction which he found.

He assured me that the parts were so extremely sensitive to the touch, that it would be impossible for him to accomplish it, although he acknowledged that the introduction of the male organ, if not pushed *too* far, gave no great pain.

At his desire, I called upon the lady and learned from her that she had never suffered from any inflammation of the parts, even when a child; that she had menstruated regularly until after her marriage, and that her health had been uniformly good.

She submitted to an examination, but the moment my finger came in contact with the inner surface of the labia majora she showed such symptoms of real suffering that I desisted at once. Some washes of cold water, and borax, etc., were used, but without the least effect in diminishing the sensibility of the parts.

Her husband then proposed that an ocular examination should be made, as he was sure that the obstruction could be seen. I made it, and on separating the labia, I noticed, lying at the posterior part of the vagina and some distance in, a fleshy *looking* mass, which nearly filled the orifice, but it was impossible to touch it.

At first I had thought that the fear of the touch was affectation, but when, in the second examination, I had tried to pass a female catheter into that part of the vagina where there was no obstruction apparent, the pain occasioned by it was such as nearly to make her faint; and she exhibited, throughout the different attempts which were made to explore the parts, so much determination to control her feelings, that I soon became satisfied there really was as much pain as she said there was.

I next determined to use chloroform. Here another difficulty arose; for she assured me that when she had taken chloroform for the extraction of a tooth, though she was under the influence of it, she had felt the pain distinctly. At the first trial I made she came very readily under the influence of the anæsthetic, but the least attempt to pass the finger roused her at once and rendered her so nervous that I feared an attack of hysteria. The second attempt succeeded no better; but these two trials satisfied me that she could bear the use of chloroform without any danger. The third time, I brought her rapidly and fully under the influence of the anæsthetic, and, whilst she was breathing with stertor, I passed my finger into the vagina and up as far as I wished, and was convinced that the only obstruction which existed was an unusually dense hymen.

Although so completely insensible that she had no recollection, when she awoke, of what had passed, still she tried to move away from my finger and her countenance expressed pain.

Though relieved from apprehension of any serious mechanical obstruction, I could not but be anxious about the expected confinement, for if this extreme sensibility of the vagina continued, it would be impossible for me to make an examination during the progress of the labor, and I could not rectify a mal-position or even know whether the uterus was dilating or not. My only hope was that the severity of the labor pains would mask the pain caused by the examination. In that hope I was not disappointed.

Her labor came on naturally but slowly, as was to be expected in a first accouchment. The parts were distensible and lubricated by a copious secretion. I was called at nine o'clock in the evening, and remained with her during the night, the pains all that time being of that kind which characterizes the first stage of labor. About half past eight in the morning the expulsive pains came on, but not very intensely. In attempting to make an examination in the interval between the pains, I found the same sensibility, and was forced to wait until the next contraction of the uterus, when I introduced my finger and made out the presentation to be of the vertex, and in the first position. In withdrawing the finger I could now distinctly feel the sharp crescentic edge of the hymen, which was hard and dense. The perinæum was supported most carefully, and frequent examinations made to note the progress of the child's head. The pain which delivered the head was very severe, and I distinctly felt the hymen rupture as the head escaped into the world. The rest of her labor presented nothing of importance. She recovered without a bad symptom, without fever, and with no tearing either of perineum or walls of the vagina.

Remarks. I have thought this case to be both instructive and interesting. It adds another to the already numerous examples of the possibility of impregnation without the loss of the hymen, and proves that its presence cannot be considered an evidence of virtue. The presence of the hymen, and its rupture before labor by surgical interference, or during labor by the passage of the child's head, have already been placed on record by most obstetrical writers. Dr. Chas. D. Meigs, of Philadelphia, in his Treatise on Obstetrics, page 81, remarks: "I am well convinced that I have, in many instances, met with the unruptured hymen during my obstetric practice. I may venture to assert, that whoever attends a great many women in first labors, will have occasion to observe the existence of a very narrow hymen in many such persons. I make this statement, not unaware that I may be charged with having mistaken the fourchette for the organ in question. I think, however, that my opportunities in midwifery practice for acquiring experience have been sufficiently ample to warrant me against the commission of so gross an error."

In this case, the hymen was not a narrow band, but broad, thick, and nearly closing the orifice of the vagina, and there was no possibility of mistaking it for the fourchette. It proves, further, that the complete entrance of the male organ is not neccessary to impregnation. In fact, Murat, in the Dictionaire des Sciences Medicales, art. Vagina, page 457, quoting from Baudelocque, mentions the case of a young woman who allowed her lover to get the seminal liquor on the external parts of generation; she conceived, and at full term she was delivered only by making an incision through the dense membrane which closed the entrance to the vagina, and presented a narrow passage into which with difficulty they introduced the head of a pin."

In the case which I report, the most interesting feature is the great sensibility of the parts; for without this complication, the mode of procedure was perfectly simple. All that was required was a division of the dense membrane which formed the obstruction. But, owing to this tenderness, it was impossible even to make an examination without chloroform; and from the fact that even under its influence she shrank from the touch, an operation, which would require much more manipulation, could not be attempted. In looking over the different books to find some guide in the management of this case, I have been unable to meet with anything which at all resembled it in this particular.

The patient was anxious to be satisfied that there was no danger from malformation of parts, and nerved herself again and again to stand the pain, but it was impossible. She did not complain as much of the pains of childbed as she did of the simple touch of the finger. There was no inflammation of the mucous membrane, and there never had been. In short, there was no way of accounting for it. Since her delivery the same sensibility has returned, or, rather, remains, for it has never left her; it was only masked during labor.

April 18, 1858.

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RETENTION OF CATAMENIA—IMPREGNATION EXTRAORDINARY.

Beaver Dam, Clarke County, Miss., May, 1858.

Messrs. Editors: The following case having occurred in my practice, in this State, I have determined to give you a history of it. I think that it is one singular in its nature and most highly interesting in both a physiological and medico-legal point of view.

On the 25th of September, 1854, I was called to see Miss J. T., æt 17 years, and rather below medium size. I found her confined to her bed, and very much prostrated; pulse 120 to 140 per minute, and complaining of pain in the lower portion of the abdomen. By applying the hand over the region of the uterus, it was discovered to be enlarged and very hard. She was under the treatment of Drs. A. Bridges and T. Smith, who had learned that she had never menstruated, but had all the characteristics of a full grown woman, and was even troubled at regular periods with the symptoms commonly attendant on the flow of the "menses." On further examination, the labia minora were found in a state of permanent adhesion, also the lower portion of the external labia, all of which she had ever strangely concealed from her parents. The meatus urinarius, I believe, was unobstructed, although there was perfect adhesion of the nymphæ; which malformation, no doubt, had existed from birth. Now, what should be done? The uterus was very much distended, hard and painful, and the patient's strength nearly exhausted. It was evident that the uterus was filled with retained menstrual fluid, and there was no outlet through which it could escape. The case was plain. We agreed to separate the parts by incision, and let the retained fluid escape. After administering a little wine, I operated, using a director and a probe-pointed bistoury. I divided the labia through their whole extent, at one sweep, and, immediately, not less than a gallon and a half of very dark, thick, offensive fluid escaped. The flow continued a week. A tent was introduced to prevent reunion, and proper means adopted to prevent inflammation; still metritis supervened on the third day, but it yielded readily to mercury, opiates, blistering, etc. She was well in six weeks, became healthy, and menstruated regularly afterwards. We now come to the point I wish to state more particularly.

Twelve months after I operated, she married. Her husband was young and uninformed. She became pregnant in three months after marriage. On the 26th September, 1856, I was called to see her again. She was in the first stage of labor. I attempted to make the usual examination per vaginam, but could not introduce the finger. She then told me that reunion of the nymphæ had occurred soon after she got up in 1854, but she had hitherto concealed the fact. I exposed her to ocular inspection, and found an opening, at the upper part of the vaginal orifice, hardly large enough to admit a small female catheter. It was impossible to introduce a finger. Through this opening she had been menstruating regularly. Labor was progressing, and the pains were very severe. Drs. Bridges and Toole were called in. We agreed to enlarge the opening, which I did, as before, with a bistoury. As there was constriction of the vagina, it was with some difficulty the head of the child approached the inferior strait. The pains now became expulsive, the membranes ruptured, the head pressed against the exterior parts, the newly made opening was enlarged by laceration, and a fine, living child, weighing nine and a half pounds, was born. The mother speedily recovered, and the opening into the vagina is now of natural capacity.

The question now naturally suggested itself, how was sexual intercourse performed, and conception produced? I am not able to say, but leave it with you, or some physiologist, to solve. Her husband expressed himself as having been always fully gratified during coition, notwithstanding the vagina had never been penetrated. Conception took place, too, notwithstanding the opening into the vagina, as I have before observed, was hardly large enough to admit a goose-quill. It is evident that coition was never fully performed. Now, is it possible that, during the act of coition, a jet of semen was thrown into the vagina through this small opening? or, shall we yield to the exploded theory of the absorptionists, that conception was effected by the absorption of the seminal fluid?

[The foregoing cases are of great interest, both in a physiological and medico-legal point of view. Since the discovery, however, of the seminal fluid in the fallopian tubes, and even on the surface of the ovaries, there can be no room for the theory of absorption; the fluid, even when injected fully against the os uteri, has still to travel, through some strange physiological law, upwards to the localities mentioned, as, for all-sufficient reasons, impregnation *must* take place outside the uterus. What this physiological law is, we may never be able to determine, but we have a most reliable fact before us—that the seminal fluid is found, in the most natural condition of things, far away from the point reached by the male. At once the question arises: if the fluid can thus travel, is penetration of the vagina *necessary* for effecting impregnation? We have but to read the foregoing accounts, together with the published past experience of our profession, and we find that there is no such absolute necessity. This point settled, and we are at once led to the irresistible conclusion, that a rape is not penetration,—not physical injury inflicted; but, seeing the imminent danger to which incomplete coition subjects the woman, it may safely be reduced to the definition indicated by the history of the above cases.—D. W. B.]

A CASE OF POISONING BY STRYCHNINE, WITH REMARKS.

By I. L. Crawcour, M. D., F. S. A., Prof. of Chemistry and Medical Jurisprudence, New Orleans School of Medicine, etc., etc.

In January, I received from the hands of Dr. Glass, one of our students, a sealed bottle, containing a human stomach, for analysis. The following was the history. In December, the gentleman from whom the stomach was taken was found dead in bed, under suspicious circumstances. The appearance of the body was that of one who had died in a fit; the face was distorted and the limbs rigid. He had lived on unfriendly terms with his wife; and it was stated that the latter, two days prior to her husband's death, had purchased ten grains of strychnine, and was not to be found when the body was discovered. Under these circumstances, it was considered advisable to have a chemical examination, and the stomach was forwarded to me for that purpose. The murder (for it was one) occurred in a neighboring State, and as it is probable a judicial investigation will take place, I do not feel myself at liberty to mention either the State in which the crime took place, or the names of the parties concerned in it. It was not until March that I was enabled to commence the investigation. The bottle was opened in the presence of witnesses, and the stomach, in a state of perfect preservation and free from decomposition, from having been placed in alcohol, was taken out. It was tied at both ends, and contained about four ounces of a yellowish, almost odorless fluid. There were two spots of erosion on the mucous membrane, and the entire organ was pale. No solid substance was present either in the contents or in the mucous membrane. As the deceased had been an habitual drunkard, it is probable that the ulcerated portions, which were smooth and circular, and about the size of a twenty-five cent piece, situated near the pylorus, were the results of drink, and not the effect of poison. The analysis was made with the view of searching for strychnine, but as there was a probability that this might not be found, I used only half the stomach and its contents, reserving the rest for future examination. The process employed was as follows. The stomach was cut up into small pieces, and, with its contents, was boiled for an hour in a pint of distilled water, acidulated with half an ounce of pure muriatic acid. In a short time almost complete solution took place; when cold, it was filtered, and the filtered liquid evaporated nearly to dryness over a steam bath. The residue was now digested in alcohol, also acidulated with muriatic acid, and gently heated for a short time. It was again filtered, and again evaporated to dryness over a steam bath. This was again digested with warm distilled water, and when cold filtered into a long cylindrical vessel. Ammonia was added in excess, and the whole was well agitated for fifteen minutes with two ounces of chloroform and allowed to settle. After some time the chloroform settled at the bottom of the vessel, having acquired a yellowish tinge. It was removed by means of a pipette, care being taken not to disturb the supernatant liquid. The chloroform, which would contain the strychnine if any were present, was now allowed to evaporate spontaneously in a small dish. The residue was treated with concentrated sulphuric acid, to remove foreign organic matter, and gently heated for half an hour. Distilled water was again added, the solution filtered into a long tube, ammonia again added in excess, and the whole again agitated with chloroform. The chloroform was again removed by a pipette, and allowed to evaporate spontaneously in a watch glass. There was now left on the watch glass a mass of yellowish white crystals, which, under the microscope, exhibited the characteristic spiculæ of strychnine. This residue was now dissolved in slightly acid water and filtered, in order to purify it still more completely. In order to test it, a few drops were evaporated on a white porcelain plate, a minute drop of sulphuric acid added, and then a glass rod, tipped with a small quantity of powdered bichromate of potash was stirred in. Instantly the vivid blue changing to crimson, so characteristic of strychnine, was developed. This was repeated several times, and always with the same result. A drop of the solution placed on the tongue gave a permanent and intensely bitter taste. In order to place the matter beyond a doubt, I determined to use, in addition, the physiological test, and, in the presence of Dr. Brickell, Dr. A. Peniston, and others, I injected a minute drop under the skin of a frog. In two minutes, tetanus and spasmodic convulsions took place, speedily followed by death, the limbs acquiring an intense rigidity. The experiment was repeated on four other frogs, and with the same result. These three tests, therefore, (the reaction with bichromate of potash, the bitter taste, and the tetanic convulsions produced in the frogs,) enabled me unhesitatingly to state the existence of strychnine in the stomach, and to conclude that the deceased came to his death by the agency of this poison. I estimated the quantity I procured from the half of the stomach and its contents, to be about a quarter of a grain; it is possible it might be more, as I did not weigh it, but estimated only by the eye.

This case is peculiarly interesting, as being one of the few in which strychnine is extracted in a pure condition from the contents of the stomach. Could this not have been done, it would have been impossible to have stated the cause of death, as there were no symptoms to guide us, the man having been found dead; and the purchase of strychnine by the wife might easily have been accounted, for by her. The cases in which strychnine has been separated from the tissues by chemical analysis, are very few. Orfila mentions none. The experiments tried by him, consisted in placing strychnine and brucia in vessels containing intestines or putrifying animal matters, and then separating the alkaloids—a very different thing from extracting the poison after it has been swallowed by a living being, and then separating it from the organism. Christison mentions no cases. In the latest work on medical jurisprudence we possess, (Wharton and Stille, ed. 1855,) there is but one case mentioned, and in this instance the reactions were so slight as not to lead to a direct conviction of the presence of the alkaloid In the case of Dove, hanged for murdering his wife, in England, strychnine was found in the stomach. And in a case of suicide, reported by Mr. Hazel, (London Lancet, December, 1857,) three grains of pure strychnine were separated from the stomach.

The tests for strychnine, in its pure state, are simple and easily applied. It is usually in the form of white or yellowish white prismatic crystals, very slightly soluble in water; soluble in alcohol, ether, and chloroform. All its salts are very soluble in water. Its taste is intensely and persistently bitter, one part giving that property to one hundred thousand parts of water. It gives a white precipitate with solution of tannin; a yellowish color with strong nitric acid, or, if brucine be present, a blood-red hue is produced. The most characteristic test, however, is the following. Place the strychnine on a white porcelain plate, and then add a drop of strong sulphuric acid; allow the two to remain in contact a few minutes, and then add gently, by means of a glass rod, a minute quantity of bichromate of potash, in powder. A beautiful violet hue, changing to a rose red, is instantly produced. This reaction is exceedingly delicate, and I have found it to give a distinct coloration with the fifty-thousandth of a grain of strychnine. Another test is the physiological one, for which we are indebted to the late Dr. Marshall Hall. If a minute quantity of strychnine, in solution, be injected under the skin of a frog's back, violent tetanic spasms are rapidly produced, and the animal dies in a time varying with the strength of the solution. Some interesting experiments by Dr. Brickell and myself, connected with this point, will be found in the numbers of this Journal for September and October, 1856; and it will be observed, that we found this test sufficiently delicate to produce tetanic spasms when the thirty-thousandth of a grain was employed, and to produce death in the dose of the four-thousandth of a grain.

In all medico-legal investigations, there must be fulfilled three conditions, before we can swear to the presence of strychnine: first, the salt must be presented in the crystalline form; secondly, it must have a bitter taste; and, thirdly, it must present the characteristic reaction with the bichromate of potash and sulphuric acid. Many substances may fulfill each of these indications separately. Thus, morphine has a crystaline appearance and a bitter taste; quinine the same. Aniline will produce a violet color with bichromate of potash and sulphuric acid. But there is no single substance, as far as I am aware, but strychnine, which will combine the three conditions of taste, crystalline form, and coloration. Added to this, we should, in every instance where it is possible to procure frogs, try the physiological test, which will confirm all the others, and, in itself, is totally free from all fallacy. *There is no substance in nature except strychnine, and its accompanying alkaloid, brucia, which will produce tetanic convulsions when administered internally or applied endermically.*

A question arose on the trial of Palmer, for the murder of John Parsons Cook, (May 14, 1856, England,) as to the possibility of poisoning by strychnine, and its non-detection in the system. The subject has been investigated in the most masterly manner by Dr. Taylor, and his opinion, confirmed by that of some of the first physiologists and medical jurists in England, is that, under certain circumstances, the poison may be entirely eliminated from the system, or be so changed in its progress through it, as to leave no trace of its presence. My own experiments on this point, lead me to concur entirely with Dr. Taylor in this view. The organic poisons are not like the inorganic ones, indestructible. They are oxydized in the system, and when we detect them, it is the unaltered surplus, the portion which has not been used by the economy, that yields itself to chemical reactions. Some destroy life in such minute portions, or modify the vital actions in such infinitessimal quantities, as not to allow us an opportunity of operating upon a sensible quantity. No chemical tests have yet isolated the poison of the rattlesnake, of the wourali, or of the poisonous mushroom. In like manner the subtle poisons of small pox, cholera, scarlet fever and typhus, elude our view; and yet, that they are entities, and have an existence, is proved by their infectious properties.

The fallacy of the reasoning, that because strychnine has produced death, it must, of necessity, be discovered in the tissues, will be evident from the following. One grain of strychnine may produce death. Before this result can occur, it must leave the stomach, enter the blood, and pervade the tissues. It is true, we can discern the fifty-thousandth of a grain out of the body; but in this case we should have to isolate a much smaller quantity. The quantity of blood contained in the body may be estimated at about twenty-five pounds, or one hundred and seventy-five thousand grains. So that our tests must be delicate enough to enable us to isolate the one-hundred and seventy-five thousandth of a grain, before we could be sure of the presence of the poison. In the case of Dr. Warner, who swallowed half a grain of sulphate of strychnine, in mistake, for morphine, and where death occurred in fourteen minutes, we should have to isolate a much smaller quantity, and this, even on the supposition that strychnine did not undergo any metamorphosis while traversing the system. All analogy would lead us to suppose that strychnine, like quinine, morphine, caffeine and theine, undergoes a change in the economy, enters into the intimate composition of the tissues, and, by virtue of this ultimate division, produces its peculiar effects upon the nervous system. In the words of Dr. Taylor, "that strychnine acts as a poison by producing some change in the properties of the blood, either vital or chemical, is a generally received view among physiologists. To suppose that it alters or affects the blood without in itself undergoing some change, is contrary to the generally admitted doctrines of chemistry. The extraordinary changes of color which strychnine undergoes by contact with sulphuric acid and the peroxides in the application of the color tests, are properly ascribed to its combinations with nascent oxygen. In taking oxygen from these substances, is it to be supposed that it undergoes no change in itself. Is it in accordance with the laws of chemistry that A can affect B, without B affecting A? The amount of this conversion in the application of the color test, is well known to be dependent on the relative proportions of strychnine and nascent oxygen produced from the substances employed. There is in the blood, nascent oxygen, or oxygen in a state of high chemical tension, for combination with the carbon and hydrogen of the waste tissues of the body. Considering the remarkable affinity which strychnine shows for oxygen, out of the body, would it be a matter of great surprise that it should combine with or remove from the blood, a portion of that element, on which all vital actions and the proper secretion of nervous force depend? If the oxygen of the blood can combine with the carbon and hydrogen of the tissues, there is nothing to prevent its combination with the carbon and hydrogen of part of the strychnine. It is not necessary that the whole of the absorbed strychnine should be thus changed. The conversion of a part would satisfy the theory, and the altered blood would thus become the poison, affecting the motor tract of the medulla oblongata, producing an increased secretion of the nervous force which excites the voluntary musculary system." We must always bear in mind that it is only the *surplus* of a fatal dose, which is found in the stomach; and it is upon this that we operate in our tests.

It is fortunate for the ends of justice, that the symptoms produced by a poisonous dose of strychnine, are so peculiar and well marked as to be as diagnostic as the chemical reactions. They are *sui generis,* and it is impossible to mistake them for anything else. A person who has swallowed a fatal dose of the poison, is rapidly attacked with general trembling of the muscles, stiffness of the back of the neck, inability to walk, a feeling of suffocation and general uneasiness. These symptoms usually come on in about five minutes. The body and extremities become stiff, the hands are clenched, the body assumes a bow-like form, resting on the head and heels, the muscles of the abdomen are tense and hard, pain is usually severe, from the general cramps, the face gradually becomes livid, and death occurs, either from suffocation or exhaustion, in a period varying from ten minutes to two hours. Remissions occur, but there *is never any distinct intermission* unless recovery takes place. The intellect is usually clear to the last. During the remission, a slight touch will instantly aggravate the symptoms; firm pressure has, in some cases, relieved them. The differences between the tetanus from strychnine and ordinary tetanus are well marked. In traumatic tetanus, we can uniformly trace the disease to some wound or bruise. In idiopathic tetanus, which is rare, the symptoms commence slowly, the bowing of the body is usually a late symptom, and the patient may live for days or weeks. In tetanus from strychnine, the symptoms usually commence within a short time after the ingestion of food; the spasms are violent and uncontrollable, the sense of suffocation imminent, opisthotonos very early, the jaw not primarily affected, and the termination in death or recovery very rapid. If the dose be not fatal, the effects entirely pass off, and complete recovery is the result, *unless the poison be repeated,* when the same set of symptoms recommence. The post mortem appearances are not to be depended on, as in idiopathic tetanus and that from strychnine, nearly the same appearances are observable.

With regard to the treatment of a case of strychnine poisoning, we have nothing very definite. If a small dose has been swallowed, it is useless to resort to emetics, as in all probability the whole would have been absorbed before the symptoms made their appearance. If a large quantity has been taken, we must then evacuate the stomach; but I should decidedly oppose the use of the stomach pump, which has been recommended and resorted to by some. The danger is in the production of laryngismus, and any irritating body, such as the tube of the pump, is exceedingly liable to induce it. An emetic that will prevent spasms, is the one we need—and a large dose of tartarized antimony will fulfill the indication. As strychnine is soluble only in acid liquids, we might combine the antimony with some alkali, as soda or ammonia, which, by neutralizing the acids of the stomach, would prevent further absorption of the poison. One evident indication is, to keep the patient perfectly quiet. In the experiment we tried on frogs, we frequently found that if the animal were undisturbed, the effects, after a time, passed off; whereas, in those poisoned by exactly the same dose, but who were irritated and kept in motion, death speedily ensued. In fact, in many, symptoms did not appear until they were touched or disturbed. Among the best remedies is chloroform; not given by inhalation, but by the stomach. The first stage of chloroformic intoxication is one of excitement, and I have invariably seen intense involuntary action produced before the stage of coma arrived. Chloroform, given internally, in drachm or two drachm doses, does not produce this effect; and it may be judiciously combined with camphor and morphine. In a case reported in the London Lancet for July, 1857, the treatment by camphor was eminently successful. The patient had swallowed a large quantity of strychnine. When seen she was in the most violent tetanic movements, severe opisthotonos, and in extreme agony, especially in the situation of the dorsal vertebræ; the respiration was labored and gasping. Camphor, in the solid form, in from three to five grain lumps, was given freely. In about twenty minutes the tetanic movements gradually subsided. Camphor and opium was now given, and by the following day she only complained of muscular pain, great stiffness and headache. Dr. Marshall Hall recommends tracheotomy. He believes that strychnine destroys life in three ways: first, by inducing laryngismus and apnœa; secondly, by exhausting the nervous power; and, thirdly, by a secondary asphyxia. Tracheotomy disarms laryngismus of danger, of its apnœa. He gave a poisonous dose of strychnine to two dogs, and performed tracheotomy in one. The one on which tracheotomy was performed, lived, the other died. In India, the Cannabis Indica, or Indian hemp, is used with success in the treatment of tetanus; and a case is reported in the Edinburgh Medical Journal, for April, 1858, of traumatic tetanus cured by it. The remedy was given in two grain doses, hourly, until narcotism was produced. Entirely pure Indian hemp produces a peculiar catalepsy, and would, doubtless, be a valuable remedy, could we depend on its purity. All that I have hitherto seen in this city has been worthless. I have myself taken over forty grains of the extract, on several occasions, without its producing the slightest effect.

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CASE OF HÆMATURIA, SUCCESSFUL TREATMENT WITH SULPHATE OF QUINIA.

Translated from the April Number of the *Revista Medica de la Isla de Cuba*, by J. F. GRALL, M. D., Demonstrator of Anatomy, New Orleans School of Medicine.

Being, in the month of September last, in the district of San Anton de la Negada, a hurried call, to render professional assistance to the wife of an old acquaintance, occasioned the following observation, very important in a practical point of view, particularly to physicians practising in a swampy locality; and since it is a phenomenon only mentioned in a vague way by authors, even the most modern, when they speak of local affections sustained by a miasmatic, or, better said, malaric cause, I shall try to give a clear idea of the case.

The patient was a woman about thirty years of age, of bilious temperament, and already mother of five children. She complained of severe pains in the hypogastric, lumbar and inguinal regions, and in the posterior part of the thighs. These pains were not continuous, but observed, marked intermissions, which circumstance, united with a cessation of her menstruation for two months, made me suspect a commencing abortion; but I was not able to verify my suspicion by a vaginal examination, on account of the repugnance, sometimes invincible, which most women evince against that kind of exploration. This hypothesis established, my remedial means were naturally directed towards an arrestation of the abortion. I recommended, therefore, absolute rest in the horizontal position, evacuated the rectum by an emollient injection, followed by a demi-lavement with ten drops of laudanum, and gave her a "potion calmante," consisting of aqua laurocerasi, aqua florum aurant, and infusum tiliae. In consequence of this treatment the pains were somewhat relieved; sometimes they returned with the same intensity as before, to disappear after the administration of the medicine. She remained in this state for about four days, when, the urine which up to this time had been clear and normal, began to have a turbid aspect, and I noticed floating in it particles of mucus, and a few drops of blood, which latter gradually increased, so that three days afterwards she discharged almost as much blood as urine. Under such alarming symptoms, I considered a rigorous examination of the bladder of absolute necessity, to which the patient readily submitted, comprehending the critical state in which she found herself. A catheter introduced found no obstacle, neither in the urethra, neck of the bladder, nor the bladder itself. Although I endeavored to arrest this hæmorrhage by employing the different local and general remedies recommended by the majority of authors, I produced only a palliative effect; from time to time the urine presented a sanguinolent aspect, accompanied by the inseparable sequitus of pains above mentioned.

Twenty days had already passed, and my patient remained in the same state, with more or less prolonged interruptions, when I decided to call in consultation Ldo. D. José Manuel Nunez, who has already practiced twelve years, with excellent success, in that neighborhood. I related to him the past and what I had done, to which he answered, that in his long practice, which brought him in contact particularly with diseases of an intermittent character, and their larved (masked) types, he had observed four cases altogether similar to the one I presented to him; that, full of doubts at the beginning of his career, he could not fail to recognize the miasmatic element, after he had observed, that all the patients, who presented themselves with this affection, lived in swampy localities, and that consequently he had recourse to quinine, always with very satisfactory results.

I, in consequence, determined to administer to my patient the sulphate of quinia, not with full confidence, although I relied on the good faith, and the great knowledge of the medical topography of the county of Guamutas, which my colleague possesses; and not only for these reasons was I determined to give the quinine a trial, but also, because I considered the administration of some tonic necessary, to reanimate the failing strength of a patient who had suffered from a disease of a hæmorrhagic nature during twenty days. I confess openly, that I did not hope for the result I obtained with a scruple of sulphate of quinia, associated with the extract of absynthium, divided in twelve pills, which were taken in twenty-four hours. I took good care not to divulge to my patient the composition of these pills, because, in spite of my short practice, particularly in this island, I am not ignorant of the repugnance, the horror, they have in the country of this salt of the Peruvian bark, because to it they attribute generally the effects or consecutive lesions of swamp fevers, which terror sometimes goes to such an extent, that they refuse to take it even in the most urgent cases, as they consider it as a fire (un fuego)—to use their own expression. Unhappy error, that has conducted more than one victim to the grave!

Two days after the administration of the quinine, I saw my patient, and what was my surprise, when she told me that the sanguineous fluid had ceased from the moment she began to take the pills; and it did not return afterwards, at least not until the month of December, at which period I left that neighborhood.

When we consider on the one hand, the habitation of our patient, which was situated close to a swamp, fed by the overflow of a ditch, which does not empty itself where it ought to, in consequence of the embankment of the railroad of Jucaro, causing continuous miasmatic emanations to such a degree that few persons inhabit that part of San Anton de la Negada, without suffering from some form of intermittent fever; and, on the other hand, the immediate good result from the antiperiodic, *par excellence*, the quinine—if we consider these facts, there remains not the least doubt that the hæmorrhage, which might have carried our patient to the grave, was caused and sustained by the malaric or swampy miasmata, which she constantly respired.

We believe that our readers will find this observation really and truly interesting, because, far from being very common, I have not seen this occurrence mentioned by any author, not even in the very modern work of Dr. Wanderlich, (*Handbuck der Pathologie und Therapie, Vierter Band ziveite Haelfte. Stuttgart,* 1856,) which is, perhaps, the most complete treatise on pathology now existing in the field of science. When treating of hæmhorrhages caused by intermittent affections, he confines himself to the following words: "Hæmorrhages are very frequent in malaric diseases. In more than half the cases of recent intermittent fevers, more or less abundant, epistaxis presents itself, and is sometimes repeated. But it is when already a prolonged cachexia exists, that with more certainty these hæmorrhages and other sanguineous extravasations present themselves."

DR. JUAN C. OXAMENDI.

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OVERFLOW OF THE MISSISSIPPI—THE BELL CREVASSE.

The great "Father of Waters" is descending this season in all his grandeur from the vast area of the West, defying the vain efforts of man to confine him within his legitimate channel, in many places breaking his frail fetters and spreading desolation far and wide among the cotton and sugar plantations of the South. It is a remarkable fact that the inhabitants of the lower portion of this great valley display the same apathy and negligence in respect to the preservation and protection of their immensely valuable property, that they do towards the prevention of disease and the preservation of life. There is not an instance of a crevasse on record that is not traceable to the most culpable neglect on the part of the neighboring proprietors. So, likewise, a large portion of the sickness and mortality which has characterized the settlement of the lower valley may be attributed to the neglect of approved and well attested sanitary measures. This is imminently true of the citizens of New Orleans in regard to both property and health. Timely precautions are not taken to ward off inundation from the mighty river which, when swelled by its thousand tributaries, literally overhangs our streets and dwellings, and threatens us with distruction almost every Spring.

Even when a crevasse occurs, prompt and efficient measures are not used immediately to arrest its progress; but, as in the instances of McCarty's in 1816, and Sauvé's in 1849, the civil authorities creep and poke along, until the breach becomes so wide and deep that it is impossible to close it. Then comes the time for spending money, and thousands are lavished and millions are sacrificed in consequence of an evil which a little foresight, and perhaps the judicious expenditure of a few hundred dollars, might have *prevented.*

So, likewise, in respect to the public health; as a late writer forcibly remarks: "What miserable littleness it is, for instance, to erect fever hospitals, while we permit the seeds of fever to be scattered broadcast among the population; of how little avail to whitewash the sepulchre while the elements of destruction—inviting their fitting guest, the King of Terrors, to enter—are left rankling within." That stern preceptor, experience, will teach us wisdom after a while, it is hoped. A few more such disastrous overflows as that of the present year, and a few more such appalling epidemics as that of 1853, may possibly serve to convince the people that it is much easier, and more economical to *prevent* disaster, than to remedy it when it comes upon them.

These remarks have been thought appropriate to the introduction of the following interesting account of the celebrated *Bell Crevasse*, which we take from one of our city newspapers, the *Picayune*. It was accompanied by a wood cut, which we regret not being able to furnish.

"This Crevasse occurred on the plantation of Mr. John M. Bell, on the morning of the 11th of April.

"Those at all familiar with the topography of the Lower Mississippi must be aware that, for hundreds of miles above the mouth of our mighty Father of Waters, the lands on each side are protected from overflows by a vast system of Levees or embankments. The entire length of these artificial barriers is estimated to be at least 1200 miles, and the cost of their construction not less than six millions of dollars.

"Within the limits of the State of Louisiana, there are about 15,000 square miles of fertile alluvial beds, which lie considerably below the level of the river when its waters are at their greatest height; and notwithstanding the extensive and costly Levees which have been erected for the preservation of the cultivated portions of those lands, still, the waters at times become unmanageable, and, breaking through the Levees, spread devastation around.

"On no less than five different occasions has the city of New Orleans been partially inundated by Crevasses which occurred in the Levees above it. The first of these visitations was in 1785, the second in 1791, the third in 1799, the fourth in 1816, and the last in 1849. This spring the river is unusually high, higher indeed than it has been since 1849; but the Levees in the vicinity of the city are so well watched and kept in such a thorough state of repair, that no fears of an overflow are entertained.

"In studying the dynamics of the Mississippi, we learn that its descent from its source to its *embouchure* averages a fraction over six inches to the mile, while its average velocity is about sixty or seventy miles per day. Freshets at the remote sources of the river do not reach the mouth for nearly two months. Along the lower Mississippi the water usually begins to swell in February and does not again subside till June, and, when in its elevated stage, it bears down immense quantities of earth in suspension. On the occurrence of a Crevasse the water rushes through the breach with a velocity proportionate to the level of the river above the adjacent plains. As soon as the water passes the breach and begins to spread over the lower plains, its velocity is diminished, and the earthy matter which it had previously borne in suspension is deposited, the heaviest particles nearest the river and the finer atoms of soil at correspondingly greater distances. Thus the cultivable lands along the margin of the river become greatly widened by every Crevasse, and the subsequent increase in the fertility of plantations is a measureable compensation for the disadvantages of an overflow.

"The plantation of Mr. Bell, on which the Crevasse occurred, is situated about two miles above the upper limits of the city, and on the opposite, or right bank. For some time previous, the river had been gradually rising, and on the night before there had been a heavy storm of wind and rain which caused the wash along the sides of the "big drink" to be much higher than usual. It appears that through the Levee at this point there was formerly a wooden sluice, enclosed on all sides, through which water was conveyed from the river to rice fields in the rear of the plantation. In repairing the Levee after the cultivation of rice had been abondoned, this sluice was removed and the place which it had occupied was filled with earth. But the earth does not appear to have been packed with sufficient solidity, and when the river rose to its greatest height it found the week spot, and having once forced a passage it continued to widen the gap with a might and fury which were wholly irresistable. The neighboring planters immediately joined their forces to those of Mr. Bell, and put into requisition the ordinary appliances for the closing of such breaches. Their efforts, however, proved unavailing, and not only was the whole plantation in a short time submerged, but the inundating waters, contrary to expectation, crossed a neighboring canal and embankment, which were relied on as a protection for the plantations and villages farther down the river, and carried devastation in their track for many miles. The damages caused by this overflow are variously estimated at from four to five millions of dollars, the general impression being that for two seasons no sugar crops can be raised on the submerged plantations.

" After the first efforts to close the breach failed, a regular contract was entered into by a gentleman of science, skilled in such works, and piles and the pile driver are a part of the machinery which he employed. But his attempt proved no more successful than the first rude plan adopted by the negro forces, for after he had nearly crossed the gap with a triple row of piles, the water, in the centre of the breach, gradually wore out its channel till it measured twenty-five feet in depth, and the leverage which it then exerted against the piles was so great as to leave them an easy prey to such trees as were swept into this lateral off-shoot from the river.

"In our columns we have given, from day to day, an account of the persistent attempts made to close the Crevasse, until finally the piles were torn away, and the project of closing it was reluctantly abandoned. When thus left to itself, the channel of the Crevasse was at least two hundred and fifty feet in width, and its average depth was twenty-two feet; it will, therefore, not be necessary to resort to figures to convince our readers that an enormous quantity of water has passed through on its inundating mission during the five weeks which have elapsed since the breach was first opened.

"As this Crevasse is so immediately in the vicinity of the city it became from the first a centre of intense interest and attraction. New ferry lines were established to accommodate the thousands of visitors who repaired to it daily. Among other means of transit an inland line of skiffs, with awnings, and all appliances for comfort, was started from the half submerged village of Algiers, and conspicuous in the heralded attractions of the line were the opportunities afforded of passing the Marine Hospital by water, and of paying a visit to the inundated tomb of John McDonogh.

"About the time that the Bell Crevasse was abandoned a similar breach occurred in the neighboring Parish of St. Charles, on the plantation of Madam Labranche, and all efforts to close that breach having failed, there is every probability that the waters from both Crevasses will unite and greatly extend the area of devastation. The Bell Crevasse submerged several miles of the New Orleans, Opelousas and Great Western Railroad track; and as another section of the road is now submerged by the waters rushing through the Labranche Crevasse, it is not unlikely that the now interrupted trips on the road will be entirely stopped until the subsidence of the river.

"A favorite poet intimates that all partial evil is "universal good," and if from these lamentable disasters occurring at our very doors, practical wisdom can be deduced which will result in future safety, we will be inclined to acknowledge that the general good has been subserved by their occurrence, though private interests have largely suffered."

The river has been about as high this season as it was ever known. The water has been at the top of the levee in front of the city, and even ran over slightly, both in the upper and lower portions. The villages on the opposite side are almost submerged by the water of the Bell Crevasse. A Crevasse actually occurred at Algiers, but, for a wonder, by prompt and vigorous efforts, was soon closed up. It fortunately took place in the day and was discovered immediately. The loss of two hours would probably have placed it beyond control.

Numerous and destructive Crevasses have occurred along the river, as high as Napoleon, at the mouth of the Arkansas, the centre of the cotton growing region.

What will be the effect of this extensive inundation upon the health of the lower and more densely populated parts of the State of Louisiana remains to be seen. If the river falls early and the submerged lands become dry in August or the early part of September, we should expect disastrous consequences in the way of sickness.

ELEVENTH ANNUAL CONVENTION OF THE AMERICAN MEDICAL ASSOCIATION.

This large and important body, consisting now of about two thousand members in all, met yesterday in Eleventh Annual Session at the Hall of the Smithsonian Institution. Four hundred and six delegates and members presented their credentials and took their seats in the Convention. At a few minutes past the appointed hour of eleven o'clock the meeting was called to order, and the President, Dr. Paul F. Eve, of Nashville, Tennessee, took the chair. He was supported right and left by Drs. D. M. Reese, of New York, and H. F. Campbell, of Georgia, as Vice Presidents. The Secretaries were Dr. Robert C. Foster, of Tennessee, and Dr. J. Semmes, of Washington.

Business was preceded by an address to the Throne of Grace by the Rev. Dr. Sunderland, in behalf of the objects and proceedings of the Association, after which Dr. Harvey Lindsly, of Washington, Chairman of the Committee of Arrangements, welcomed the delegates to the Federal city. He spoke of the preceding sessions of the Society and the progress made, and, touching upon the self-sacrificing character of the medical profession, congratulated the delegates upon the philanthropic purposes which laid at the basis of their Association, and had called them together. Though Washington had yet little to excite their interest like some other capitals, there was one object of regard in its neighborhood, the Mecca of each pilgrim, the home and grave of Washington. He concluded by repeating to the delegates a hearty welcome to the city, and tendered the hospitalities and regards of its inhabitants.

The next business was the calling of the roll and the registration of names. Nearly all the States were represented, together with the United States Navy, and the American Medical society of the city of Paris.

The Committee of Arrangements, through its Chairman, reported that the sessions would be held from nine, A. M., to three, P. M.; also, that the President of the United States would be happy to receive the members of the Association and their ladies at the Executive Mansion in the evening, at eight o'clock.

Dr. J. M. Snyder, of Georgetown, was added a member of the Committee of Arrangements.

In order to furnish each State delegation an opportunity to choose a member to represent them on the Nominating Committee, a recess of fifteen minutes was taken, at the conclusion of which the following gentlemen were named for the committee:

Nominating Committee.—Job Holmes, Maine; George H. Hubbard, New Hampshire; P. Pinneo, Vermont; Eben. Alden, Massachusetts; Ashbel Woodward, Connecticut; J. Mauran, Rhode Island; H. D. Bulkley, New York; J. P. Coleman, New Jersey; Isaac Hays, Pennsylvania; H. F. Askew, Delaware; S. P. Smith, Maryland; Noble Young, District of Columbia; A. S. Payne, Virginia; W. H. McKee, North Carolina; W. F. Wragg, South Carolina; Jos. P. Logan, Georgia; J. T. Hargraves, Alabama; R. J. Breckinridge, Kentucky; J. Berrien Lindsly, Tennessee; W. M. McPheeters, Missouri; George Mendenhall, Ohio; Calvin West, Indiana; H. Luce, Illinois; Zena Pitcher, Michigan; Thomas O. Edwards, Iowa; O. Harvey, California; George Clymer, United States Navy.

One of the members having moved that the Army and Navy of the United States, as well as the Paris Association, be represented on the Nominating Committee, the motion was debated with some warmth, when the Chair decided that they should be represented. This decision of the Chair was appealed against, and the sense of the meeting taken, but the Chair was sustained.

A Committee on Essays was then appointed, consisting of Drs. Bohrer, of Georgetown, D. C.; Flint, of Buffalo, New York; and Hargraves, of Alabama.

The Chair announced that one of the Vice Presidents of the Society, Dr. Reese, desired permission to make a communication to the Association, which having been granted, Dr. Reese read a paper, which was in substance an apology for an act of his, in signing a certificate of the professional qualifications of a practitioner under the ban of expulsion from the Association for quackery and nostrum vending.

Dr. Condie moved that the apology be received. For this a substitute was moved, by way of amendment, declaring that the Code of Ethics of the Society had been violated. This amendment was put and lost, and the original motion of Dr. Condie prevailed.

Dr. Lindsly moved that the President be requested to deliver his Annual Address. This was carried.

Dr. James Bryan, of Philadelphia, stated that he had committed the same act as Dr. Reese, and begged to offer the same apology.

Dr. Condie moved to accept Dr. Bryan's apology, which was opposed, on the ground that the Philadelphia County Medical Society, which had taken action on Dr. Bryan's case, should first be consulted.

Dr. Condie did not think this Association should be governed by the Philadelphia County Medical Society, but act upon its own sense of what is right.

Dr. C. C. Cox, of Maryland, strongly resisted receiving either of the apologies offered, not deeming them sufficient for the wrong done to the etiquette and ethics of the profession.

Dr. A. B. Palmer, of Michigan, appealed to gentlemen to put an end to this matter. The apologies offered comprised an admission of the nature and extent of the transgression. What more could gentlemen ask or want? He hoped that a discussion, which might prove discreditable, would be concluded at once.

A motion to refer the matter at once to a special committee was then made, put, and lost, when Dr. Condie's motion was carried.

Dr. Eve, the President of the Association, then commenced his Annual Address, a truly able and comprehensive one, which was at times warmly cheered. At its conclusion, it was moved and carried that the President be solicited to furnish a copy thereof for publication in the Transactions of the Association.

Reports from Standing Committees being in order, Dr. Grafton Tyler, Chairman of the Committee on Prize Essays, reported that three Essays had been received, each of which the Committee had read with great care, and considered them all in respect to their intrinsic and relative merits. They had declared two of the Essays worthy of a prize each, and the third one they highly commended. The first prize, they had adjudged to the Essay on the Clinical Study of Heart Sounds in Health and Disease; and the second prize, to that on Vision and some of the anomalies as revealed by the Opthalmoscope.

Dr. Tyler then, in the presenee of the Association, opened the sealed packets respectively, disclosing the names of the authors of the Essays, and found the first named Essay to have been written by Dr. Austin Flint, of Buffalo, New York, and the second by Professor Montrose A. Palen, of St. Louis, Missouri.

It was then, on motion, resolved that the judgment of the Committee be confirmed, and that the Essays be referred to the appropriate committee for publication.

Dr. Palmer, of Michigan, moved, if agreeable to him, that Dr. Flint now give the Association a synopsis of his Essay, with which Dr. Flint readily complied.

Dr. Palen (quite a young man) also gave an outline of his Essay on Vision, the first idea of which was suggested by a railroad disaster in England, which took place by reason of the physical incapacity of the engineer to distinguish colors.

Dr. Lindsley, from the Committee on Arrangements, presented a letter from Captain M. C. Meigs, calling the attention of the Association to the subject of Ventilation, and inviting the members to visit the Capitol extension with a view to its examination on that head. A letter from Dr. Nichols, in charge of the Insane Asylum, was also read, inviting the members, either individually or collectively, to visit that establishment. A third communication was received, from the Rev. Mr. Maguire, President of Georgetown College, soliciting the favor of a visit to that seat of learning at any time they may deem convenient. On motion of Dr. Hammond, of New York, these several invitations were accepted, and the thanks of the Association were accorded in return.

It was moved and carried that Dr. Fitch, of the United States Senate, and Drs. Chaffee, Cranston, Robbins, and Shaw, of the House of Representatives, be members by invitation of the Convention. Dr. John Dare, of Maryland, was also added.

A resolution was offered, and passed unanimously, inviting Dr. Rose, of the British Navy, to take a seat as a member of the Association, and that the seat be on the platform with the President and Vice Presidents. (It was at first understood that Dr. Rose was in the city, and perhaps in the hall, but it was subsequently stated that he had gone to Niagara, and was preparing to join his ship in the West Indies.)

Reports were then in order from the Committee on Publications, which reported through their Chairman, Dr. F. G. Smith, of Pennsylvania. Report accepted.

Dr. Caspar Wistar, the Treasurer, presented his report, with some resolutions proposing a disposal of certain copies of the Transactions of the Association at lower rates than at present, so as to disencumber the Society of their longer possession. Passed. Balance of funds on hand, $806.

The Chairman of the Committee on Medical Education, Dr. G. W. Norris, not being present to report, the omission was referred to the cognizance of the Committee on Nominations for proper action thereon.

The Chairman of the Committee on Medical Education had his report at the hotel. Leave was granted him to report this morning.

The Committee on Nominations then entered the hall, and reported as the result of their deliberations the following names: For President, Dr. Harvey Lindsley, of the District of Columbia. For Vice Presidents, Dr. W. L. Sutton, of Kentucky; Dr. Thos. O. Edwards, of Iowa; Dr. Jonah Cosby, of New Hampshire; Dr. W. C. Warren, of North Carolina. For Secretary, Dr. A. J. Semmes, of the District of Columbia. For Treasurer, Dr. Caspar Wistar, of Pennsylvania.

These nominations were then put to the Association, and unanimously adopted, when three gentlemen were appointed to conduct the newly chosen officers to their seats, which duty having been performed—

Dr. Lindsley addressed the Association, thanking them for the undeserved and unexpected honor they had conferred upon him, one greater than he had ever before enjoyed, and the greatest, in his opinion, which could be enjoyed by any medical man in America. He should use his best efforts in the discharge of the honorable and responsible duties of the office, trusting to their forbearance for his inexperience. [Applause.]

It was moved and carried, that the outgoing officers take seats on the platform, and the thanks of the Convention, for the able manner in which they had performed their duties were also voted unanimously.

On motion of Dr. Palmer, it was ordered that the Special Committee on Medical Education (Dr. James R. Wood, of New York) report this morning, immediately after the report on Medical Literature.

Next in order were reports from the Committee on Medical Topography and Epidemics, from each of the several States of the Union. The report from Maine was referred to the Committee on Nominations. Dr. Smith, of New Jersey, read, from extracts, a careful and pains-taking report of his State, containing a fund of highly valuable information. His remarks on vaccination were especially excellent. There were also in it many touches of with and humor.

The Association then, on motion, at near three o'clock, adjourned to nine this morning.

SECOND DAY.

The Association was called to order at twenty minutes past nine o'clock, by Dr. Harvey Lindsley, the President of the Association.

The journal of yesterday was read by the Secretary, and approved.

Dr. Watson, of New York, moved that Dr. Delafield, the President of the first meeting of this Association, and its principal originator, be invited to take a seat on the platform with the Vice Presidents. Carried unanimously.

Dr. Atkinson, of Virginia, offered a resolution to amend the constitution in respect to the character of persons who may be presented for membership of the Association.

The Chairman said that all resolutions contemplating a change in the constitution, must lie over one year, for consideration at the next meeting.

Dr. Bond, of Maryland, called for the reading of that article of the constitution relating to the description of persons offering for membership.

Dr. Watson said that the subject could not now be discussed; it must lie on the table one year.

Dr. Atkinson withdrew his resolution so to amend it that it may secure his object and not fall under the rule to lie over one year.

Dr. Eve, of Tennessee, proposed that Dr. W. H. Cumming, of Georgia, for some time missionary to China, be a member by invitation, and have a seat on the floor. He had a valuable paper to submit, and moved its reference to the Committee on Publications. Agreed to.

Dr. Dove, Chairman of the Committee of Arrangements, offered the names of Drs. Hupp and Knight as members by invitation. Accepted.

Dr. A. B. Palmer, of Michigan, Chairman of the Committee on Medical Literature, then read his report on the progress of medical literature of the United States since the last meeting of the Association. It was a masterly review of the numerous publications of the country during the last year. Its full reading was dispensed with, and the whole report was referred to the Committee on Publications.

In bringing their labors to a close, the Committee present the following as a very brief resumé of some of the leading positions in the report:

The periodical literature of the United States is regarded as possessing great abundance, variety, richness, and general excellence; and though still possessing defects, is constantly improving. Many of the contributions are of great weight and value, indicating an enterprizing and industrious profession. Serious defects are regarded as existing in the review department, arising mainly from the fact that the income of the journals will not justify pecuniary disbursements for literary labors; and editors necessarily engaged in other pursuits cannot command the time, if all possessed the ability, to do the work thoroughly and well.

A few well supported journals, in place of the many illy sustained, might tend to correct this evil; but the multiplicity of local journals is considered as peculiarly beneficial, by collecting from a greater variety of sources a larger number of facts, and developing the powers of a larger number of writers. The interests of this part of our literature demand a more prompt and liberal pecuniary support.

The number of original American Medical works is increasing, and their character is improving, and in some respects, particularly in practical utility, they will not suffer in comparison with those of Europe; yet serious imperfections exist, and great improvements are demanded. Great and permanent improvements in medical as in general literature must be gradual, depending more upon the advancement of education, of taste and intelligence, than upon any specific measures which may be adopted. Still, various particular measures, such as the frequently writing of medical theses during professional pupilage, and keeping systematic records of cases when in practice, would do very much in hastening on improvement. But, for the greatest perfection of our literature, we must wait the fuller development of our country, and for those changes of time and circumstances which shall produce a larger number of devoted savans and scholars, placing them in situations where a variety of absorbing pursuits shall not prevent the concentration of great talents upon a comparatively limited range of subjects.

On the subject of the reprint of foreign works, it is held that while the free circulation of the best class of these works among us increases the knowledge and improves the taste of the masses of the profession, it does not interfere with the production of the higer order of original works, and that the moral obligation of our Government to join with Great Britain in the enactment of an international copyright law is by no means clearly established.

In conclusion, your committee would say, that if, as sentinels placed upon the walls of our medical Zion, they are asked in relation to its literature, "What of the night?" the response must be, "The morning cometh." The darkness which has hung over that literature is breaking away. There is at least a dawning in the east, and though the chariot of day may roll on but slowly, the full effulgence will come at last.

Dr. Delafield was here announced, and took his place on the platform.

At the close of the reading of Dr. Palmer's report—

On motion, Dr. Hoseman, of Alabama, was admitted to the floor as a member by invitation.

The next matter in order was Dr. James R. Wood's special report on Medical Education, which that gentleman came forward and read. The results looked to in this report are thus summed up:

1. Primary medical schools should be encouraged; but, as office instruction will continue to be sought by students, practitioners should either give them necessary advantages of demonstrations, illustration, and recitations, or, if not prepared to do so, they should refer them to such primary schools or medical men as will give them proper instruction.

2. The number of Professorships should not be less than seven, viz: A Professor of Anatomy and Microscopy, Physiology and Pathology, Chemistry, Surgery, Practical Medicine, Obstetrics, and Materia Medica.

3. There should be but one term annually, which should commence about the 1st of October, and close with the March following, thus lengthening the term to six month. The commencement of the term in October should be uniform in all the colleges throughout the country. During the session there should never be more than four lectures given daily.

4. The qualifications for graduation, in addition to those now required

by the schools, should be a liberal primary education and attendance upon a course of clinical instruction in a regularly organized hospital.

In order to give our medical colleges an opportunity to consider the recommendations here advanced, and that this body may have the advantage of their wisdom and their mature views, before any definite action is taken upon them, your committee submit to the Association the following resolutions:

Resolved, That the several medical colleges of the United States be requested to send delegates to a Convention, to be held at ———, on the ——— day of ———, for the purpose of devising a uniform system of medical education.

Resolved, That the present report of the Special Committee on Medical Education be referred to such Convention, for its consideration.

Resolved, That said Convention of delegates from the several colleges of the United States be requested to submit to the meeting of this Association in May, 1859, the result of their deliberations.

This report was ordered to be referred to the Committee on Publications.

The Committee on Nominations then reported the result of their deliberations, as follows:

That the city of Louisville, Kentucky, be the place of the next annual meeting of the Association, and that Dr. S. M. Bemiss, of Kentucky, be the second Secretary; that the standing committee for the next year consist of Dr. F. Gurny Smith, of Pennsylvania, chairman; Caspar Wistar, of Pennsylvania; A. J. Semmes, of the District of Columbia; S. M. Bemiss, of Kentucky; Samuel L. Hollingsworth, of Pennsylvania, Samuel Lewis, of Pennsylvania, and H. F. Askew, of Delaware.

That the Committee on Medical Literature consist of Dr. Jno. Watson, of Mississippi, chairman; Lyndon A. Smith, of New Jersey; C. G. Comegys, of Ohio; R. W. Gibbs, of South Carolina; Wm. M. McPheeters, of Michigan.

The Committee on Prize Essays consist of Dr. J. B. Flint, M. Goldsmith, Henry Miller, of Tennessee, A. Evans, of Kentucky, and Calvin West, of Indiana.

The Committee of Arrangements to consist of Dr. R. J. Breckenridge, of Kentucky; G. W. Ronald, B. M. Wible, D. W. Yandale, D. D. Thomson, A. B. Marshall, G. W. Bugloss, R. C. Hewitt, and A. B. Cook.

The Committee on Medical Education to consist of Dr. G. W. Norris, of Pennsylvania; A. H. Luce, of Illinois; E. R. Henderson, of South Carolina; G. R. Grant, of Tennessee, and T. S. Powell, of Georgia.

This report of the Committee on Nominations was received and adopted by the Association, and leave granted it to sit again.

Dr. Comegys, of Ohio, called for some action on the resolutions at the conclusion of Dr. Wood's report.

Dr. Atlee, of Pennsylvania, said that whilst the body of reports from committees should go to the Committee on Publication, it was proper that resolutions appended must be acted on by the Association at large.

Dr. Hamilton, of Buffalo, N. Y., made a motion, which was so amended, after much discussion, and carried, as to refer the resolutions to a committee consisting of one member from each of the representives of the medical colleges of the United States, these members to be named by the colleges themselves, to report on this (Thursday) morning.

Dr. Smith, of New Jersey, moved that the newly elected Secretary take his place at the table, and that the Association accord its thanks to the retiring Secretary for his valuable services. Motion carried.

Dr. Lamb, of Pennsylvania, moved to reconsider the vote upon Dr. Condie's resolution whereby the apology of Dr. Bryan was accepted by the Association.

The Chairman thought the reconsideration was not in order without a suspension of the rules.

A motion to suspend the rules was made and put, when there were 111 votes for suspension to 82 against it. The Chair said that as two-thirds had not voted in the affirmative, the rules were not suspended.

A delegate appealed against the decision of the Chair, and Cushing's Manual was referred to, whereupon

The Chair changed his decision and stated the question on reconsideration to be in order. On putting this question to the Convention, there were 142 votes for reconsideration and 70 against it. This brought up immediately the subject of the McClintock affair of the day before, which it was supposed had been put at rest by the apologies of Drs. Reese and Bryan.

First came up Dr. Condie's resolution on the acceptance of the apology of Dr. Bryan, but the consideration of this was, after some brief discussion, laid on the table.

Dr. Bond, of Maryland, moved to pass to the order of the day; whereupon a great deal of excitement was aroused and unreportable conversation took place on the McClintock subject.

Dr. Jewell, of Pennsylvania, called for the reading of Dr. Reese's letter of apology, read the day previous.

Dr. Baker, of Indiana, appealed to the Association on the subject of the disorder occasioned by this subject whenever introduced, and moved to postpone it indefinitely.

Dr. Cox, of Maryland, called for the reading of the memorial of the Philadelphia County Medical Society, which he understood to be in the hall.

Dr. Jewell said that what was done yesterday by gentlemen on this subject was done in ignorance of all the facts, and that gentlemen had since regretted their action. He proposed to read the memorial.

Dr. Biddle, of Philadelphia, said that the memorial ought not to be presented; if presented it would be in violation of plighted faith to the contrary.

Dr. Jewell submitted an explanation, and was followed by several other gentlemen, when the excitement became such as to make it necessary to go into Committee on the Whole, Dr. Thomas O. Edwards in the Chair.

The minds of members were so full of the topic that it was brought immediately before the committee, and the floor was accorded to

Dr. Jewell, who read the report (a voluminous one) of the Philadelphia County Medical Society, on the agency of Drs. Reese and Bryan, in the appointment of Dr. James McClintock (an expelled member of the Society) to the office of Physician-in-Chief of the Blockley Hospital, in Philadelphia. This report severely commented on Dr. Reese's connection with the matter.

Dr. Humphries, of Iowa, moved to restrict each delegate from speaking longer that five minutes in committee.

Dr. Bond moved to amend this so as to except Dr. Reese from its operation.

Dr. Reese suggested that he would require some little time to prepare a reply to this long report, and asked to be allowed till to-morrow for the purpose.

The excitement here became considerable, and motions of various kind were made one upon the other.

Dr. Cox thought each member of the committee should be allowed fifteen minutes. Every body present was not used to speaking, and some would require five minutes before they could get fairly upon their subject.

This proposition was lost, and the motion of Dr. Humphries, with Dr. Bond's amendment, was carried.

The floor was then accorded to Dr. Reese, who spoke in reply to the

Philadelphia report, and gave a narrative of his whole course and conduct in reference to Dr. McClintock, and concluded by saying that if the apology of yesterday was insufficient, he was willing to change and make it satisfactory.

The Chairman announced the receipt of a communication from the President and Faculty of Georgetown College, apprizing the Association that an entertainment would be spread for them at the College, at five o'clock in the afternoon, and that carriages and omnibusses would be in waiting at all the principal hotels of the city to take members thither.

This invitation was subsequently accepted, and the thanks of the Association tendered to the President and Faculty of the College in return.

Dr Atkinson, of Virginia, moved to refer the whole matter touching Dr. Reese's apology to a committee of seven, to report to-morrow (Thursday) morning.

Dr. AtLee, of Pennsylvania, opposed this motion with great warmth and vehemence, saying, at the conclusion of his remarks, that to be a worthy member of the medical brotherhood it required something more than professional ability; it required something here, pointing to his heart. [Loud applause.]

Dr. Payne, of Virginia, related an anecdote in illustration of the difficulty, which produced a laugh.

Dr. Condie questioned a certain statement made by Dr. Reese during his remarks; when

Dr. Bowling, of Tennessee, rose to set both gentlemen and all parties right, and show that the veracity of neither was impugned.

The debate was further participated in by Drs. Condie, Cox, and others, when

Dr. La Roche, of Pennsylvania, said that the reason why the Philadelphia County Medical Society's report was introduced to-day was, that Dr. Reese's apology of yesterday was deemed imperfect and insufficient.

The discussion was now eager and warm, and various statements, motions and propositions by numerous members were made, when the general wish appearing to be that the apology should be fuller and without suspicion of mental reservation—

Dr. Reese rose and made such an apology, which he said was to be taken as unreserved and without qualification.

The committee here rose and reported progress, and asked leave to sit again; soon after which it rose, and President Lindsley took the chair in the house.

A member moved that the apology of Dr. Reese be received as full, entire, ample, and satisfactory. This was put by the Chair and carried, and several members thereupon came and shook hands with Dr. Reese.

An apology of similar character and extent was then offered for Dr. Bryan, at which juncture, under the suggestion of some members, Dr. Reese was induced to present his amended apology in writing.

Dr. La Roche suggested that the word "regret" should find place in the apology; when

Dr. Reese said he would not consent that the gentlemen should dictate to him what language to adopt.

Dr. AtLee insisted on the expression of "regret;" without it the apology would be nothing worth.

Dr. Wood did not deem the apology sufficient in its present form.

Dr. Reese remodified his apology, but it did not yet satisfy gentlemen.

Dr. Bryan would go to the same extent of apology as Dr. Reese, but no further.

The Chairman here apprised members that the Pennsylvania Railroad Company, the Wilmington and Manchester North Carolina Company, the Illinois Central, the Northeast Company of South Carolina, and the Richmond and Petersburg Company had made free the return home of members on their roads.

Dr. Reese made another effort at writing a satisfactory apology in the following terms:

"The undersigned regrets that he certified to the professional qualifications for the Blockley Hospital in Philadelphia of an expelled member of this body, and hereby offers this apology for his departure from the Ethical Code.

D. M. REESE."

It was then moved and passed by the whole Association without a dissentient voice that this apology was satisfactory.

Dr. Bryan then offered the same as the language of his apology, which was likewise accepted as satisfactory.

The Convention then adjourned to this morning.

THIRD DAY.

The chair was taken by President Lindsley at half past 9 o'clock and the Convention called to order.

Dr. R. Foster, of Tennessee, moved the following resolution, which was carried :

Resolved, That all reports from committees on epidemics be referred to the Committee on Publications, and that all communications from the same committee be referred to the Committee on Nominations.

Dr. Grant desired leave to introduce a memorial of complaint, and moved to suspend the rules. After some discussion this was withdrawn by the mover.

Dr. Eve moved that a Committee on Voluntary Essays be appointed; also, that Dr. Heywood be appointed to represent this Association in the societies of Europe.

A communication was received from Prof. M. F. Maury, in charge of the National Observatory, inviting the members of the Association to visit that establishment.

The Committee of the Representatives of Medical Colleges on the subject of Medical Education having been called, its chairman, Dr. Hamilton, of New York, reported. This report was received and referred to a Committee of the Whole. Whilst this was under consideration—

Dr. Rogers, of New York, suggested that he thought the subject would be more properly referred to the lay members of the profession for a report.

Dr. Kemp, of Baltimore, moved the adoption of the resolutions appended to the special report of Dr. J. R. Wood, of New York, on Medical Education, and that the committee meet in Louisville on the Monday preceding the session of 1859. Carried.

The report of the special committee on Spontaneous Umbilical Hemorrhage of the newly born, by Dr. J. F. Jenkins, of New York, was then offered and read, and referred to the Committee on Publications.

Following this were read, and similarly referred, reports on the Influence of Marriages of Consanguinity upon Offspring, by Dr. S. M. Bemiss, of Kentucky; on Functions of Different Portions of the Cerebellum, by Dr. E. Andrews, of Illinois ; on Nervous Concomitants of Febrile Diseases, by Dr. H. F. Campbell, of Georgia; on the Treatment of Cataract, by Dr. Mark Stevenson, of New York; on the Treatment of the Results of Obstructed Labor, by J. M. Sims, of New York. The reading of the several other reports was then dispensed with, and they were read by title and referred to the Committee on Publications.

Dr. AtLee, of Pensylvania, from the committee on the subject of preparing a stone for the Washington National Monument, reported that such had been done, that it was of Vermont marble, and was ornamented with a relief representing Hippocrates refusing the presents of Artaxerxes, with the inscription "Vincit amor patriæ." It was prepared by a young native artist, S. Augustus Beck, of Pennsylvania.

Dr. Edwards, from the Committee on Nominations, reported the names of gentlemen on numerous committees, and also a resolution that a committee of nine be appointed to wait on the Hon. Howell Cobb, Secretary of the Treasury, and solicit the restoration of Dr. Bayly to the post of inspector of imported drugs and medicines in New York, and (as added by subsequent amendment) that this application is not to be considered to have a political bearing. In offering this on behalf of the committee, Dr. Edwards addressed the Convention in explanation, alluding to the act of Congress passed some years ago to protect the country from the shameful adulterations of drugs then prevalant. Dr. E. said that it would be better to repeal this act altogether than to leave one hundred and ten millions of annually imported drugs in such hands as it is at present. He spoke of Dr. Bayly's super-eminent ability and competency, and of his having no equal for the place on this continent or perhaps on the other.

Dr. Grafton Tyler warmly opposed the latter branch of the resolution as being highly injudicious and likely to bring politics into the Association, to its great and enduring injury. He implored gentlemen, as they valued the efficiency and success of the Association, to have nothing to do with this matter, which, in his judgment, was solely one for the Executive Government of the country.

Dr. Palmer moved the passage of the resolution so far as regarded the nominations for committees. That branch was then passed unanimously.

Dr. Bolton moved the adoption of the other branch of the resolution.

Dr. Cox, of Maryland, proposed a substitute.

Dr. Edwards said that the present incumbent of this place, upon the proper filling of which the health and safety of the whole country so much depends, was notoriously incompetent. Thus the object of the law of Congress, of which this Asssociation was the real parent, was defeated, and it had better be repealed than administered as it is now. No charge whatever had been made against Dr. Bayly, and the Secretary had stated that he was removed for no other reason than that his place was wanted by somebody else. His successor was recommended for the place by four men, one of whom, Isaiah Rynders, he would name ; they were politicians and acted only as politicians in making the recommendation, whilst in his (the speaker's) view the place was one in which politics should have no concern, and the only question should be as to eminent fitness.

Dr. Tyler replied, urging the impolicy of interfering with political appointments.

Dr. Cox withdrew his substitute and subsequently offered it as an independent resolution, when it was lost.

Dr. Dunbar thought the Nominating Committee had gone out of their way to introduce such a resolution, and moved to lay the whole subject on the table. This motion was lost.

Dr. Parker, of Virginia, offered a substitute for the resolution. Lost.

Dr. Wilcox, of Connecticut, moved the addition before referred to, disclaiming all political considerations in the resolutions. Carried.

Dr. Rogers, of New York, asked if Dr. Bayly may not be incompetent, and would be glad to hear from some members of the New York delegation on that subject.

Dr. Batchelor, of New York, testified, in the most decided terms, to Dr. Bayly's superior attainments and qualifications; he had known him long, and said that there was no diversity of opinion as to his fitness. Would anybody say as much of the other?

Dr. Jewell opposed the resolution. If gentlemen got a change in the New York inspection, he might try to do the same for Philadelphia, and others might try the same for Boston, Charleston, etc.

Dr. Wood, of Philadelphia, rose only from a sense of duty and against his personal inclination to say that he should wish heartily for the restoration of Dr. Bayly, but thought the Association would be setting a bad precedent, and that, instead of making the application for his restoration in its corporate capacity, they should do it as individuals.

Dr. Tyler warmly thanked Dr. Wood for his remarks, calculated to keep the Association on its true course of safety.

Dr. Rogers moved to lay the whole subject on the table. Lost.

Dr. Sayre, of New York, deemed the resolution of the Nominating Committee essential to the preservation of the health and safety of the people of the whole country. The present man did not know the difference between ipecacuanha and a bulbous root.

The resolution was then put and carried by acclamation, but a division being demanded, the vote stood 79 in its favor and 50 against it.

Dr. Bohrer, from the Committee on Voluntary Essays, reported a want of sufficient time for their examination, and prayed their reference to the Committee on Publications. This was adopted, with an amendment by Dr. Condie that only such papers should be referred to the latter committee as the former might read and deem fit for publication.

On motion, Dr. Edwards was appointed chairman of the committee to apply to the Secretary of the Treasury.

The rules were then snspended to allow charges to be brought against the New York Medical College by the Newark, N. J., Medical Association, for putting a most notorious quack into reputable standing by granting said quack their diploma. Similar charges were entered in other cases by Drs. Edwards and Oakley, and all were referred to the Committee on Ethics.

Dr. Sutton, of Kentucky, moved to appoint a committee of three to report a system of uniform registration of births and deaths.

A committee was moved and appointed to urge on the next Census Bureau certain features of improvement in the manner of taking the census. This committee consists of Drs. Miller, Antisell and Garnett, of Washington City.

Dr. Hyam, of Ohio, moved so to amend the Constitution as to prevent all new admissions to the membership of the Association, except the party shall have graduated in some reputable medical college. Lies over to next year.

Dr. Smith, of New Jersey, moved a similar amendment, that only those should be admitted members who have passed examination by a Board of Censors.

Dr. Humphries moved a resolution for an exchange of publications between the different Medical Colleges of the country.

Dr. Campbell, of Georgia, moved resolutions of regret for the decease, and respect for the memories of, Drs. Walton, Ambrose, Mitchell and others.

Dr. Phelps moved the following resolutions, which, being slightly amended, were passed:

Resolved, That the thanks of this Association are eminently due to the Regents and Professor Henry, of the Smithsonian Institution, for the ample and convenient accommodation afforded for the transaction of business.

Resolved, That the Committee of Arrangements are entitled to our praise and highest appreciation of their exertions to promote the comfort of the members, and the best interests of the Association.

Resolved, That to the Physicians of Washington and Georgetown, and the Faculty of Georgetown College, we accord the homage of our sincerest thanks for their elegant hospitalities extended to the members from abroad, by which the pleasure of their sojourn here has been so greatly enhanced.

Resolved, That we feel assured that the impressions on the tablet of memory received here, in our National Metropolis, in this, the first year of the second decade of the Association, will long remain an evidence of the urbane attentions received, not only from the Chief Magistrate and other public functionaries of our glorious Union, but of private citizens and the community at large.

Resolved, That the manifestations of union of heart and purpose in the action of this session inaugurate a new era, and call for devout acknowledgment to Divine Providence, and presage, as we trust, not only a bright future for the Association, but also as contributing to the perpetuity and prosperity of our great National Confederation.

Permission was granted Dr. Hamilton to use his publications in the Society's Transactions, in the year 1860, for his own private use.

Dr. Arnold described a new and effective method of preserving pathological specimens, and exhibited samples, which brought much applause.

On motion of Dr. Condie, the Convention heard Dr. Gaston, of Columbia, South Carolina, in explanation of his new Uterine Supporter.

On similar leave, Dr. Parker, late Missionary to China, exhibited thirty-eight specimens of urinary calculi, extracted by him in Canton, China, in the course of his residence there.

On motion of Dr. Miller, of the District of Columbia, it was resolved that two copies of the Transactions of the Association be transmitted to the British Association, with a request for an interchange.

A reconsideration of the subject of the appointment of a committee to call on the Secretary of the Treasury in respect to restoring Dr. Bayly to to his former post of inspector of drugs in New York was moved, but the Convention called for Dr. Parker's remarks on the calculi. He said that the specimens were thirty-eight in number, and that out of all upon whom he had ever operated only five patients were lost. He had had in the hospital, and under his charge in Canton, fifty-three thousand patients, including members of the imperial family, imperial commissioners, and through all grades of the better classes down to the poorest. He had operated in one thousand cases of cataract, and in most with perfect success. He referred also to a series of some thirty or forty paintings of cases of tumors and scrofulous swellings, exhibited in the lower hall of the Smithsonian building, which had come under his inspection. In conclusion, Dr. Parker said he would some day publish an account of his medical labors in China, in all its departments. [Applause.]

The thanks of the Convention were then voted to Dr. Parker, and the request was made that he would publish his work as early as possible. The hope was also expressed that members would patronize its sale.

The reconsideration of the case of Dr. Bayly on the past resolutions from the Nominating Committee was then moved and debated.

Dr. Hayne, of Virginia, said he thought the gentlemen who so much disclaimed politics were just the most political gentlemen in the Convention. Had he not been told there was politics in the resolution he never should have dreamed of such a thing. He was as much against the introduction of politics in the Convention as any man, though he was a Democrat of the strictest sect, and had gone it pretty strong for James Buchanan. Yet in this matter, and because he could see no politics in so doing, he would go with the resolution, and so would the State of Virginia *in toto.*

Dr. Tyler was almost inclined to do any thing to please his honest Virginia friend, but really must deprecate the introduction of politics here. He reaffirmed his previous views.

Dr. Morgan, of the District of Columbia, strongly urged the political character of the resolution, and said that he had understood from friends around that Dr. Myrtle was better qualified than Dr. Bayly.

Dr. Batchelor said this could be no political movement. Both of these men were Democrats; one of them (Dr. Bayly) was eminently qualified all knew, but who would undertake to say the other was?

The subject was further debated by Drs. Watson, Burns, and others, when Dr. McCarty, of New York, stated that he had been informed that Dr. Myrtle is a German, knows four or five different languages, reads the invoices in them all, and makes the analysis himself, which, as he had heard, Dr. Bayly did not, being incompetent thereto.

Dr. Atkinson, of Virginia, pledged more than a million of the people of Virginia in support of the resolution. It is a matter of life and death to the whole people. Dr. Bayly had been faithful, and in the name of the State of Virginia he called for his restoration.

Dr. Wood, of New York, said that in New York there were different sections in the Democratic party, and these different interests had ousted Bayly and put in Myrtle. The latter appointment was contrary to the wish of the profession in New York and of the New York College of Pharmacy.

The vote on reconsideration was then loudly called for and put: for it 51, against it 32.

Dr. Atkinson moved to postpone the subject till there is a fuller house and to lay it on the table; but another member proposed to postpone indefinitely; which last motion was carried.

The Convention then adjourned to meet at 5 o'clock.

At 5 o'clock the Association again met, but the attandance was thin. The proposed amendments to the constitution were deferred to another year. A proposition to appoint a permanent Secretary was debated, but no final action taken, when the Convention adjourned *sine die.*

SPECIAL SELECTIONS.

EXPERIMENTAL RESEARCHES ON THE USE AND PHYSIOLOGICAL PROPERTIES OF RED AND BLACK BLOOD, AND THEIR PRINCIPAL GASES.

By Dr. Brown-Sequard, of Paris. Translated for the New Orleans Medical News and Hospital Gazette, by Anthony Peniston, M. D., Prof. of Physiology, N. O. School of Medicine.

The investigations, of which we now publish the principal results, were commenced more than ten years ago. As far back as 1848, a few months after the foundation of the Society of Biology, I had already laid before that learned assembly some of the facts, whose interpretation I now propose to consider. Encouraged by the distinguished President of that Society, Dr. Rayer, I have continued my researches almost uninterruptedly since that time. I have already published in the Memoirs of the Society of Biology, and in the "Comptes Rendus of the Academy of Sciences," a summary of the principal results which I have obtained, but without entering into details, or drawing those conclusions which are naturally deducible from them. We shall now bring together facts observed long ago, with those since discovered, and thus we shall be better prepared to appreciate the relative importance of the conclusions to be drawn from the premises in question.

I shall not attempt a long historical introduction to the questions we now set forth, nor to the facts upon which we have based our conclusions, for they are either entirely new, or obscurely hinted at previous to my investigations.

The admirable researches of Haller on the cause of the heart's motion have given the sanction of experiment to an idea current before his time, and which has generally been admitted as true even by those who denied the correctness of the theories which this great Physiologist propounded on the heart itself. We are now speaking of the function of the blood considered as a stimulant, for there is scarcely a physiologist who does not consider the blood as the natural exciting agent of the brain and various other organs. Yet this theory makes no distinction between two different functions of the blood, viz : its nutritive and its stimulating properties, though it be important, as we shall show, to discriminate between them. Few attempts have been made to establish the difference between arterial and venous blood ; it was only generally asserted that arterial blood is the stimulus of the brain and other organs, while, on the contrary, venous blood was supposed to possess either no stimulating properties at all, or far less than red blood. Therefore it is that the celebrated physiologist of Berlin, Jean Müller, attributed the cause of the respiratory movements to the stimulation of the medulla oblongata by means of the oxygen of arterial blood.

In the same way Goodwin attributed the cause of death in asphyxia to the black blood, which is incapable of stimulating the left side of the heart. Again, Marshall Hall admitted that venous blood could only stimulate the right side of the heart, whose irritability was much greater than the left ; for the latter could only be stimulated by arterial blood. Moreover, it has been asserted that the oxygen contained in arterial blood is the exciting principle of all the different organs, and from the discovery of oxygen to this day this opinion has been almost universally admitted.

As to venous blood, it is generally believed that it has no stimulating properties, and no special purposes in the system ; it has even been asserted that when black blood circulates in the arteries, it can no longer support life ; and Bichat supposed that it exercised a deleterious influence.

Nevertheless, some authors have attributed a stimulating agency either to venous blood, or to the carbonic acid which it contains. Thus John

Reed. (Physiological, Anatomical and Pathology, research, 1848, p. 285) and Marshall Hall (New Memoir on the Nervous System, 1843, pp. 69–80) admitted that the respiratory movements in asphyxia are excited by the black blood acting on the medulla oblongata. In the same manner, Volkman and Vierordt think that the respiratory movements are due to a reflex action of the medulla oblongata, caused by a stimulation of the sensitive nerves throughout the body, by the venous blood. On the other hand, Marshall Hall explained the normal respiratory movements to the excitement of the filaments of the pneumogastic nerve, by the carbonic acid contained in the bronchial ramifications. Moreover, Erichsen was disposed to admit that, in asphyxia, the blood loaded with carbonic acid acts as a stimulus to the muscular coat of the small pulmonary vessels, and brings on the contraction which stops the circulation. Finally, Marshall Hall has expressed his opinion that convulsions in asphyxia and epilepsy are mostly due to the excitement of the pneumogastric nerve by the carbonic acid of the lungs.

I shall not stop to consider, for the present, the fallacies which are contained in the above opinions. I simply cite them to show that very few physiologists have pointed out the stimulating properties of venous blood, and that even by those who did admit this property, it was supposed to be limited to a few organs. I may add that not one experiment has been adduced in support of the views which were thus advanced.

As to the influence of venous blood on nutrition and vital properties we shall have little to say. Every one knows that, according to Bichat, venous blood is incapable of supporting life. On the other hand, this opinion has been strenuously denied by W. F. Edwards, in an admirable work entitled "A Treatise on the Influence of Physical Agents on Life;" and afterwards by J. P. Kay and Erichsen.

As to red or arterial blood, every one knows that it is necessary for the different vital functions; and I shall also show that even when these functions are entirely suspended, either in nervous or muscular tissue, they may still be renewed by means of oxygen contained in red blood.

I shall attempt in this memoir to demonstrate the physiological properties of both kinds of blood, and to show that these properties are dependent upon the oxygen and carbonic acid which they relatively contain. Among others, I shall more particularly attempt to establish and demonstrate the following propositions, viz:

1. That the blood possesses two distinct physiological properties: one

by which nutrition and the vital functions of tissues are maintained; the other by which the different tissues and organs are stimulated to action.

2. Arterial and venous blood only differ from each other in their physiological properties on account of the proportions of oxygen and carbonic acid which they relatively contain.

3. All contractile tissues, after having lost all their vital properties, may recover them under the influence of blood containing oxygen.

4. All nervous tissues, after having lost their vital properties, may recover them under the influence of blood containing oxygen.

5. When the brain has lost its functions it may recover them by means of blood containing oxygen.

6. All nervous or contractile tissues are susceptible of being stimulated by blood containing carbonic acid; certain organs being more easily and more energetically stimulated than others.

7. The convulsions in asphyxia appear to be caused by the blood containing a great deal of carbonic acid, acting as a stimulus on the nervous centres, the nerves themselves and the contractile tissues.

8. Many of the phenomena which are observed in an epileptic attack appear to be dependant upon the stimulation which blood containing carbonic acid exercises upon the cerebro-spinal axis, and other organs possessing contractile tissues.

9. In profuse hemorrhage the cause of convulsions and tremors appear to be caused as in asphyxia, by the stimulating action of carbonic acid upon the nervous centres.

10. The respiratory movements and the motion of the heart, appear to require the presence of a certain amount of carbonic acid.

11. In parturition, and other cases, the contractions of the uterus are brought on by the stimulus of carbonic acid.

12. The movements which are sometimes seen in the limbs of subjects which died from Cholera are dependent upon the stimulus exercised by the accumulation of carbonic acid.

13. We can produce at will two states of the organism entirely different from each other; one caused by the presence of an excess of oxygen in both venous and arterial blood; the other owing to an excess of carbonic acid in both bloods. In the former case life ceases, notwithstanding the excessive

energy of the vital functions, because the stimulating property of the blood is deficient, whilst in the latter case these stimulating properties being excessively increased, the vital functions are rapidly exhausted through want of nutritive materials.

In order to establish the first proposition, we must now explain what is to be understood by the words *physiological properties* of blood—we mean by these words to say, that blood has the property of changing the physiological state of the tissues, whether it be physically, chemically, or in any other manner whatsoever. It especially causes two different modifications in the various tissues; by one of these the vital properties of tissues are increased; by the other these tissues are stimulated to action. We omit, for the present, the influence of the blood upon the various secretions, though these functions, which are so exceedingly complex in their nature, from their partially chemical and physiological attributes, still afford ample ground for investigation, notwithstanding the admirable researches of Claude Bernard, of Ludwig, and other Physiologists.

In reference to secretions, however, we may point out the following facts, viz: 1st. That certain secretions are considerably increased under the influence of blood saturated with carbonic acid, the cause of this being, probably, the stimulation of the nervous centres, the state of the blood vessels and other contractile parts in the glandular structures; or, perhaps, some chemical modifications of the cell walls under the influence of venous blood. 2d. On the other hand, when there is an excess of oxygen in the blood the secretions are, for the most part, either diminished or suspended.

In the present memoir we shall confine our attention to those tissues or organs which are endowed with dynamic properties, viz: nervous and contractile tissues. The nutrition and repair of those organs, or, in other terms, the maintenance of their vital attributes depends upon the oxygen of the blood, as will be shown further on. But it must not be supposed that all the nutritious properties of blood consist in oxygen, but simply that oxygen is the indispensable medium of those exchanges which take place between the component elements of the blood and the various tissues; so that under this point of view we may define nutrition to be the result of the exchanges which take place between the blood and the various tissues, in order to maintain their vital functions. Nutrition is suspended when the blood no longer contains the necessary amount of oxygen.

Thus the blood possesses two distinct physiological properties: one by which the different nervous and contractile tissues derive their vital properties, and the spontaneous principle of action which appertains to one of these; on the other hand, the other property of blood is that by which the different nervous and contractile tissues are stimulated, excited, and caused to manifest or exercise their various vital functions.

In other words, carbonic acid is an exciting agent, like the external application of heat, cold or electricity, produces stimulating properties on those tissues with which they come in contact. So that, on the one hand, the oxygen of the blood possesses the property of maintaining the vital properties of the nervous and contractile tissues, and even of regenerating those properties for a certain period after they have disappeared; on the other hand, the carbonic acid of the blood is the especial stimulus of nervous and contractile tissues.

Through this peculiar property possessed by the blood, of nourishing contractile and nervous tissues, they obtain the *faculty of acting*, or the power of accomplishing the acts which they are intended to perform. From the other property of the blood, viz: its stimulating power, is derived the source of action in nervous and contractile tissues, or the manifestation of those acts which they perform as organized bodies. By one of these properties the blood gives the faculty, the power of action, but not action or activity itself. The other, on the contrary, begets action, by which, of course, the faculty of action is diminished proportionally. The first of these properties may be said to give the power of life, the second causes life to manifest itself in its various ways.

It follows, again, that as blood owes to its oxygen the first of these properties, and to carbonic acid the latter, that oxygen is an essential agent to the production of vital properties in nervous and contractile tissues, and carbonic acid the means of stimulation to the various tissues, in order that those properties may be called forth.

Certain organs require a great deal of oxygen for the maintenance of their vital properties, such as the brain and spinal marrow; others, on the contrary, as the muscles and different contractile tissues require very little. As to the stimulating property of blood, some organs, as the heart and medulla oblongata, are sufficiently excited though the blood contain very little carbonic acid, but a large quantity is required for the stimulation of the muscles of animal life, and of many contractile tissues.

Normal arterial blood, on account of the considerable quantity of oxygen which it contains, is endowed with the power of producing and maintaining the vital properties in the nerves and contractile tissues, but the carbonic acid which it contains in considerable proportion is the means of stimulating the heart and contractile tissues. Thus it is that the blood possesses the double function of producing vital functions, and of exciting them to action; it gives the power, and then causes the waste of that power by exercising it; it begets at once the faculty of action and the exercise of that faculty. We shall show that when blood contains an excess of oxygen, but a deficiency of carbonic acid, then the vital properties are considerably increased, but its stimulating powers disappear almost completely.

As to normal venous blood, it cannot maintain in the brain the faculty of spontaneous action which appertains to this organ, viz: the different intellectual operations and acts of volition; nor can it maintain in that organ the power of perceiving sensations and of comparing them with each other; it appears only able to maintain for a very short time the vital properties of moter nerves and of muscular tissue. But it has not the power of regenerating those vital properties when they have completely disappeared. Venous blood has the power of stimulating the heart and medulla oblongata much more than arterial blood, and on account of the carbonic acid which it contains, it can stimulate the pneumogastric nerves, the spinal cord, the uterus, intestines, etc., whereas arterial blood has no such power. In the normal state however, it does not contain sufficient carbonic acid to stimulate energetically the latter organs, and therefore does not act as a stimulant to muscles of animal life; but in asphyxia it may become changed with such an increased amount of carbonic acid that it acquires the property of stimulating all contractile tissues.

Proposition II. The essential difference between venous and arterial blood, as to their physiological properties, consists in the different proportions of oxygen and carbonic acid which they relatively contain.

The following experiment will demonstrate the above proposition:

Exp. 1. About eight ounces of arterial, and the same qnantity of venous blood were drawn from a stout, healthy dog; both kinds of blood were beaten with a bunch of sticks, in order to remove the fibrine and charge them with oxygen. Both kinds of blood thus became of the sam scarlet color, whereupon the arterial blood was injected into the right femoral artery, and the venous blood into the left femoral artery of a rabbi which had been dead for about an hour, and whose lower extremities were already stiffened from rigor mortis about ten minutes previously. Both injections were made at the same time and produced the same effects simultaneously; the rigid state of the limbs ceased after five or six minutes; at the same time muscular irritability reappeared, and was manifested by electric and galvanic excitement. After the injection, muscular irritability persisted for the same length of time, viz: about twenty minutes, in both limbs.

This experiment proves that both kinds of blood, in similar circumstances, that is to say, when they contain the same amount of oxygen, have the same physiological effects. It appears, therefore probable, that we must attribute the differences in the physiological properties of venous and arterial blood to the different proportions of these two gases which they respectively contain. If, instead of the preceding method, the blood be defibrinated, and then the oxygen be removed by replacing it either with carbonic acid or hydrogen, we find that arterial blood modified in this manner cannot restore muscular irritability; however lately it may have disappeared. Venous blood acts in the same manner, so that if it be defibrinated and charged with oxygen, and then the oxygen be replaced by carbonic acid or hydrogen, it thereby loses the property of regenerating muscular irritability.

The two kinds of blood act, therefore, in the same way, so far as muscular irritability is concerned, when they have been put in the same conditions in relation to the gases which they contain. The same result is obtained, if, instead of acting on muscles, the same experiment be performed in order to restore the spontaneous actions of the brain, or the vital properties of the spinal cord and medulla oblongata; for when oxygen and arterial blood have been defibrinated and charged with oxygen they restore the various properties so characteristic of those organs; but if on the contrary the oxygen be subtracted no result whatsoever is obtained.

We may therefore conclude that oxygen is essentially the medium of nutrition in arterial blood, and the greater quantity existing in this species of blood accounts for its predominence in that respect. But venous blood owes to carbonic acid its greater stimulating qualities, and the following experiment shows that arterial blood if charged with carbonic acid may possess even greater stimulating properties than venous blood, if the latter contains a less amount of the same gas.

Exp. 2. The following experiment was performed upon two female rabbits in gestation, and within two or three days of their term. We opened the abbomen, and on one we injected into the aorta arterial blood, deprived of fibrine, and afterwards charged with carbonic acid; on the other venous blood was injected, deprived of fibrine, but containing less carbonic acid. In about two minutes the uterus of the first rabbit began to contract, and in about three more minutes one fœtus was expelled, and four minutes afterwards two more fœtuses were born. The uterus of the second rabbit, upon which we injected simple venous blood, only commenced to contract after five minutes, and ten more elapsed before one single fœtus was expelled. We then modified the experiment: both of these animals were asphyxiated by putting them in an atmosphere of carbonic acid, and the result was the expulsion of another fœtus from each female.

We think that this experiment fully demonstrates the fact that the exciting power of the blood resides in the carbonic acid which it contains, and that arterial blood can possess the same property if it be loaded with carbonic acid, by simply displacing the other gases.

The following experiment shows that when venous blood contains a great deal of oxygen it no longer possesses the same stimulating qualities.

A rabbit was put to death by asphyxia with carbonic acid, and the abdomen being rapidly opened, the intestines were seen animated with violent and rapid movements, characteristic of asphyxia. An injecting pipe filled with venous blood, containing an excess of oxygen, was introduced into one of the mesenteric arteries, and immediately the vermicular movements stopped in all that portion of intestinal tube in which this blood circulated. Having changed this blood for another kind containing an excess of carbonic acid, the movements immediately returned.

(*To be Continued.*)

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EDITORIAL AND MISCELLANEOUS.

American Medical Association.—We give our readers elsewhere a summary of the proceedings of the American Medical Association, lately in session at Washington City. This summary is extracted from late numbers of the *National Intelligencer*, and, although necessarily incomplete, may be considered as authentic, as no doubt it was prepared under the supervision of the Secretaries. We shall await with anxiety more complete reports, and will present the same to our readers at the earliest possible date.

It will be seen that Drs. Reese, of New York, and Bryan, of Philadelphia, have fully apologized for having recommended James McClintock to the Governors of the Blockly Alms House.

Dr. Wood, of New York, Chairman of the Committee on Medical Education, presented his report. He recommends the extension of the lecture term to six months, not more than four lectures daily, and not less than seven Professorships. He also recommends a Convention of the Medical Schools, to meet at Louisville next Spring, in advance of the meeting of the Association, and to report to the Association some uniform system of education.

From the signs before us, we think great and good results will grow out of this last meeting of the Association. As we have ever done, we look to the American Medical Association as the fountain from which shall flow the living waters, which will not only refresh, but will impart new life to the profession of medicine in this country.

NON-LIABILITY OF SCIENTIFIC MEN TO BE SUBPŒNAED TO GIVE EVIDENCE.—From the *Medical Times and Gazette,* for March, we perceive that Lord Campbell has recently given a judgment at Warwick to the following effect, viz :

" A scientific witness having asked his opinion as to whether he was bound to attend upon being served with a subpœna, he would say that a scientific witness was not bound to attend, and ought not to be subpœnaed. If he knew any question of *fact,* he might be compelled to attend, but her Majesty's subjects were not compellable to give their attendance to speak on matters of *opinion.*"

This is certainly a very important little item to medical men. If there is a nuisance connected with the fact of being a medical man, it is that we are at all times liable to be picked up on the street and hauled off to Court to give scientific *opinions* in cases in which we have not the slightest interest, and from the parties engaged in which we receive no remuneration whatever for our detention and trouble. If there is a correct principle among men, it is that which teaches that the laborer is worthy of his hire. If scientific men are *necessary* in Courts of Justice, then they are as much entitled to pay for their service as are the lawyers who question them, and our laws should be so amended as to compel parties concerned to pay them. We have always felt rebellious against this system of extortion of gratuitous services from medical men—for it is the medical man who is mostly preyed on—and we earnestly believe that if the prevalent principle were tested before our higher Courts the matter would be decided in our favor. At the very moment that we write a case in point occurs to demonstrate the absurdity and injustice of the present practice. A woman is found dead; a physician is called in to make a post-mortem examination; he makes it and his opinion is carried before the Court because another party is accused of having caused the death. But other medical testimony—in the shape of *opinion*—is deemed necessary, and another physician is stopped from his business—actually detained from doing his duty to himself and his patrons—and hauled off to the Court to give these opinions, and with no earthly prospect of being paid one dime for his detention and services. Is there any show of justice in this? No! medical men are as liberal in the gratuitous dispensation of their services as any men living, and it is their pride to be so; but there is reason in all things, and we are for rebellion against this abuse of our services and our good nature.

MEDICAL REQUIREMENTS IN CUBA.—A friend has permitted us to make some extracts from a private letter lately received from an esteemed medical friend in Cuba, in relation to the ordeal through which a man must pass before he can pursue the practice of medicine on the island. Speaking of the admissability of a gentleman recently from the United States, he says:

"The laws require seven full years' connection with the profession, as pupil or practitioner, to be able to *apply* for an examination. To practice without a license is a fine of one hundred dollars for the first offence, two hundred for the second, and to persist after that the offender is subject to arrest and to be sent from the island. If the applicant fails to show the necessary time, there is nothing more to be said or done in the matter. All documents must have the certificate of a Spanish Consul, otherwise they cannot pass for more than so much waste paper.

"Examinations must be in Spanish. Interpreters are not permitted. These preliminaries being settled, application must be made to the Captain General to direct the Government of the University to proceed to the examination. This order goes to the Secretary of the University, with whom two hundred and fifty dollars must be deposited, after which the examination takes place.

"If so much be required to be able to knock at the door of the profession, the entrance is, also, well taken care of. The preliminaries being settled, the examination opens in the chapel of the University, in O'Rielly st., opposite the notorious coffee-house, the Dominica. The first session embraces the theory and practice of medicine, something of its history and objects, and a glance at the ancient founders—the plain object being to determine the character of the candidate as a student or scholar. The next session is purely clinical, in one of the hospitals. A number of beds is set apart, and the candidate is required to write the diagnosis, therapeutics and prognosis of each case. He then returns to the chapel to be questioned on whatever he said, believes and saw, and the reasons and authorities for his opinions and practice. If satisfactory, the next session is in the surgical wards, the operating hall, or the dissecting room—perhaps in all—or at least so far as to prove, without any doubt, qualifications or deficiencies sufficient to approve or condemn.

"I am confident you will not object to any part of these exactions. No one knows better than yourself, that a person possessed of qualifications will show them by the bedside, or at the side of a cadaver, and that questioning alone might not detect a tyro. In this respect, I hope you will concede something good even in this Nazareth."

MEDICAL STUDENTS IN THE UNITED STATES—*Session of* 1857–58. We continue to furnish our readers the returns of the Medical Schools as they come to hand. We are indebted to the May No. of the North American Medico-Chirurgical Review for the following:

	Students.	Graduates.
Jefferson Medical College	501	209
University of Pennsylvania	435	145
University of New York	—	127
University of Nashville	353	109
University of Louisiana	276	68
Georgia Med. College	—	61
College of Physicians and Surgeons	—	53
St. Louis Medical College	125	49
Medical College of Ohio	—	43
Bush Medical College	100	36

Pennsylvania Medical College	140	35
New Orleans School of Medicine	126	34
New York Medical College	—	33
University of Louisville	—	31
University Medical College	—	27
Missouri Medical College	—	25
Memphis Medical College	—	19
Philadelphia College of Medicine	63	18
Massachusetts Medical College	—	16
Transylvania University	—	12
Oglethorpe Medical College	37	11
Kentucky School of Medicine	—	11
Starling Medical College	—	10
University of Buffalo	—	9
Medical Department of York	—	6
Medical College of South Carolina	216	—
Richmond Medical College	60	—
University of Virginia	88	—

We have no desire to make unpleasant comparisons, but it is such a source of gratification to us to see in the above list the evidences of the success of the institution to which we are attached, that we cannot refrain from directing attention to the fact. In the above list will be found returns—more or less complete—from twenty-eight Medical Schools; with one of the first eleven of these, the New Orleans School of Medicine is nearly equal, and she is ahead of fifteen out of the twenty-eight; and this in the second year of her existence, with difficulties surrounding her which she can never know again.

New Medical Journal.—We are in receipt of the first number (May, 1858) of *The Savannah Journal of Medicine*, edited by Drs. J. S. Sullivan and Josiah Harris, with Dr. R. D. Arnold, as associate editor. This Journal is published every two months, each number containing seventy-two pages of matter. It is issued in excellent style, and we trust that it may meet with all the success it deserves.

Rape and Impregnation.—In our original department the reader is furnished with some very interesting matter on the subject of impregnation. We have thought it by no means inappropriate to introduce into the same number a very interesting account of "Evidence afforded by the Microscope in a Case of Rape," which we copy into our Excerpta Department, from the late number of the *Boston Medical and Surgical Journal.*

THE PHILADELPHIA COLLEGE OF PHARMACY graduated thirty-one gentlemen recently.

BUFFALO MEDICAL JOURNAL.—Dr. Sandford B. Hunt has retired from the practice of medicine, and this Journal is now conducted by Dr. Austin Flint, Jr., who gives fair promise of ample ability to discharge the onerous duties.

BROMIDE OF POTASSIUM IN SPERMATORRHEA.—M. Binet recommends the use of this medicine, in doses of ten to thirty grains, as a remedy for this distressing and obstinate affection, spermatorrhea.

ADMINISTRATION OF QUININE TO PREGNANT WOMEN.—There are those who believe that quinine should not be administered to pregnant females. Dr. J. J. West, of Savannah, Ga., furnishes a very interesting article on this subject in the first number of the *Savannah Journal of Medicine.* He insists that the medicine exerts no such injurious influence as has been attributed to it; on the contrary, he asserts that it may be made a preventive of abortion by putting a stop to the repeated paroxysms of intermittent fever, which, of itself, tends to produce this accident.

NEW TREATMENT OF PROLAPSED FUNIS.—Dr. S. Gaillard Thomas, of New York, has recently read before the New York Academy of Medicine, a paper describing a new method of restoring, and keeping in position, the prolapsed funis. In a few words, the remedy consists in placing the woman on her hands and knees, when, of course, the fundus uteri becomes the most dependent portion, and then the tendency of the cord (more especially when aided by the hand of the accoucheur) is to fall from the os towards the fundus. If necessary, the patient may keep this position until the head of the child becomes engaged in the pelvis, and the danger of prolapsus is over.

This is a very ingenious idea of Dr. Thomas, and we shall certainly try the experiment when occasion offers. One thing, however, we must say in passing; we are sorry to see that he has fallen into the same error as Dr. Sims, in supposing that atmospheric pressure has anything to do with the matter. Dr. Sims, strange to say, imagines that atmospheric pressure opens the vagina when the woman is placed in this posture for operation for fistula, and now Dr. Thomas actually says, that "the uterus and the intestines falling forwards, a partial vacuum is created, and the air rushing into fill it, tends to carry the cord before it." If Drs. Thomas and Sims will think this matter over again, we feel assured they will abandon the atmospheric pressure idea.

Since writing the above, we have received the May Number of the *New York Journal of Medicine*, and in it is the following correspondence:

Mr. Editor: In an essay put forth by me upon the treatment of "Prolapse of the Funis," by position, aided by manipulation, I find that I have stated one or two points in such a way as not to have conveyed the meaning which I intended. I am, of course, anxious that the method advanced should prove efficient, and would thank you to aid me in removing some of its chances of failure, by publication of this note.

1. I am convinced that the introduction of the whole hand into the vagina in returning the cord is *essential to success; the fingers alone will fail,* more surely when the position of the presenting part is altered by the change of attitude advised, than they would do when the patient is on the back. The cord should then be pushed entirely beyond the part presenting.

2. The position alone will rarely, if ever cause the return of the cord, without the aid of manipulation, unless the bag of waters is unbroken, and even then it may not."

These two propositions admitted—and they will surely be admitted by all who have any experience in cases of prolapsed cord—and the atmospheric pressure idea falls to the ground.

CUPPING AND LEECHING THE UTERUS.—In the *Nashville Journal* for May is a communication from Dr. G. T. Stull, of Somerville, Tenn., on the subject of cupping the uterus. Dr. S. says he has been in the habit of cupping this organ with a "vaginal glass syringe tube" fastened to "the gum elastic bulb from a common breast pump." With this, the Doctor says, he has "never failed to get blood quickly." But, of leeching the uterus, the Doctor says, "I must confess I never could get a leech to bite those parts in all my life." Surely there must be some peculiarity about either the leeches or the uteri of West Tennessee. Here we never fail to get leeches to bite. Only yesterday we applied eight, and every one of them sucked greedily. We should be sorry to exchange good leeches for a glass tube with a breast pump bulb at the end of it.

Rupture of the Vagina.—*The California Case Again.*—In the late number (April) of the *Pacific Medical and Surgical Journal*, Dr. James Blake, F. R. C. S. E., reviews the great rupture case which created such a stir in Sacramento a year or two ago. The Doctor seems to be writhing under the effects of the unenviable notoriety he acquired at the time of the occurrence, and at this late day renews his efforts to prove that Dr. S. did actually rupture the vagina through ignorance. We have no desire to wade through the rigmarole of evidence adduced, but present the reader with one of his wise conclusions, viz: that "the hand (of Dr. S.) was attempted to be introduced *into* the uterus, to peel off what was supposed to be an adherent placenta, but passing *behind* it, the vagina was peeled off from its posterior surface instead, and immediate symptoms of collapse manifested themselves."

Does not the idea seem preposterous? As we have before said, no man an use the force necessary to inflict such an injury, without causing a legree of pain which would at once elicit the loudest complaints from the atient. And we now say even more. Let an honest inquirer after truth ake these parts from the human body, and attempt (with the hand passed n the direction which it must assume on the living subject,) to separate he vagina from the posterior portion of the neck of the uterus; and we onfidently predict that either separation or rupture of the vaginal wall will e found sufficiently difficult to settle the point against Dr. Blake. It is ne thing to talk about *peeling* off the vagina and another to do it.

Necrology.—Death has been doing its sad work amongst the good en of our profession lately. We are called on to record the deaths of rof. J. K. Mitchell, of Philadelphia; Drs. Benjamin Travers and Forbes oyle, of Great Britain; Prof. J. K. Walther, of the University of Leip-g; Dr. Schaff Merei, of the University of Pesth, and Mons. L. Baudens, f Paris.

Changes in Medical Schools.—The death of Professor J. K. Mitchell, the Jefferson Medical College, is announced; Prof. S. H. Dickson, of e Medical College of South Carolina, has been elected to the vacant air of Theory and Practice.

Prof. Geddings has resigned the chair of Surgery in the Medical College of the State of South Carolina, and Dr. J. J. Chisholm, of Charleston, has been elected to fill his place. Dr. P. C. Gaillard, of Charleston, has, also, been elected to the chair of Theory and Practice, vacated by Prof. Dickson.

Prof. Miller has resigned the chair of Obstetrics in the Medical Department of the University of Louisville.

Prof. R. K. Thomas, "in consequence of ill health," has resigned the chair of Obstetrics in the University of Maryland. Dr. G. W. Miltenberger, Prof. of Materia Medica, in the same institution, has been appointed to the vacant chair, and Dr. Charles Frick has been appointed to Dr. M.'s chair.

Dr. Joseph Jones, late Professor of Chemistry in the University of Georgia, has been appointed to the same chair in the Medical College, at Augusta. We congratulate our friends of Augusta on the occasion of their having secured the services of this eminent gentleman. Dr: Jones ia rapidly making his mark in the medical world.

List of Payments to May 26th, 1858.—Drs. B. H. Moss, $5, vol. 5; S. H. Hazard, $5, vol. 5; Wm. Rushton, $5, vol. 5; C. L. Smith, $5, vol. 5; W. W. Brown, $5, vol. 4; H. G. Bowen, $15, vols. 3, 4 and 5; S. F. Abbey, $10, vols. 4 and 5; W. P. Smith, $5, vol. 5; Harrington, $5, vol. 5; R. G. Wharton, $5, vol. 5; G. E. French, on acct. $5; Gourrier, $18, vols. 2, 3, 4 and 5; W. L. Cook, $5, vol. 5; A. Hensly, $5, vol. 5; Mr. Grorart, $5, vol. 5; Dr. J. Hays, $5, vol. 5; G. O. Stark, $5, vol. 5; R. Bein, $5, vol. 5; S. Cartwright, $5, vol. 5; John Morrison, $5, vol. 5; C. J. Bickham, $5, vol. 5; D. S. Mills, $5, vol. 5; J. Calderwood, $5, vol. 5; W. B. Pollard, $5, vol. 5; Wm. Kennedy, $5, vol. 5; M. Schuppert, $5, vol. 5; H. McGuffey, $10, vols. 3 and 4.

To our Louisiana Country Subscribers.—Cannot our friends in the interior of the State come to our assistance pecuniarly? We have recently sent set bills to eighty of them, who owe us, in the aggregate, the large sum of $900! Is not this a one-sided business?

BOOKS AND PAMPHLETS RECEIVED.

"The Institutes of Medicine. By Martyn Paine, M. D., L. L. D., Prof. etc., etc. New York: Harper and Brothers, Publishers. 1858." A very valuable book, and one which we hope to notice more fully hereafter.

"Elements of Inorganic Chemistry, including the application of the Science in the Arts. By Thomas Graham, F. R. S. L. and E., late Professor of Chemistry in University College, London. Edited by Henry Watts, B. A., F. C. S., and Robert Bridges, M. D. Second American, from the second revised and enlarged London edition. Complete in one volume. With two hundred and thirty-three illustrations on wood. Philadelphia: Blanchard and Lea. 1858." The very best work on Inorganic Chemistry extant, and published in the best style.

"Plates illustrative of Wilson on Diseases of the Skin. Fourth edition. Philadelphia: Blanchard & Lea. 1857." We do not pretend to be a connoisseur in the matter of pictures, paintings, and such things; but, from our knowledge of the appearance of diseases of the skin, as derived from personal observation on the living subject, we are compelled to pronounce these colored plates very poor indeed.

"University of New York. Medical Department. Annual Announcement of Lectures. Session 1858–59. With a Catalogue of Graduates."

REPORT OF DEATHS IN THE CITY OF NEW ORLEANS, FROM THE 18TH OF APRIL TO THE 23D OF MAY, 1858.—Men, 221; women, 102; boys, 189; girls, 188. Adults, 323; children, 377. Males, 410; females, 290. Total, 700.

Under 1 year, 185; 1 to 2 years, 78; 2 to 5 years, 48; 5 to 20 years, 74; 20 to 40 years, 181; 40 to 60 years, 118; over 60 years, 25.

Consumption, 100; still-born, 33; cholera infantum, 18; diarrhœa, acute and chronic, 29; scarlet fever, 13; convulsions, infantile, 40; congestive fever, 10; marasmus, infantile, 26; croup, 18; dysentery, acute and chronic, 22; typhoid fever, 18.

It will be seen that the mortality of our city has been large during the five weeks ending on the 23d of May. There has been an average of 140 deaths per week. The last week there were 147 deaths. The reader will at once be struck with the enormous mortality from Consumption—an average of twenty per week—a large majority of these are the unfortunate strangers, who have been sent hither to get *well* of the disease. The still-born list keeps up to the standard it must ever maintain while ignorant midwives are permitted to practice.

EXCERPTA.

THE CURABILITY OF CONSUMPTION.—From a monograph of undoubted merit, entitled *Assimilation, Consumption, and Scrofula,* by Dr. B. H. Washington, of Hanibal, Mo., we abstract the following. Each point Dr. W. has well elaborated, and each position has been well proven :

According to Prof. Gross, one-third of the deaths occur from tuberculous diseases, and in view of this fact it behooves every one to take especial notice of everything on this subject, and to endeavor to prove as speedily as possible the good results likely to flow from any plan of treatment which may be recommended. Dr. W. offers the following propositions :

"1. Consumption and scrofula are caused by impurity of the blood.

2. That impurity is caused by unassimilated chyle in the blood.

3. Assimilation is a physiological process, and cannot be assisted otherwise than by attention to physiological laws.

4. Good food and healthy digestion are two essential requisites for the furnishing of healthy chyle.

5. A third physiological law to be attended to is, that respiration plays a very important part in the assimilative process ; and the functions of respiration is not "solely to eliminate carbon from the blood," as ordinarily maintained.

6. Perfect respiration is absolutely essential to perfect assimilation, and tuberculous depositions occur because respiration fails to put the finishing touch to the assimilation of the chyle.

7. According to the laws of heat, one thousand degrees of heat become latent upon the conversion of water into vapor.

8. In consequence of that law, a full healthy action of the skin is necessary to keep up a proper equilibrium of temperature ; and as the temperature is exactly in proportion to the respiration, it must be equally necessary to perfect respiration.

9. So long as we regard those diseases as pathological, to be remedied by officious intermeddling, they will be opprobia of the healing art, and conversely, whenever we strike at the true cause of the impurity of the blood, viz : imperfect assimilation, to be remedied by careful attention to *physiological processes*, the practice will be successful."

Certain incidental cases caused him to inquire into the phenomena connected with respiration, and he arrived at the conclusion that imperfect respiration was the cause of consumption, scrofula, etc. He instances the fact that drunkards rarely present tubercles, and hence many have considered alcohol the proper remedy. This position he denies, and considers their exemption due to the fact of their breathing more freely and perfectly.

Concerning the fact of tubercles always making their appearance in the apex of the lungs before showing themselves elsewhere, he considers it due to the imperfect aeration of that portion of the lungs, by the position, etc., of those who are afflicted with tubercles. When respiration is only partially carried on, so that the lungs are not completely filled with air at each inspiration, the apex is almost sure to be deprived of its due quantum, and thus is explained the reason why the apex should be the first portion generally to be affected.

In further proof of his position, he adduces the fact of those who lead active lives not being so liable to tuberculous diseases. Here, while exercise is active, "respiration is necessarily frequent and full, and, as a matter of course, assimilation is perfect." In illustration, we have instances presented of diseases of a similar nature in animals being commonly relieved by perfect freedom from restraint.

While on this portion of the subject he suggests the idea of our using as food the meat of cows, hogs, etc., who have died tuberculous, and yet expecting, while ingesting such matter, that we shall enjoy immunity from similar diseases.

To show the effect of exercise, etc., he instances the case of a gentleman who was informed by his physician that he must die of consumption. "He swore he would not die; quit all medicine; took to sponging himself every morning; forcibly inflated his lungs to their fullest extent as soon as he arose; did so frequently during the day; threw his shoulders back so as to give his lungs fair play; always walked with his arms thrown over a cane across his shoulders so as to avoid attracting too much attention; took as much exercise by walking as his strength allowed, gradually increasing it as his strength increased; got rid of his cough and hectic fever; regained his health, and is now fat and hearty, measuring six inches more in the circumference of the chest than he did when told he was bound to die." He mentions singing as having excellent effects in relieving weak lungs, as rendering assimilation more perfect, enlarging the chest, etc.

Again, "it is a well established fact, that pregnancy generally suspends the progress of consumption, which seems to go on faster after the termination of gestation." He considers this as due to the fact of the pregnant woman being compelled, as it were, to hold herself more upright, and fill her lungs more perfectly at the same time.

He also explains the appearance of contagion in consumption in certain cases. "The attendant breathes the same heated, vitiated atmosphere, is worn out with fatigue, anxiety, and want of rest, and digestion is imperfect." Hence, the deposition of tubercles is not to be wondered at. Tight lacing and its consequences are familiar to all, and forms another instance in proof of these propositions.

He sums up as follows:

POSITIVE.		NEGATIVE.
Drunkards.	Perfect Respiration and Perfect Assimilation.	Sober men.
Base of Lungs.		Apex of lungs.
Active lives.		Sedentary lives.
Capacious Chest.		Narrow, contracted chest.
Dairy cows roaming.		London dairy cows.
Monkeys at liberty.		Monkeys caged.
High situations.		Cellars.
Well ventilated rooms.		Crowded, badly ventilated rooms.
Out-door life.		In-door life.
Indian belles.		Tight lacing belles.
Chyle first passed through the lungs.		Chyle not thrown directly into the arterial system.

He then turns to examine into the success met with from the practice in accordance with these views. He quotes a very interesting letter from Dr. Jas. Norcom to Dr. Physick, in 1813, relating his own case in detail, and showing conclusively very advantageous results from a continuance of exercise under the most unfavorable circumstances.

Another case from the *American Journal of the Medical Sciences*, April, 1855, reported by Dr. Stokes, confirms his propositions in the most marked manner.

A case from Dr. Parrish, of undoubted consumption, is reported in detail, and certainly forms undeniable evidence in favor of this practice. See *North American Journal of Medicine*, vol. v., iii., p. 276.

Thus he leads us on, quoting case after case, each one showing clearly the truth of his mode of practice, and compelling the reader to believe that perfect respiration and assimilation are alone competent to cure consumption, and certainly to prevent it, when hereditary predisposition exists.

But another organ is deemed as important as the lungs; that is the skin. Without it the lungs cannot do their duty. From the law of latent heat, in the seventh proposition, a large quantity of caloric is taken up by the conversien of the perspiration (in ordinary cases insensible) into vapor, and thereby a due equilibrium of animal heat is kept up; and when the heat of the body would be certain to rise above the healthy standard from active exertion, the conversion of the profuse perspiration into vapor takes up such a large quantity of animal heat, that an equilibrium is still kept up, and the person is relieved of those excessively oppressive sensations arising from active exertion when the skin is inactive, so well known by the term "short-winded." The skin may therefore be considered the safety-valve of the lungs, and its healthy action is absolutely necessary to a healthy action of the lungs; when the safety valve is loaded down too heavily, evil always results.

The necessity of the skin being kept clear is too well known to need any proofs of evil results from inattention to its requirements, though he gives some interesting instances of virtual, and, in some, actual "coatings" being applied to the skin, and resulting in death. He gives us the following chain of reasoning: "The skin, say from cold, not being in a healthy working order, respiration cannot be properly performed; respiration being imperfect, assimilation is imperfect; assimilation being imperfect, disorder of the digestive apparatus in some form or other is the result." Again, "indigestible food being taken into the stomach, unhealthy chyle is formed, consequently unhealthy blood is circulated, the skin cannot perform its functions in a proper manner, hence a dry, inactive skin." Or, "respiration being impeded by posture, tight lacing, etc., assimilation is imperfect, disorder of the digestive apparatus follows, and hence dispepsia."

In conclusion we will quote the following: "In order to cure our patients, it would appear far more reasonable to see *what* and *how they eat;* to see what kind of air, and how it was breathed." So convinced is he of the practical success of these principles, that he says: "I have no doubt if the community could be induced to live as they ought, in obedience to physiological laws, consumption and scrofula might be exterminated in the course of a century, as effectually as smallpox by vaccination."

Many physicians of much ability have at various times succeeded in relieving permanently consumptive cases, and have attributed their success to certain medical agents which have been employed. For instance, the late Professor J. K. Mitchell, M. D., we have repeatedly known to speak of his marked success with the *liq. potas. arsenitis,* and exercise on horseback, etc. Now had he ordered the exercise alone, we opine his success would have been as marked. We are connected with a celebrated case of his, where *a family of seventeen children,* with one single exception, died of pthisis of the most marked form. This one case he always quoted as having saved by the persistent use of the liq. potas. arsen. and exercise. That single exception, I firmly believe would also have died of consumption but for the exercise; perhaps the use of the arsenic may have contributed to produce the good result. We hope this matter will be more thoroughly attended to by our professional brethren, and instead of looking in foreign lands, among Indians, etc., for some healing plant, they will seize hold of what is ever before them, and promises such good results.

TRANSMISSION OF SYPHILITIC POISON—*Letter from Professor Sigmund.*—MESSRS. EDITORS.—I send you the translation of a letter received by me a short time since, from Prof. Sigmund, of Vienna, in answer to questions on some disputed points in the venereal disease. It will be seen that Prof. S. asserts his belief in the communicability of the disease in question from secondry appearances. To this part of his letter I beg leave to invite the attention of those members of the Suffolk District Medical Society present at the meeting of March 27th.

I am, very respectfully, yours,
2 *Bumstead Place, April* 13*th.* HENRY K. OLIVER, M. D.

VIENNA, GENERAL HOSPITAL, 6TH JANUARY, 1858.

Dear Doctor.—I send you, herewith, the answers, according to my observations, to your four points. In all these questions there has been a great deal of personal matter brought into the subject, from authorities being apt to adhere to such opinions as they have once made public, at all hazards. Further, the assertions of the patients themselves have been too much relied on. * * * * If you wish more communications on syphilis from observations in the Vienna Wards, I would call your attention to the Wochenschrift (Woltelshoefer, Vienna), and to your Yearly Report (Braumueller, Vienna.) I enclose you, hereby, such as have already appeared, and as I shall prepare those of 1855–57 together, you can have these too, in from four to five months, if you will let me know through whom to send them.

With best wishes, your devoted

SIGMUND.

1st, From *observations on patients* I have remarked the communication, from one individual to another *healthy* person, only in the case of *plaques muqueuses* (breite Condylome). This result takes place through frequent or long continued contact, which, aided by the warmth and moisture of the parts, causes excoriations. The communication requires from five to six week's time; I never saw it take place in less than five weeks. Here belongs also the infection of child by nurse and nurse by child, through papules or excoriations of the skin, or fissures in infiltrated skin.

2d, I know of no case where a common catarrh of the uterus, or of the vaginal mucous membrane, has given rise in the male, to gonorrhœa; that is, a contagious hypersecretion, with its consequences, of the urethral mucous membrane. I know, however, cases in which the man has infected the woman, who was pregnant, or became so, and in whom, after delivery, the lochia, leucorrhœa, etc., infected other men. It is from the *very numerous* cases of this kind that this question has become so intricate, for, strange to say, many physicians consider the gleet (nachtripper), especially the prostatic, and the glandular secretion of the fossa navicularis, which may easily block up the opening, as non-contagious. They are so, however, and only the circumstance that the secretion is often washed away before intercourse, or is even entirely absent, prevents sometimes the infection. I have known men to sleep with women with cancer of the uterus, and no result follow it; sometimes a hypersecretion of the mucous membrane of the urethra comes on, which, however, yields in a few days to cleanliness. *Gonorrhœa arises from gonorrhœa.*

3d, The *vegetations* are always the result of the gonorrhœal secretion alone. Errors in diagnosis, and filthiness, (especially in hospitals, but also in private practice), have deceived authors in their opinion of the origin of these morbid productions. A thorough destruction, by local applications, of *all* vegetations present, removes them permanently, but it is not the same with general remedies; or, they may shrink up, die and fall off, with or without these general remedies. Balano-blennorrhœa alone often gives rise to them, as also blennorrhœa of the glands of Bartholini, in women. Thence arise the errors of observers, who overlook these two forms, or fail to find them because already disappeared, but find chancres, and impute to them the origin of the vegetations. Moreover, authors have been, and are still, very careless in the diagnosis of the growths in question; simple hypertrophy of papillæ, even the coagulations in exudations, were mistaken for them; also growths, similar in appearance, often seen in pregnant women, and lastly, exudations in the deep parts of the eye, even, as well as in the anterior chamber.

4th, Inoculation with the vegetations—that is the transfer of the blood and contents of the cells—gives a negative result; producing neither blennorrhagia nor chancre. When blennorrhagia still remains, this is often inoculated, since an incredibly small amount of mucus or pus is sufficient to transfer the disease. I have inoculated with a few little drops, scarcely to be discerned by the eye, and when I employed warm secretion, the inoculation has always succeeded, provided also the mucous membrane itself was reached, and not a layer of pus or other secretion.—*Boston Med. and Surg. Jour.*

ON THE PROXIMATE CAUSE AND SPECIFIC REMEDY OF TUBERCULOSIS*—*By John Francis Churchill, M. D.*—The total number of cases of phthisis treated by me amounts to thirty-five. All were in either the second or third stages of the complaint; that is they had either softened tubercles or cavities in the lungs. Of these, nine recovered completely, the physical signs of the disease disappearing altogether in eight out of that number; eleven improved considerably, and fourteen died; one still remains under treatment.

I believe that the results, of which the preceding is a summary, taken in connection with the considerations I have set forth at length in the paper now in the hands of your Hon. Secretary, will be found to justify the following conclusions:

The proximate cause, or at all events an essential condition of the tubercular diathesis, is the decrease in the system, of the phosphorus which it contains in an oxygenizable state.

The specifie remedy of the disease consists in the use of a preparation of phosphorus, uniting the two conditions, of being in such a state that it may be directly assimilated, and at the same time at the lowest possible degree of oxydation.

The hypophosphites of soda and lime are the combinations which hitherto seem to best fulfill these two requisites. They may be given in doses varying from ten grains to one drachm in the twenty-four hours. The highest dose which I have been in the habit of giving to adults is twenty grains.

The effect of these salts upon the tubercular diathesis is immediate, all the general symptoms of the disease disappearing with a rapidity which is really marvellous.

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*Abstract of a paper laid before the Academy of Medicine, of Paris, on the 21st of July, 1857.

If the pathological deposit produced by the dyscrasy is of recent formation, if softening has only just set in, and does not proceed too rapidly, the tubercles are absorbed and disappear. When the deposit has existed for a certain time, when the softening has attained a certain degree, it sometimes continues in spite of the treatment; and the issue of the disease then depends upon the anatomical condition of the local lesion, on its extent, and upon the existence or the non-existence of complications. I have made numerous attempts to modify the local condition of the lungs by the inhalation of different substances, but have never obtained any satisfactory result independent of what was to be attributed to the specific treatment. The hypophosphites of soda and lime are certain prophylactics against tubercular disease.

The physiological effects which I have observed to be produced by the use of the hypophosphites of soda, lime, potash, and ammonia, show these preparations to have a two-fold action. On the one hand, they increase the principle, whatever that may be, which constitutes nervous force; and on the other, they are the most powerful of hæmatogens, being infinitely superior to all medicines of that class hitherto known. They seem to possess in the highest degree all the therapeutical properties formerly attributed by different observers to phosphorus itself, without any of the danger which attends the use of that substance, and which has caused it to be almost forgotten as a medical agent. The different preparations of hypophosphorus acid will undoubtedly occupy one of the most important places in the materia medica.

The Academy resolved that the paper be referred to a committee, consisting of MM. Louis, Trousseau, and Bouilland.—*Dublin Hospital Gazette.*

Evidence afforded by the Microscope in a Case of Rape.—*By A. F. Sawyer, M. D., San Franscisco, Cal.*—Last November I was called to see a child of Mr. H., a little girl about five years of age, on account of suspected violence against her person. After the occurrence, the child came into the house looking very pallid, and complained to her mother of being sick, whilst her clothing was observed to be disordered and stained with blood.

On examination, the inside of the thighs and the legs of the drawers, were found to be much marked with blood, and evidently some attempt had been made to remove the blood on the thighs by wiping. The external parts of the child were bruised and tumefied. The hymen had lost its natural pale color, and appeared highly congested. Its apperture was about the size of a crow quill. There was no appearance of laceration in it, or in the surrounding textures. There was also a constant dribbling of urine during our examination.

All her clothing, consisting of Canton flannel drawers, a faded yellow petticoat, and a light outside frock, I caused to be sent to my office. I transferred, also, the little particles of dirt, noticed about her privates, especially in the folds of the integument, to a piece of fresh white paper, and afterwards sponged them with a clean rag, and removed the piece of the sheet which had been moistened by the escape of the urine; all of which I preserved for future inquiry. I then visited the privy where it was supposed the outrage had been committed. There was about half an ounce of fluid blood on the stool of the privy, and on the floor was a piece of newspaper smeared with blood, which had evidently been used for the purpose of wiping.

My attention was afterward directed to an examination of the party suspected of the crime, and who had been arrested in the afternoon of the same day. He wore, at the time of his arrest, an outer red flannel shirt, considerably soiled, covering a bluish grey woollen shirt, which exhibited three or four small stains in front resembling blood; and a pair of drab-colored woollen pants with two or three small suspicious-looking spots along the opening in front. His clothing was also sent to my office for subsequent examination.

From the considerable amount of hæmorrhage that had been noticed at the privy, and from the absence of distinct laceration of the hymen of the child, I thought it quite probable that the external parts of the accused would exhibit tearing—perhaps of the frænum—to account for the large amount of blood lost; and which, had it existed, would have supplied important evidence of his guilt. This was not the case, however. His yard was perfectly clean, without trace of blood, and bearing no marks of laceration or other injury.

By a microscopical examination of the spots on his clothing already referred to, those on the red shirt, more marked about the wrists, were found to have been caused by blood, as also the smaller stains on the grey shirt; and blood globules were distinctly visible in the field of the microscope, where a selection had been made from one of the stains of the pants that bore indications of attempts having been made to remove it by scraping or otherwise.

On examination of the piece of sheet which the child had wet, an abundance of blood globules was found, mixed with the cell-growth characteristic of semen. The rag used to bathe the privates of the child exhibited the same appearances. The most important proofs were, however, detected in the microscopical examination of the particles of dirt that had been transferred to the piece of paper. In this was found, besides an abundance of blood globules and sperm cells, *fibres of wool mixed together, of distinct color, some of them being of a bright red, and others of a dirty indigo color, corresponding exactly with the wool fibres of the two shirts which the accused party wore at the time of his arrest.* The contrast of the colors was more marked with the reflected light of a candle, although perfectly distinct when observed by sunlight. After repeating these examinations, to remove every source of fallacy, the flannel petticoat of the child was examined, and its fibres preserved the characteristic color of the texture—a light yellow. This garment had lost its brightness of color by use.

The presence of sperm cells about the person of the child clearly illustrated the cause of her injuries, and the detection of the wool-fibres from the same source was sufficient evidence to connect the party arrested with the commission of the crime. If the red fibres, or the indigo-colored fibres alone had been discovered, there might have been a wide margin for doubt; but the complete identification of the texture of both shirts afforded the strongest presumptive proof of the guilt of the prisoner, especially when taken in connection with the stains of blood detected on his clothing.

The large amount of blood which the child lost, without any apparent laceration of the genital apparatus, is remarkable. It is clear to our mind that it came principally from the hymen by the force of pressure, and most probably as a hæmorrhagic exudation similar to what is known to occur with other textures of the human body. It is to be borne in mind that the child was found in a weak and fainting condition. Notwithstanding the external bruises, sufficient force had been applied to create incontinence of urine. We had, also, the presence of blood globules in the urine, which washed her parts as it escaped from her, and an examination of the urethra by a probe failed to detect any injury to this canal. The insides of the thighs were stained with blood, and apart from the known vascularity of the hymen it was in a highly congested state at the period of our examination. There is no doubt that the hæmorrhage came from the child; and if from the child, in all probability from the hymen.

Otherwise, the principal testimony in the case rested with the child, and although given by a person of an age that would scarcely warrant its credibility, seems to be entitled to some authority from its consistency. Besides conducting the police to the privy where the outrage was perpetrated, she selected the prisoner, after his arrest, as the guilty party, from a number of others who had purposely dressed themselves similarly to deceive her; and the same identification took place in the court room during the trial, when the prisoner was presented in citizen's dress.

The remaining testimony was but meagre. For some days previously the prisoner had been observed playing with the child, who had received from him little presents of fruits, etc. No evidence existed to show that the prisoner had been seen in company with the child on the day this attempt at rape had been committed.

The defence asserted an alibi, which could not be sustained, and the jury found a verdict of guilty without leaving their seats.—*Boston Medical and Surgical Journal.*

CASES SHOWING THE ADVANTAGE OF HEROIC DOSES OF CALOMEL.—*By Wm. Hauser, M. D., of Jefferson County, Ga.*—An experience of fourteen years has convinced me that no one thing, nor all other things combined, are equal to calomel *in Herculean Doses,* for the treatment of acute gastritis and bilious colic. Without any waste of words I give a few cases: First premising that my range of practice lies in that beautiful long-leaf pine and sand belt of Georgia, near 33d parallel of North latitude, which is checkered alternately with high and dry pine barrens, celebrated for their healthfulness, and rich alluvial "oak and hickory" lands, where Hepatic derangements are found in plenty.

Case I. Wm. Caruthers, of Burke county, aged about thirty, tolerably plethoric and of steady habits, when I knew him, was attacked with symptoms of acute gastritis, most violent, October, 1843. Venesection was attempted but failed. The inflammation appeared to progress more rapidly than in any case I have ever noticed before or since. Having no time to trifle with doubtful medicines, I instantly poured down him forty grains of calomel and the same of jalap. He retained it, and in a few hours I repeated the dose. In twelve hours he was happily convalescent, and soon got well.

Case II. James Wooten, aged about thirty-five, of robust frame and vigorous constitution. Called to him on Monday, May 6th, 1844. He was excessively bilious; looked as though his liver might have failed in its functions a month previous. Suffering with symptoms of acute gastritis, I premised venesection, and gave eighty-five grains of calomel. It operated in a few hours, and he was quite relieved. The second day he rode out, and seemed entirely cured.

Case III. Mrs. Lucinda Bryant, of Washington County. I was called to her in consultation with my esteemed friend, Dr. William B. Folks, on the 15th of October, 1855. Her stomach would retain nothing. She was suffering most terribly. My friend had tried every thing he could think of, and even calomel itself, but in doses too small to remain in the stomach. I administered, without the least hesitation, ʒi of this article. She retained it, was relieved in ten hours, and her recovery was rapid She had been suffering two or three days with gastritis, and death seemed inevitable under any circumstances. Calomel is one of the finest antiseptics in the world, as I have found by a great multitude of experiments, although Dr. Dunglison does not even mention it as one of the chief articles of that class. I believe its virtues are not fully known. Every body has learned something of its cathartic and febrifuge properties, and its very commonness in the hands of physicians has induced a carelessness about studying its properties and watching its effects. New remedies are the rage, while some of the older and more valuable are treated with unjust neglect, to the detriment of Doctors and their patients.

Case IV. Miss Julia Parker, of Jefferson County, 185–, case of remittent fever, with frequent vomitings and inability to retain any medicine except calomel. A large dose of this produced immediate quiet, and prepared the way for a speedy recovery.

Case V. Myself. Called on the 16th of October, 1854, to perform a journey of twenty-five miles to consult with my friend, Dr. Wm. G. Allen, over a dying lady. My accustomed gastralgia came on by the time I had made about three-fifths of the distance, and the jolting of the buggy and vomiting for the remainder of the way, developed symptoms of gastritis, and such excessive irritability of stomach, that the gentlest shake of the bed would bring on convulsions horrible to behold. Bleeding, injections, sinapisms, all failed to bring sufficient quiet to the stomach to enable it to retain anything that was swallowed. At last I asked my friends to give me a teaspoonful of calomel. This remained only a few minutes. I then requested a tablespoonful. That remained and checked the vomiting. In thirty-six hours it operated and I was relieved.

I might encumber the *Journal* with a large number of cases; but these are deemed sufficient to show the importance of giving calomel in heroic doses to patients suffering from gastritis. And the same holds good in bilious colic, wherein bile is vomited in a tide, and the bowels so constricted that no operation can be induced by injections, hot baths, or any medicines that may be given, save calomel. But it must be given in *large doses* to bring speedy and full relief; and there is not the least danger in so administering it. I have, in a few cases, been called long distances to consult with young physicians, who, I found, were pursuing the right plan of treatment, saving their timidity in administering calomel. In such cases I have only had to nerve them up to bolder doings, and success has always followed.—*Atlanta Medical and Surgical Journal.*

TRANSFUSION OF BLOOD.—I have communicated several papers on this subject to the *Académie des Sciences* (see *Comptes Rendus,* 30th Nov. 1857, p. 925), to the *Société de Biologie,* and to the *Cercle des Sciences.*

It is known that Blundell had found that a dog, bled almost to death, can recover, even if blood of a mammal of another species, be transfused into its veins; but that after a few days death always comes; while the blood of another dog being employed for the transfusion, may reproduce a lasting life.

Messrs. Prevost and Dumas declare also that mammals cannot be recalled permanently to life after a great loss of blood, if transfusion be made with blood of mammals of a species different from theirs. They state also, that the blood of animals transfused in the veins of birds, and *vice versâ,* produces almost immediate death, after having caused violent convulsions. M. Rayer affirms, also, that the blood of man kills rabbits, after having produced convulsions. Dieffenbach has sometimes seen animals recover after transfusion of blood of animals of other species, but never when they were in a state of apparent death after a considerable loss of blood. Bischoff also has never succeeded in restoring to life mammals that had lost a great deal of blood, in transfusing blood of birds in their veins.

An important fact, found by Bischoff, should, however, have opened the way to more successful results in transfusion. He ascertained that the arterial blood of mammals can be injected in the veins of birds, without killing them, while the venous blood causes rapid death. I have found that venous and arterial blood do not differ one from the other, except on account of the different quantities of carbonic acid and oxygen they contain. Both may kill, if they are rich in carbonic acid; both may not have an injurious influence if they contain a great quantity of oxygen. Numerous experiments have led me to the following conclusions:

1st. That arterial or venous blood from an animal of any one of the four classes of vertebrata, containing oxygen in a sufficient quantity to be scarlet, may be injected, without danger, into the veins of a vertebrated animal of any one of the four classes, provided that the amount of injected blood be not too considerable.

2nd. That arterial and venous blood of any vertebrated animal, being sufficiently rich in carbonic acid to be almost black, cannot be injected into the veins of a warm-blooded animal, without producing phenomena of asphyxia, and most frequently death, after violent convulsions, provided that the quantity of injected blood be not below one-five-hundredth of the weight of the animal, and also that the injection be not made too slowly.

The reasons for which Blundell, Bischoff, and others have failed to restore a lasting life after the transfusion of blood of an animal of a spieces different from that of the transfused one, are: 1st, That the blood employed was not fresh; 2nd, that it was in too large a quantity; 3rd, that it was injected too quickly; 4th, that it was too rich in carbonic acid, and too poor in oxygen. The greatest causes of failure were this last one, and after it the quantity of blood.

From my experiments I have arrived at the conclusion, that there is no danger in employing the blood of dogs, cats, or other mammals in transfusion in man. Besides, I have ascertained, after Dieffenbach and others, that defibrinated blood is as good as blood containing fibrin. As regards the quantity of blood, I think that four or five ounces would be as much as can be needed for an adult man or women. It is not necessary to warm the blood, but it may be useful to do it in some cases. The blood to be transfused, either that of man or of mammal; should be received in a large open vase, and whipped at once, then passed through a thick cloth. If it is not injected at once, it must be either whipped again, or, at least agitated to be charged with oxygen just before transfusion, which can be performed with any kind of syringe. The injection must be extremely slow, and if after the injection of two or three ounces, there is a great increase in the respiratory movements, it is well to stop for ten or fifteen minutes before finishing the transfusion.—*New York Journal of Medicine.*

ON URETHRAL INTERMITTENT FEVER—*By M. Chassaignac.*—This is the name M. de Chassaignac attaches to the febrile attack which all surgeons have observed as a consequence of catheterism, and which, if it were desired to indicate the cause giving rise to it, might be termed catheteric fever. It it is a curious point to determine, whether the contact of the instrument with the entire length of the urethra is necessary for the production of the paroxysmal attacks, or whether it is sufficient for one portion of the canal to have undergone such contact, and in that case, which portion. Judging from the facts known to him before the case which gave rise to these observations came under his notice, M. Chassaignac was disposed to believe that neither the membranous nor the prostatic portion of the urethra was in the portion in question. Thus he never met with an instance of such febrile paroxysm being produced after catheterism in the female, nor is he aware of any one who has ever met with one. A natural inference would be that it is not observed in woman, because the paroxysm arises from the contact of the instrument with a portion of the urethra which does not exist in her. An opportunity offered itself for confirming this conjecture, by the counter-proof of the induction of the paroxysm by the exclusive catheterism of the portion of the uretha which is proper to man. This occurred in the case of a patient suffering from multiple urinary fistulæ. The catheter being passed from one of these to the meatus, and back again from the meatus to the fistula, no difficulty or pain occuring, an intense intermittent paroxysm was produced. The patient had had the catheter passed before, and had borne one fixed in the urethra without such effects having been produced. It was quite certain that on this occasion the instrument had not come in contact with either the membranous or prostatic portion of the urethra, or with the neck of the bladder; and until further information upon the subject, M. Chassaignac feels justified in localizing in the bulbous or anterior portion of the urethra the physiologico-pathological process which gives rise to the production of the paroxysm in these cases.—*Moniteur des Hôpitaux*, 1857. *No.* 135.

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MEMOIR ON SOME POINTS OF THE NORMAL AND PATHOLOGICAL ANATOMY OF THE MUCOUS MEMBRANE OF THE UTERUS.

By DOCTOR CHARLES ROBIN, Professor Agregé of the Faculty of Paris, Member of the Academy of Medicine, etc.

Written expressly for the New Orleans Medical News and Hospital Gazette, and translated by ANTHONY PENISTON, M. D., Professor of Physiology, New Orleans School of Medicine.

PRELIMINARY REMARKS.

In the examination of every tissue, whether normal or pathological, the following principles must be borne in mind: Every pathological production, and every normal tissue, owes its characteristic properties to one or more anatomical elements, which are specially endowed with these properties; though these properties are only made manifest when the elements themselves are combined into masses, for separately they are not visible to the naked eye, and can only be studied by means of the microscope. Thus, muscular tissue owes its contractility to the fact that its principal element and the mass of its substance are composed of contractile fibres; in the same way the sensibility of nerves is due to certain anatomical elements of a tubular form, having peculiar properties just as muscular fibres; in the same way elastic tissue has its distinct and special properties and texture. Therefore it is that we only determine the properties of a tissue by ascertaining the nature of its anatomical elements, and as these are invisible to the naked eye we must necessarily use a microscope of sufficient magnifying power in order to recognize the appearances which distinguish them one from another. By proceeding in this manner we soon learn that the microscope is a sure guide to the naked eye; for, passing from the anatomical element itself to the tissues which are formed by its textural arrangement, we learn that certain external appearances always coincide with certain internal peculiarities and elementary characteristics. Therefore it is that whenever a tumor presents some peculiarity of color, form, consistency, etc., it will have some corresponding anomaly in its anatomical elements, though they may be of a secondary nature, such as molecular granulations, amorphous matter, etc., and then the anomaly may consist in a departure from the usual proportion of fundamental elements, or peculiarities of texture. From a knowledge of these facts it has resulted that many peculiarities which were previously unheeded are now attentively observed; so that the peculiarities of color, consistency, etc., must be necessarily taken into consideration, since they are found to be characteristic of some special fundamental element: on the other hand some appearances are of secondary importance, or denote the presence of elements subsidiary to others. Thus it is, that passing from the anatomical elements to the tissues, which they form, we learn to what internal structure and arrangements are due the external appearances which they present; for the one is dependent upon the other. The different appearances which are peculiar to the numerous varieties of morbid growths are dependent upon some departure from the normal arrangement of the elementary particles which compose them, although these particles may require high magnifying powers to reveal their structures and properties.

These anomalies of structure are almost always caused by some change in the form or shape of the anatomical elements of which it is composed, for they may be atrophied or hypertrophied, or abnormally increased in number in any given point, or they may be altered by the development of fatty granulations in their substance, or by the development of other elements not usually existing in that portion of the body; but whatever changes take place in these morbid structures, it is evident that the examination of these elements themselves can alone enlighten us as to their nature. No importance should be attached to the differences of opinion which are found to exist between those who arrive at their conclusions by a superficial examination with the naked eye and those who use the microscope, since the former are guided by external appearances and the latter by an examination of the very elements of which these bodies are composed.

Pursuing the subject still farther, we find, upon examination, that from the period of the earliest appearance of the different anatomical elements in embryonic life, to their full development in adult life, they undergo successive modifications; though these changes are not so great as to prevent us from recognizing the different varieties of tissues, tubes, cells, etc., from a previous knowledge of the same elements at their period of maturity. Still these periods of transition require to be known to arrive at a correct interpretation of certain lesions and morbid phenomena. This is especially true with respect to the different tissues, for they are progressively modified, not so much by any changes in the elements themselves, of which they were at first composed, as by the production of new elements, or the multiplication of others already existing. General anatomy, then, is a knowledge of the anatomical elements and structure of the different tissues, not only in their mature state in adult life, but during their periods of evolution from the embryonic state to their full development, and afterwards during their periods of senile decline or morbid degeneration. We mean by anatomical elements certain organized particles which are extremely small, having nevertheless a constant and determinate structure, and which cannot be reduced by mechanical means to any simpler, integral form.

These preliminary notions are necessary in order fully to understand the following remarks, for in any course of scientific study a method and rules of investigation are necessary in order to obtain any useful results.

In the *first part* of this memoir I propose to describe a certain number of facts relative to the arrangement and apparent structure of the fibrinous clots of blood which are sometimes found in the cavity of the uterus and in the ovaries.

In the *second part*, I shall describe the connection between the placenta and the mucous membrane of the uterus, describing the structure of the latter and the relation existing between it and the function of respiration and nutrition in the fœtus.

In the last part, I shall describe the changes which take place in the epithelium of the uterus during pregnancy.

As we advance on this subject it will be seen to present many important applications to pathology, many of which are but little known.

PART FIRST.

Anatomical arrangement of the clots found in the cavity of the uterus and in the ovaries, as seen under the microscope.

SECTION 1. Of the intra uterine clots which are sometimes expelled during menstruation.

In certain states of the system, and especially of the uterus, the blood seems to have a peculiar tendency for coagulation, so as to be eliminated in clots at the period of menstruation. Dionis says that he attended upon a princess who voided regularly every month, small flesh-like bodies formed by coagulated blood, and that these discharges took place even though she may not have had for some time, any connection with her husband. Other writers assert having observed the same thing in young women living in convents and other religious institutions. These masses generally remain but a short time in the uterus, and are voided soon after being formed. They are remarkable by their shape which often appears to be a perfect cast of the internal cavity of the uterus. The narrow or inferior extremity, corresponds to the neck of the uterus, into which the clot extends, and it is generally more irregular than the other part. These clots, which are generally pyriform in shape, and fill the cavity of the uterus, often present on their surface a peculiar reticulated appearance, or they may be smooth with small elevations like those sometimes observed on the surface of the body. This appearance is due to the fact that the fibrine in coagulating atom by atom, is moulded upon the uterine cavity, and reproduces as hollows the folds of the uterus at the period of menstruation, or on the other hand it presents in relief the small depressions which are caused by the orifices of the uterine follicles slightly dilated at this period.

Some women only void one of these clots at each menstrual period, but others throw off several, say one in twenty-four hours or every two days during the menstrual flow. The latter do not cause any pain in the intervals between the expulsive efforts, that is to say, so long as the cavity of the uterus is not filled ; but as soon as it is full, and begins to be distended, these expulsive pains and uterine colics are brought on. In some women the formation and expulsion of these clots are phenomena which are repeated at almost every menstrual period, but these cases are very rare. It is important to distinguish between this kind of cases and those in which the mucous membrane of the uterus is also, either partially or entirely, cast off, surrounding either partly or completely a clot similar to those just described. In these pathological conditions a careful dissection of the specimen and a microscopic examination of its various parts will always enable us to distinguish the mucous membrane of the uterus, thus become deciduous, from the clot which it contains or other coagula which may be situated exteriorly.

It often happens, that one accustomed to this anatomical manipulation, may find on the internal side of the mucous membrane of the uterus some portions of the decidua reflexa, which primitively surrounded an ovule then exceedingly small and now destroyed. Sometimes, if pregnancy dated about two months back, the embryo and the envelopes of the fœtus may be found flattened and irregularly folded, but surrounded and covered by the clot.

The amnios can be distinguished by its smooth and brilliant internal surface, by the regular cells of pavement epithelium of which it is entirely made up. The clots when accompanied with portions of the deciduous mucous membrane of the uterus, or of this entire membrane, but without any traces of embryonic structure that can be recognized as such, constitute what is called *pseudo membranous dysmenorrhœa;* on the other hand, it is called abortion, when the fœtus is evidently recognized to be present. But it is undoubtedly true that both cases belong to the same category and that they are both varieties of abortion, only at different periods. Mr. Raciborski was certainly correct when he said that the appearance, form, volume, and especially the structure of those products, though considered, since the works of Professor Simpson, to be instances of pathological exfoliation of the mucous membrane of the uterus, really authorize us to consider them, especially when we take into account the mode of their expulsion, as cases of pregnancy in which the product of conception has been early expelled, and that too at a time when the embryo is easily destroyed, and exceedingly difficult of recognition. The chorion is easily separated from the amnios on account of the thin, soft coat of lamellar tissue which separates them, and sometimes the umbilical vesicle with its yellowish contents can be found between them. The chorion can be easily known by the villosities with which its surface is covered, and which are sometimes found mingled with the fibrine of the clot. It can also be known by its own proper structure. During the first month of intra uterine life it is formed by polygonal cells in close juxtaposition and adherent to each other, measuring from fourteen to sixteen thousandths of a millimetre. These cells are very granular, with a spherical or slightly ovoid nucleus, somewhat large in proportion to the cell and containing a small nucleolus. The villosities are hollow and composed in the same manner as the chorion. They contain capillaries having a single coat with close longitudinal nuclei. After the first month of uterine life the chorial cells which always remain distinct in most mammiferous animals, in women, on the contrary, become adherent to each other, and thus united they form the fœtal chorion with its villous coat. The substance of the chorion and of its villosities then appear under the microscope as an amorphous, grey, resisting substance; not soluble in acetic acid, though rendered very transparent. It is covered over with a considerable number of nuclei, of a long, ovoid form, measuring from eight to ten thousandths of a millimetre in length, and from five to six thousandths in width. These nuclei are contained in the interior of this substance and not on its surface; they cannot be separated except by being torn accidentally, and they form a portion of its constituent material; they are more or less separated from each other according to different portions of the chorion and its villous coat, and the intervals are filled up by a considerable number of fine molecular granulations, mostly greyish, some of them being fatty. These granulations are in some places so thick as to mask the nuclei either partly or entirely.

On the clots which have been previously described, and still more frequently on those which have been formed around one of these abortive ovules expelled during these early miscarriages, and which still retain the pyriform shape, we often find the surface of this mass to be smooth and shining with a ferruginous, greyish appearance. In these cases we can separate from the surface of the clot a thick membranous coat, having in some cases from one to three millimetres in depth. This coat is either greyish or redish grey throughout its substance. It is elastic, easily torn, and when torn assuming a filamentous appearance. Beneath this is found the remainder of the clot, having a deep red color and its other usual physical characters. As to the membranous coat itself, it is principally composed of fibrine, with the arrangement above described, generally containing blood corpuscles and epithelial scales, retained in the midst of the fibrinous tissue. These red corpuscles are seen sometimes separate and sometimes accumulated together, being fewer in number in proportion as the clot is less colored; sometimes they are seen with difficulty until separated from the surrounding tissue. We sometimes also find the white corpuscles of the blood generally a little out of shape. Their finely granular state can be mistaken for that presented by the fibrine, and renders them difficult to be perceived in the mass of the latter. But acetic acid causes fibrine to become perfectly homogeneous and transparent, swells it, while it renders the nuclei of the white blood-corpuscles much more distinct. By separating small portions of the clot and examining them under the microscope, we can thus detach a large number of epithelial cells, which have become perfectly distinct, and can be examined under the microscope. Some of them can also be found in the fibrine, mixed with the white corpuscles. In consequence of all these elements being thus accumulated and woven together, it would appear as if some peculiar *organized* tissue was under observation, instead of a clot which has simply drawn together elements which other clots do not usually contain. But the action of acetic acid, as above mentioned, and the complete absence of capillary or other vessels, will always enable the observer to recognize their true nature. Those who have had an opportunity to see some of the sacs formed by the cast-off deciduous membrane will always distinguish them from the simple blood, even with the naked eye. Moreover, the external appearance of these bodies, as above described, their surface covered with minute depressions, derived from the internal surface of the uterus, are characteristics peculiar to these exfoliations of the mucous membrane of the uterus, and not elsewhere met with.

SECTION 2. Of the clots which are accidentally found in the vesicles of DeGraaf, or the ovisacs.

It is a well known fact that clots, often of large size, are found in the ovaries of women who died from small pox, measles, typhoid fever, etc., though very often no symptom during life would lead us to suspect this lesion. But, besides these cases, and those which depend upon the flow of blood caused by the physiological rupture of the Graafian vesicle, at the period of ovulation, certain hemorrhagic clots worthy of attention, are found in other vesicles which have not been ruptured.

In those clots which are sometimes seen in the ovary of women who died from typhoid fever, etc., two or three Graafian vesicles, in one or both ovaries, may be found distended with a clot. They are generally about the size of a hazel nut, or even a little larger, soft, very easily broken, having about the color and consistency of raspberry jelly.

They are found to contain the following elements: fibrine, resembling that found in the ordinary clot from venesection; blood corpuscles, rather darker than in the usual state, at the same time smaller and rounder; sometimes, though seldom, crystals of hematoidine may be found in process of formation. The clots which form the subject of the present essay were found in making the post-mortem examinations in two cases of acute pneumonia; the third was a case of cirrhosis of the liver, and in the fourth the disease was not known, being taken from a subject at the Ecole Pratique of Paris. The oldest subject (case of cirrhosis) was forty years old, had not menstruated for two years, and complained occasionally of pain on each side of the hypogastric region, though little notice was taken of this, as the pain was supposed to be caused by the disease of the liver. In the two first cases, one of the patients was twenty-eight years old and the other thirty-three, and though they still menstruated, yet the notes on the cases stated that their menses had been scanty and irregular for several years. In every one of these subjects the surface of the ovary bore marks of old cicatrices; the last alone contained no traces of previous corpora lutea. In the other cases I found several which had dwindled to the size of a small greyish tubercle, the largest of which was about the volume of a pea. In all these cases attention was drawn to the ovaries by their large size and distended aspect. In the first case only one of the ovaries was in a hemorrhagic condition.. It was as large as a hazel nut, and double the volume of the other. In two of the cases, particularly the last, two or three vesicles were seen projecting on the sides of the ovary, like a small black lump, the color of which appeared through the external coat of the organ. The last specimen contained eight vesicles, affected with hemorrhage, and the other five. The largest was about the size of a nut, containing a dark red clot something like black currant jelly, darker towards the centre, and somewhat discolored on the surface. Two vesicles about the size of a pea, contained a clot resembling that from venesection, but of somewhat greater consistency. The others presented different degrees of color, varying in shade from the former to that of white or rather yellowish fibrine, with a transparent appearance. This condition was found to be the case in several vesicles which were about twice the usual size, and in which the clot seemed to be discolored without the vesicle itself having at all diminished in size. Some of the vesicles, which were scarcely larger than normal, contained clots which were still red, or not so much discolored; the fibrine itself being scarcely altered. In these latter cases especially, we could follow the various changes through which fibrine gradually passes during the process of discoloration and absorption of the red corpuscles. In a single case, and the first which I examined, I found crystals of hematoidine in one of the clots.

I shall not here describe the gradual changes which are successively presented when fibrine happens to be effused into natural or accidental cavities, for I have already described them elsewhere. (Chimie Anatomique, vol. iii, p. 244, 264, *et seq.*) I shall only here observe that, in these pathological cases, by successively examining several of these vesicles with hemorrhagic spots, we can find all the different modifications of fibrine, from the fibrillar state to that in which it becomes amorphous or is reduced to fine molecular granulations. I shall now describe the condition in which we find the internal membrane of the ovarian cyst when filled with an apoplectic clot. It is generally red, with a smooth internal surface, the latter having generally a large number of yellowish spots over the whole or a portion of its extent. The most important point connected with it is, that its normal thickness is not much altered. In those points where it has a yellowish appearance, it varies from a fourth to half a millimetre in thickness; in other places it is not quite so thick. It is important to observe that the vesicles affected by hemorrhage are found as well in the centre of the ovary, or near the free border, as at the point of its attachment to the broad ligament. This internal membrane has exactly the same constituent anatomical elements as in the normal state, only they are somewhat harder. The peculiar color of the yellowish spots is owing to irregular fatty granulations which are not contained in distinct cells, as is found to be the case with the corpora lutea, but detached and scattered, or accumulated in the amorphous matter existing between the fibres. As to the red blood-corpuscles, we can observe here also the different phases of their destruction by gradual absorption. In different clots, and particularly in those which have a reddish, flesh-colored appearance, the blood-corpuscles are found at different degrees of discoloration. Some are found to be completely discolored, though retaining their circular flattened form and even their bi-concave appearance. These blood discs are intact so far as their form is concerned and the structure of the globuline of which they are principally composed, though deprived of their coloring matter or hematosine. These bodies are, at first, recognized with difficulty on account of their pale color; moreover, many of them contain fine granulations, with a brilliant centre, some five or six in number, disposed near the circumference of the globule. But when slightly swelled by water, their rounded edges can be seen like small greyish or dark circles with a pale centre. This condition of the globules, the intermediate states of which can be easily seen, is also met with in other analogous cases.

PART SECOND.

Of the manner in which the placenta is connected with the mucous membrane of the uterus.

SECTION 1. Observations on the decidua inter-utero-placental.

All the authors seem to agree, in repeating one after the other, that the decidua inter-utero-placental, otherwise called decidua serotina, intermediate or utero-epichorial membrane, is carried off with the placenta like the rest of the decidua is by the chorion. This fact is not however entirely correct, and in order to be understood rightly requires some anatomical and physiological explanations.

It is generally stated that the placental villosities, or the placental lobes of the cotyledons, project into the tissue of the decidua serotina, being plunged therein, as if apparently or really floating in a pool of blood from the uterine sinuses; that the blood is therefore in immediate contact with the villosities of the chorion, the placental cotyledons projecting into the deciduar serotina, or its sinuses, like the roots of a plant descend into the earth. This statement however is not perfectly correct.

The decidua inter-utero-placental is that portion of the mucous membrane of the uterus corresponding to which the villosities of the chorion have become hyper-trophied in order to form the placenta; it therefore separates the placenta from the muscular coats of the uterus. On account of its position it is differently organized from the remainder of the mucous membrane of the uterus, which becomes deciduous; at all events, its properties and functions are different. The most important difference in organization is its vascularity. For whilst the remainder of the mucous membrane of the uterus becomes deciduous because its vascularity has diminished, and because a thin, new mucous membrane is substituted to that which becomes deciduous, the inter-utero-placental mucous membrane, on the other hand, is seen to become highly vascular; so, also, while the uterine decidua, and the decidua reflexa, are greyish, soft, thin and easily torn, its tissue being perfectly homogeneous, the utero-epichorial membrane is remarkable by its large sinuses full of blood (or full of clots on the subject). These enormous veins or sinuses are continuous directly with the venous sinuses of the muscular coat of the uterus, which are there larger than in other portions of the muscular coat. This arrangement gives to the decidua serotina, when cut or dissected, a peculiar, cavernous, erectile or areolar appearance. By following the large vessels, which anastomose with each other in every direction through it, we gradually penetrate into the muscular coat itself, containing similar sinuses, with exceedingly thin coats, and inseparably connected with the tissue of the mucous membrane of the uterus, or with the body of the uterus itself; the internal surface of these sinuses being exceedingly smooth. These sinuses cease abruptly near the circumference of the placenta; and this sudden termination is especially conspicuous when the circular sinus of the placenta, with which they all communicate, happens to exist on the periphery of that organ; this sinus being nothing else than one or more veins of the periphery of the serotina, their size being generally different, according to the part of the placental circumference in which they are examined. This examination must be made by a section of an uterus whose placenta is not detached, so that when the section is made through the uterine decidua, and through the serotina, the different appearance of the two membranes becomes exceedingly well marked. The serotina appears to have the thicker coat, with large vessels passing through it, and these are not seen in the true decidua, which is much thinner. But this difference of thickness is only made manifest when the specimen is injected, or when the sinuses happen to be filled with coagulated blood, which is a very rare case; for this depends upon the circumstance, seldom met with, of finding the circular sinus above described (for it does not always exist) and the chance of having it filled with blood.

If the placenta be detached from the uterus of a woman dying between the seventh and the ninth month of pregnancy—(and while describing this appearance I have one of the kind actually before me—7½ months,) then the following facts will become evident. The chorion drags away both the decidua uterina and reflexa, which are adherent to it and to each other. In thus detaching them from the contractile coat of the uterus, the muscular tissue of that organ is found invested with a new mucous membrane, which is already apparent; it forms a thin, soft coat, of a rosy appearance, which moulds itself on the muscular fibres, that are seen either by transparency or directly when it is torn. The free surface of this new mucous membrane is somewhat irregularly villous under water; but when examined on women dying seven or eight days after delivery, it has already a smooth surface, though it be not much thicker; it has then a brilliant, glutinous appearance, and is too soft to be dissected from the tissue beneath. It will also be seen that the placenta carries with it the circular sinus which surrounds its circumference, and which is slightly exterior to it, rather than inter-utero-placental, properly speaking. It carries away with it, at the same time, about half of the serotina, or decidua-inter-utero-placental, by tearing nearly in the middle the sinuses which pass through it It also carries with this thin portion of the serotina, some parts of the sinuses which remain intact, especially in the neighborhood of the circular sinus, and in the furrows or depressions which separate the uterine cotyledons. Therefore, when the placenta is separated from the uterus, it does not carry with it the whole inter-utero-placental decidua, as is the case with the rest of the decidua uterina, but leaves the greater portion of the former adherent to the uterus. We next pass to the Placenta.

(*To be continued.*)

A VALUABLE REMEDY FOR DYSMENORRHŒA AND CONSEQUENT STERILITY.

By E. D. Fenner, M. D., Professor of Theory and Practice, New Orleans School of Medicine.

It is an unexpected coincidence, that there should appear in the same number of this Journal, a scientific treatise on the minute anatomy and special pathology of the mucous membrane of the uterus, by one of the greatest pathologists of the day, and a practical paper on the treatment of dysmenorrhœa, introducing a remedy believed to be superior to any hitherto discovered. My observations will be almost exclusively practical—a simple report of cases, leaving the reader to his own theory of the nature of the disease and the *modus operandi* of the remedy. Doubtless, the valuable researches of M. Robin will afford us more satisfactory information on both of these points; but in case they do not, we should still be in no worse condition than we are in respect to one of the most common complaints and its sovereign remedy: I allude to *intermittent fever* and *quinine*, concerning which, one of the best teachers of the day (Watson) says: "We know nothing of the seat or of the essential nature of the disease; we are equally in the dark as to the *modus operandi* of the quina in curing it; yet our knowledge of ague, upon the whole, estimated in reference to its precision and practical bearing, is more satisfactory than of many other complaints, with the seat and nature of which we are much better acquainted. The group of symptoms is so distinct, that we have no trouble or doubt as to the diagnosis, and experience has taught us a remedy which is all but infallible." (Lectures, p. 461.) Now, the diagnosis of dysmenorrhœa is fully as easy as that of intermittent fever; but it is yet to be determined by enlarged experience whether the proposed remedy is equally infallible with quinine.

I do not maintain for a moment that this or any other disease is a *distinct entity,* of uniform nature and invariable aspect, requiring only a specific remedy for its cure; on the contrary, it is doubtless quite variable in its nature and requires a corresponding modification in its treatment, according as the morbid condition of the case may partake more or less of the character of plethora, anæmia, congestion, inflammation, spasm, or neuralgia. The compound tincture here proposed contains ingredients long known to exert a controlling influence over several of these morbid conditions; but let us proceed with our subject.

Soon after I commenced the practice of medicine, I received from my brother, Dr. Robert Fenner, of Jackson, Tenn., a *recipe* for an emenagogue which, he said, was not to be found in any medical book of the day, but had been given to him by our father, a practitioner of forty years experience, and he had gotten it out of an old English work, then extinct, written by a Dr. Falk, of London. I was told that it was an excellent emenagogue, more especially in that painful form of obstructed menstruation called *dysmenorrhœa,* and that it was remarkable for almost invariably causing fruitfulness in the cases of young married women.

The following is the original *recipe* and directions:

℞. Gum guiac, ℥i.
Balsam canadens, ℥i.
Ol. sassafras, ʒii.
Merc. corrosiv. sublimat., ℈i.
Rect. spt. vini (alcohol), ℥viii.

"Dissolve the guiac and balsam in one half the spirit, and the corrasive sublimate in the other. Let the guiac and balsam digest for several days; then pour off the clear liquor, mix with the sublimate and add the oil. *Dose*—Ten or twenty drops night and morning in a glass of wine or water, *pro re nata.*"

This was called by Dr. Falk, "*Tinctura Antacrida.*"

I have continued to use this prescription for dysmenorrhœa ever since I first received it and with the most satisfactory results. I have given it to my brother practitioners wherever I have lived and they have all pronounced it the best remedy they ever used for this complaint. It is only very recently that I accidentally discovered in the tenth edition of Ellis' Medical Formulary, the same *recipe,* though not exactly the same directions for preparing it (See page 189 of that work). It is there placed amongst his alteratives, with the commendation of Dr. Emerson and other practitioners of Philadelphia, in lues venera, etc., but no allusion whatever is made to its admirable virtues as an emenagogue. Dr. Falk also used it in lues, and I myself have had some interesting experience of its virtues in that disease, but I shall confine my remarks at present to its effects in dysmenorrhœa and sterility.

Directions.—I usually direct the patient to begin a day or two before the expected period and take twenty-five drops in an infusion of sage or sweetened water, night and morning, until the discharge is freely established; then cease till the next period. In obstinate and severe cases, the medicine should be commenced a week or ten days before the period; and if the pain appears, the medicine should be taken every four or six hours till relieved. The pain usually disappears as soon as the discharge becomes free; but in most cases the discharge comes on without pain after taking a few doses. I have known immediate relief to be given by a single dose taken in the paroxysm; but I have seen cases in which the pain was excruciating, causing shrieks and even violent convulsions. In such I have had to resort to a more prompt and efficient anæsthetic, as the inhalation of chloroform, or the following, which I have often known to act like a charm:

℞. Spirit Camphor, ʒiii.
Chloroform, ʒii.
Tinct. Opii., ʒi. ♏.
S. A teaspoonful in sweetened water once an hour till relieved.

In violent hysterical spasms there is nothing comparable to the inhalation of chloroform. In the treatment of dysmenorrhœa, it is important to obviate costiveness by the use of aloetic pills. When dysmenorrhœa is relieved by this treatment, conception almost invariably soon occurs in married women, as will be shown by the following reports.

Case 1.—Mrs. M., ætat. 27; a very small, delicate lady; married seven years; had an abortion within the first six months; afterwards severely afflicted with dysmenorrhœa and luchorrhœa; no other conception. At the time she commenced this medicine she was pale, feeble, and still troubled with her old complaints. The first application of the tincture caused her to have her period free and easy. She conceived soon afterwards, and in due course of time gave birth to a fine boy, who is still living, though the unfortunate mother fell a victim to pelvic abscess.

Case 2.—Mrs. W., ætat. 28; a lady of good size and constitution; married seven or eight years; mother of three children at full time, but afterwards lost them all. She then fell into bad health, and suffered severely with dysmenorrhœa. At one of her periods she had violent spasms, and I was called in consultation. This was long before the discovery of the anæsthetic virtues of æther and chloroform. She was relieved by the large doses of camphor, recommended by Dewees in his work on the *Diseases of Females*. I then advised aloetic pills to obviate costiveness, and directed her to take 20 drops of the above tincture night and morning, commencing a few days before the expected period. The relief was effectual, and she soon began to breed again. I had the pleasure of seeing her a few years afterwards in fine health, and with two fine children by her side.

Case 3.—Mrs. B., ætat. 31; a lady of fine size and good constitution; married 14 years; had an abortion not long after marriage, but conceived no more; had been severely afflicted with dysmenorrhœa up to this time; she was rather pale and feeble, but not emaciated. She and her husband had long since despaired of family increase, but were desirous of trying any thing that promised relief to her sufferings. I had never relieved a case of so long standing, but assured them there was still hope, and put the lady under the above treatment. As they lived some distance from the city, I directed that if the next period should pass off without pain, she was not to take the medicine at the following period, unless there should be a recurrence of pain. This was a precaution in case conception should take place. About two months after, I received a letter from the husband, stating that his wife had her first period free and easy, and had missed the second entirely, wishing to know whether the medicine should be continued; I directed her to desist, and wait for the consequences. In due course of time she was delivered of a fine boy, who still lives, the hope of the family.

Case 4.—Mrs. B., of N. O., ætat. 30; medium size; rather delicate; been married eight or ten years without conception: been a great sufferer with dysmenorrhœa; also had engorgement and ulceration of the uterus, for which she had undergone long-continued treatment before I knew her.

She became a patient of mine three or four years ago. I found her pale and feeble, and still suffering with dysmenorrhœa—some engorgement remaining, but no ulceration. Leuchorrœa somewhat troublesome. I prescribed the antacrid tincture, which brought great relief, but not permanent, the pain returning whenever she omitted the medicine. She told me that on one occasion the pain was so severe that in desperation she took one or two teaspoonsful at a dose, and with prompt relief. I never knew so large a dose to be taken before or since. She has never yet conceived. Her health is improved, but she now suffers occasional severe attacks of facial neuralgia.

I have met with but one case of distinct membranous dysmenorrhœa, and that was treated without complete success, though considerable relief was afforded. The case was of long standing in a stout Irish lady. In this case there was some engorgement and great constriction of the cervical canal, so that a very small probe could only with difficulty be introduced. She dispensed with my services before I had tried all the expedients at my command.

In a recent severe case in a yellow servant girl, aged 19 years, I found the cervix hard, very much elongated, and terminating in a point. I did not ascertain whether there was any membranous discharge. Under the use of this medicine, for three months, the discharge became free and easy.

I might give numerous cases of newly married ladies in which I was consulted for dysmenorrhœa. This tincture hardly ever fails to restore the healthy function of the uterus, and "send them on their way," but not always "rejoicing," as the following incident will show. A gay, sprightly and robust married lady, mother of three or four children, and without the least desire for more, after indulging too freely in the fashionable dissipations of the *"gay season"* in New Orleans, began to have symptoms of engorgement of the uterus, and more than ordinary pain at her monthly period. I was consulted, and gave the ordinary advice in such cases; urging upon her the necessity of abstaining from late hours, too much dancing, and rich suppers. She disregarded my advice, got worse, and sent for me again. She was now a plain case of dysmenorrhœa, and I knew what would soon cure her; but I also knew that if aware of the probable consequence, she would not take the medicine. So I took the liberty of judging for her what was best to be done under the circumstances, and gave her the tincture. Her next period passed free and easy; but it was not long afterwards that she found, *to her horror*, that she was pregnant. She has since borne two fine children, but I have never ventured to reveal to her the trick that was played on this occasion.

In several instances where the ladies had learned from *experience* the *only danger* of this medicine, after putting it off until they could see no other prospect of relief, have taken the medicine again, and were soon in the condition they expected.

Only a few months since I was consulted by a young married man about the case of his wife, who was suffering greatly with dysmenorrhœa. They had been married about twelve months. The young wife had been afflicted with this complaint during the whole time, and was now very pale and feeble. Getting no relief from the prescriptions of the family physician, they had been induced to consult a *Medical Spiritualist*, one of the various charlatans who practice on the credulity of the people in this city, with great profit to *themselves* alone. He said the spiritualist told him "his wife would get better and then worse again, and thus go on for a long time, but finally recover her health; and that there was no medicine that would do her much good." The gentleman permitted her to go on this way for several months, but finding his young wife continually declining, and her monthly pains increasing in violence, he resolved to seek further medical advice. About this time he happened to mention the case to a friend whose wife had suffered in a similar way, and been relieved by medicine obtained from me. Upon this he came to consult me. I prescribed this tincture, with aloetic pills to obviate costiveness, and at the same time foretold what would be the *probable consequence*, which did not seem to terrify either of them. The very next period passed off free and easy, and she has had no return. Her health is greatly improved, and they are now rejoicing in the prospect of an heir.

In the dysmenorrhœa of virgins, a complaint by no means uncommon among our delicate young ladies of the South, I have known this tincture to produce the happiest effects. I have prescribed it in numerous instances, and never knew it fail to give relief when directions were properly attended to. Some of the most severe and obstinate cases I ever met with were in young girls from the age of fifteen to twenty years. As in married women, these unfortunates sometimes suffer excruciating agony, and even go into spasms. In this emergency I usually prescribe the compound tincture of chloroform mentioned in the preceding portion of this paper.

I will here close my remarks for the present, and perhaps resume the subject after we have perused the valuable essay of M. Robin, which will be continued in the succeeding numbers of this journal until finished.

GUN SHOT WOUND OF THE FACE AND NECK—LIGATURE OF COMMON CAROTID ARTERY—PYEMIA—DEATH.

Reported by S. Choppin, M. D., Professor of Surgery New Orleans School of Medicine, Visiting Surgeon Charity Hospital.

The subject of these remarks, W. L., aged 38 years, was admitted into the Charity Hospital in October, 1857, for a gun shot wound of the face and neck. The ball had entered the right cheek and penetrated the inferior maxillary bone at a point midway between its symphysis and ramus, and then ranging downwards, fractured the great cornua of the os hyoides and wounded the superior thyroid artery. Considerable hemorrhage followed this injury, as the patient was in a complete state of collapse when brought into the Hospital. His nervous system was very much depressed, his pulse quick, feeble and fluttering, and his body covered with a cold clammy perspiration. At the time of his admission the bleeding had entirely ceased. Stimulants were administered and the patient soon rallied from this state of prostration, but complained bitterly of great pain in the region of the neck, which was aggravated by the act of swallowing. Deglutition was so painful indeed, that it led me to the opinion that the œsophagus was wounded. Swelling of the neck and symptomatic fever soon followed. In a few days an abscess formed in the neighborhood of the os hyoides, which was opened and a quantity of bloody pus evacuated.

Ten days after the admission of this patient in the Hospital, when, to all appearances, everything was progressing favorably, he was seized with a fit of coughing and threw up a large quantity of arterial blood, which also flowed freely from the wound in the neck.

Being absent from the Hospital at the time, the assistant surgeon was called to the case, who soon arrested the bleeding by means of compression and the perchloride of iron. This lasted six hours, when another hemorrhage occurred more profuse than the first, which, however, yielded to the same means.

The patient was then exhausted from loss of blood—stimulants were administered, and a faithful nurse placed at his side to watch him. At 2 o'clock, A. M., fourteen hours after the occurrence of the first hemorrhage, a third occurred, and I was immediately called to the patient. I found blood pouring forth from the mouth and neck. The neck was much swollen, the tissues infiltrated with blood and large clots of coagulated blood lay between the superficial and deep cervical fasciæ. This, together with the impossibility of knowing exactly what vessel was wounded, precluded the idea of tying the artery at its seat of injury. I determined at once to place a ligature upon the common carotid artery of the affected side. This was done by making an incision three inches long on the internal border of the sterno-cleido mastoid muscle, extending from the cricoid cartilage downwards towards the sternum. After dividing the skin, cellular tissue, platysma-myodes muscle and superficial aponeurosis, the sterno-cleido mastoid muscle was dissected and drawn to the external side of the wound. The deep fascia or fibro-cellular layer which extends from the sterno hyoid to the thyroid muscles, to the posterior surface of the sterno mastoid and upon the fore part of the vessels, was next divided. The omohyodeus muscle now became apparent, and as it interfered with the action of the knife, I divided it upon the director. The sheath of the vessels was now laid open, the artery separated from the internal jugular vein and pneumo-gastric nerve and ligated. The hemorrhage ceased immediately. Stimulants were administered. The patient, although weak, felt comfortable. For a few days his condition seemed to improve, but this improvement was destined to be but of a short duration. He was seized with violent rigors recurring at short intervals, followed by exacerbations of fever and sweats. He complained of cough and violent stitches in his side; his respiration became hurried and labored, his pulse quick, and symptoms of cerebral disturbances showed themselves. At the same time the wound took on an unhealthy appearance; the granulations became pale and livid, and the pus dry and unhealthy. The skin took on a sallow and waxy hue. In fact all the terrible symptoms of purulent absorption or pyemia were present. All means calculated to relieve the patient were used but the disease progressed steadily on.

Amid these symptoms, ten days after the ligature of the common carotid artery, there was an oozing of blood from the wound which must have been caused by the establishment of the collateral circulation from the opposite side. This hemorrhage, however, yielded to ordinary means, but the patient died a few days afterwards with all the symptoms of purulent infection.

The autopsy made a few hours after death, confirmed the diagnosis. It revealed the existence of numerous abscesses in both lungs, and also large purulent effusions in the thoracic cavities. There were also evidences of suppurative phlebitis in the internal jugular and subclavian veins. Purulent collections were found here and there in different portions of the body.

The examination of the artery about the seat of ligature was especially interesting, as the opportunity of examining the state of a large vessel, so soon after an operation of this kind, does not often present itself. Below the ligature there was a well formed coagulum about three-quarters of an inch in length, intimately connected with the coat of the artery. Above the ligature there was a similar clot, but of a lesser size, also adherent to the internal coat. Above and below the clots the vessel was quite permeable. The brain when examined was found in a healthy state, showing no signs of imperfect nutrition. During life there was no paralysis or any other symptom indicating softening of the brain.

This case, notwithstanding its fatal termination, presents several points of interest. First, it proves the correctness of a great principle in surgery, that the *proximal and distal ends of a divided artery should always be tied in preference to the common trunk, as a safe-guard against the occurrence of secondary hemorrhages.* This principle, first advocated by the justly celebrated Guthrie, is recognized by most modern surgeons as being the correct maxim in surgery, but we still occasionally meet with operators who will prefer to cut above the seat of injury and ligate the main artery between the wound and heart, simply because the operation is generally easier of performance and more brilliant. Had I seen the patient soon after the occurrence of the injury, before the swelling, suppuration, and infiltration of tissues with blood had taken place, I would have certainly cut down upon the neck and ligated both extremities of the bleeding artery. The recurrence of the bleeding ten days after the ligature of the common carotid, proves the absolute necessity of carrying out this rule when it can possibly be done.

The other interesting feature of this case is to be found in the co-existence of *phlebitis* and pyemia. All pathologists of the present day agree to the fact that the destructive effects of pyemia are due to the alterations that take place in the blood when it has been admixed with pus, but they disagree as to the manner in which pus finds its way into the blood. That the pus is bodily taken up by absorption from the surface of a wound, is an opinion no longer tenable, since the microscope has taught us that the pus corpuscle is too large to circulate in the minute capillaries. Nor is it to be supposed that pus enters the circulation by means of the open mouth of a divided vein or artery. The theory which to my mind appears the most rational, and which is corroborated by the present case, is that which attributes the entrance of pus into the circulation as a consequence of phlebitis. It is formed directly by the inflammation and suppuration of the internal coat of a vein, and thus poured at the moment of its evolution into the current of blood passing along the vessel.

We do not absolutely deny the possibility of the existence of pyemia without suppurative phlebitis, but we can assert with positive certainty that pyemia never exists without some local inflammation and suppuration, and that it is oftener found co-existing with suppurative phlebitis than with any other inflammatory affection elsewhere situated.

REMARKS ON "GRAEFE'S OPERATION" FOR THE RELIEF OF GLAUCOMA, AND OTHER AFFECTIONS OF THE EYE—WITH A CASE.

By C. **Beard**, M. D., Professor of Anatomy, New Orleans School of Medicine.

The application of iridectomy, to the relief of glaucoma, a disease hitherto considered beyond the resources of our art, deserves to be recorded as one of the most important achievements of modern ophthalmic surgery. Notwithstanding the denial of its efficacy, by those distinguished oculists, Wharton Jones, and Mackenzie, and the opposition made by them, in a recent number of the Medical Times and Gazette to the truth of Graefe's discovery, it is now pretty generally conceded, by many of the English and most of the Continental oculists, that the disease is in many instances curable, and that it may be relieved by the operation, to a greater or less degree in nearly all cases. So successful has the operation been in the hands of Messrs. Critchett and Bowman, by the latter of whom, it was introduced into England, that we already find claims laid to the merit of having discovered it; or at least there has been some outcry made in an endeavor to identify it, with certain operations of paracentesis, for the relief of the intra-ocular pressure, so manifestly present in glaucoma. The operation differs, however, very materially, and has produced results never obtained from paracentesis.

I am not yet in possession of the process of reasoning by which the distinguished oculist of Berlin was led to apply iridectomy to the relief of glaucoma, and although I concede that he has done more than any one of his day, in elucidating obscure points in the normal and pathological anatomy of the eye, and especially as regards the pathology of the disease in question, yet I am inclined to the opinion that he was led to apply the operation, in such cases, from having remarked the material improvement which invariably results to the cornea and iris in cases where the operation has been made with a view simply of establishing an opening for the passage of light.

Cases frequently present themselves, in the practice of every ophthalmic surgeon, in which, while artificial pupil is manifestly indicated as the only means of restoring vision, the operation promises little, in consequence of the unhealthy condition of the choroid, iris, or cornea, independently of other lesions, usually the effects of inflammation, which may render the operation necessary. I have in several instances operated in such cases, with beneficial results, entirely unhoped for; the improvement to the whole organ, both in appearance and function, has been striking. Now many of these cases present symptoms not unlike those observed in glaucoma. I saw one last winter in the Charity Hospital, in which, while nearly total disorganization of the right eye existed, the left, in addition to a closed pupil with posterior synechia and partial loss of substance of the cornea, presented the bluish and turgid veins of the choroid, the dull hazy condition of the cornea, and the peculiar greenish hue of the glaucomatous eye, while a degree of blindness existed, not to be accounted for by the obstructions to the passage of light. Iridectomy in this eye, resulted in the removal of the unhealthy appearance of the globe, and in the restoration of a very excellent degree of vision. Such a case as this cannot certainly be called one of glaucoma, and yet it presents very striking analogies with it. So much so indeed, that to apply to the cure of glaucoma, an operation which produces such striking results in a case like the one described, is a most natural step.

Graefe has been the first to point out the extensive application which may be made of this operation, in another series of affections of the globe.

Every surgeon who has frequently made the operation for artificial pupil, must have observed that in those cases wherein a partially staphylomatous condition of the cornea existed, benefit resulted from it viz: that albugo diminished; that what could be seen of the iris, looked more healthy; and that the general appearance and condition of the whole organ improved. There is a case now in Ward No. 11, Charity Hospital, in which existed staphylomatous protrusion, with extensive opacity of the cornea and anterior synechia; artificial pupil by iridectomy, at a point presenting the greatest surface of transparent cornea, has not only served as an opening for the admission of the rays of light, but has resulted, now some six weeks since the operation, in the almost total reduction of the staphyloma, and in such an improvement in the transparency of the cornea, as to enable the patient to see quite distinctly.

I have no doubt of the propriety of the operation in another series of cases, in which Graefe recommends it and in which I have tried it, with beneficial results. I have reference to long standing chronic iritis, in which partial or total posterior adhesions exist, and in which, according to Graefe, repeated attacks of acute inflammation are liable to occur. I have made the operation in these cases, not with the view of curing the inflammation, but of making an opening for the passage of light, rendered necessary by the partially or totally occluded pupil; as above remarked I have always observed a great improvement in the appearance of the visible textures of the globe, an improvement which had failed to be a response to any other method of treatment previously used.

I have thus far had an opportunity to apply iridectomy in glaucoma in but one case. I report it as I think it is of interest, not only on account of the result obtained, but inasmuch as it presented several phenomena not always observed. I have not heard of the operation having been previously applied in this country.

Case.—Madam D., a native and resident of Attakapas; aged about fifty-five years, delicate appearance, nervous temperament, has led a temperate, easy life, and generally enjoyed good health. Three years ago she was attacked with pains, which from her description, were probably of a neuralgic character, located in the frontal and supra-orbital nerve of the left side. The globe of the eye became affected in a similar manner, and she suffered intensely. The first attack lasted a short time and then subsided to recur again with renewed violence at short intervals. This condition of things continued three months; sight gradually becoming dull until its final abolition.

About four months ago the large branches of the fifth nerve on the right side became affected, in a somewhat similar manner, although the pain was not so severe. Sight became gradually impaired in this eye until the patient could only distinguish the shadows of large objects.

I saw the patient for the first time in the beginning of April. Her eyes then presented the following appearance and symptoms:

Left Eye.—Conjunctiva discolored and a little injected; sclerotic bluish, with the veins of the choroid visible through it at certain points; cornea hazy and somewhat dim; iris pressed forward so as to make its anterior a convex surface, thus lessening the depth of the anterior chamber; the striæ scarcely visible, and altered in color to a leaden hue; pupil smaller than natural, uninfluenced by belladonna; lens partially opaque; the whole globe hard to the touch, and vision entirely gone.

Right Eye.—A somewhat similar condition of things as regards the sclerotic, cornea and conjunctiva. Induration here also marked, the globe feeling like a marble under the fingers. The pupil moderately dilated, unresponsive to light, unaffected by atropine, presenting a greenish aspect, with what appeared to be a partial opacity of the lens, of that color; anterior chamber shallow; iris projecting forwards; striæ invisible, and apparently tense and on the stretch; vision so much impaired that outlines of large objects only are seen.

Almost every species of treatment had been made use of when the patient came to me. I could suggest nothing which might prove of service but iridectomy, and determined, therefore, to make the operation.

Having placed the patient on a sofa opposite a bright light, and being assisted by Dr. Grall, the lids were separated, and the globe held steady by the fingers. A cataract knife was then introduced at the outer edge of the cornea, as near to its junction with the sclerotic as possible, and an incision made about two lines in length. The knife was then withdrawn when a portion of the aqueous humor escaped; the fine iris forceps was then introduced, and the iris, being caught at its pupillary margin, was dragged through the wound in the cornea and cut off. There was no hemorrhage, as is almost invariably the case when iridodyalisis is made. The wound was very slight. The eye was closed by thin isinglass plaster, and cold applications ordered.

It is now three weeks since the operation, and I am enabled to note the following changes:

The appearance of the whole organ is much improved. Its preternatural hardness is entirely gone. The iris is beginning to look natural; its striæ are visible, and its convexity much less. The pupil is not so much discolored; there is still a greenish hue in the centre of the pupil, but around it is a healthy black circle, through which light passes. There is no pain, and the patient's sight has materially improved. She can now see the hands of a watch, and indicate the position of the numbers on the dial-plate without being able to define them; she can recognize faces, and see to conduct herself.

The case seems to improve daily, and I think will continue to do so; and although it cannot be cited at present as one of complete success, yet have the results of the operation been sufficiently striking to render the case worthy of notice. We must remember, moreover, in estimating the value of the operation in such cases by the results in this, that it was scarcely of such a nature as would have been chosen by an advocate of the operation as a test of its excellence, having advanced fully to the second stage.

CASE OF POISONING BY STRYCHNINE.

By G. E. Elmer, of Marksville, Louisiana.

Messrs. Editors.—I send you the particulars of a case of poisoning by strychnine, which gave rise to no small degree of excitement in our proverbially quiet village a few days since, wherein the patient's life was saved by the liberal exhibition of opium. Should you see any thing worthy of interest therein detailed, you are at liberty to give it a place in that useful and highly respectable journal, the "New Orleans Medical News and Hospital Gazette."

A man, R. R., between fifty and sixty years of age, of healthy constitution, and active and laborious habits, but living a very distressed and unhappy life at home, had strychnine administered to him in a cup of coffee, by his wife. He swallowed but half of the contents of the cup, and shortly after complained of an unusual feeling in the mouth and throat, and an intolerable itching and tingling sensation in the lower extremities, which induced him to remark to his wife that he had taken something in his coffee that was not natural. She immediately sent a messenger for me. I arrived about three quarters of an hour after he had taken the coffee. Mrs. R. remarked to me that her husband had been drinking largely of ice water all the evening, and that probably he had a cramp of the stomach. This he denied. On entering his room and examining into his case, I immediately perceived that some sudden and violent disturbing agency had been brought to bear upon him of a nature to produce spasm and convulsions, though I was ignorant at the time that strychnine had been administered to him, this fact being disclosed next day by the wife's own confession. On further investigation, with a view to a correct and rational prescription, I recognized the following phenomena: rigid and permanent fixation of the muscles of the lower jaw, rendering it quite difficult to open the mouth; lower extremities rigid and stiff, and incapable of being flexed; violent constricting pain, referable to the epigastric region, with a sense of tightness across the chest, and some slight difficulty of breathing.

Prescription.—Sixty drops of tinct. opii., to be given immediately; the same dose to be repeated in due time, should the symptoms seem to evince no disposition to abate. Half an hour passed, when there was a necessity for repeating the dose. The patient then experienced relief, and fell into a quiet slumber. My morning visit found him free from all pain and uneasiness. Ten grains of calomel, followed by a dose of castor oil, then were administered, which ended the treatment. On the trial of the case, as to the question of bail, a few days afterwards, before his Honor Judge Cullom, Mrs. R. confessed that she had put into the coffee as much of the strychnine as could be put on the end of the blade of a small knife. From the rapidity of the effects consequent on the exhibition of the poison, I think I may safely infer that half a grain at least was taken. Enlightened as is the present state of our science upon all subjects in relation to poisons, and their *modus operandi* upon animal life, still an antidote to strychnine is yet a desideratum in practical medicine. The most rational course, therefore, to be pursued in the management of a case of this kind, is, in my humble opinion, to treat it as a case of tetanus, relying upon our great sheet-anchor, opium, with such adjuvants and other appliances as exigencies may seem to authorize or require.

[The writer does not seem to be aware of the fact that camphor has recently been very highly recommended as an antidote to the poisonous effects of strychnine. The July number (1857) of the London Lancet contains the report of a case of poisoning by strychnine, in which the administration of solid camphor in three and four grain doses proved highly beneficial.

Experiments have also been made upon animals, which go to show that camphor possesses the power of preventing and allaying the symptoms produced by an over dose of strychnine. Recently other cases have been reported, all proving the efficacy of the remedy.—Eds.]

SPECIAL SELECTIONS.

GULSTONIAN LECTURES FOR 1858, AT THE ROYAL COLLEGE OF PHYSICIANS.

By John Addington Symonds, M.D., F.R.S.E., Consulting Physician to the Bristol General Hospital, etc.

On Headache.—*Lecture* 1—continued.—It is an interesting but a difficult investigation to ascertain the mode of production of some of these sympathetic pains in the head. Here, for instance, is a question. What is the cause of a so-called bilious or sick headache in a person who suffers such an attack once in a year or so? In him there cannot be any great susceptibility on the part of the cerebral nerves, or it would occur oftener. But what is the course of events when it does occur? We need not stop to ask whether it is some imperfectly chymified substance in the duodenum, or some depraved secretion which begins the mischief. We will assume it to be some disturbing impression on the nerves of the alimentary tube.

This impression may or may not produce a sensation of indigestion before the headache. If it does, we might suppose that the impression has passed by extension from the part of the sensorium in relation with the nerves of the stomach to that which is in relation with the nerves of the head, and that the pain is, so to speak, reflected on the latter nerves. If this is the course, I think it must be presumed that there must have been some previous disposition in the centres related with the nerves of the brain to be so affected.

I doubt if this hypothesis is so tenable as the supposition that the impression is communicated along the sympathetic chain to the nerves of the brain, and there excites the disturbance.

Whatever may be the function of the sympathetic nerves as to sensation, motion, and reflex action, it is impossible to observe their intricate nexus, as well as their distribution, without suspecting that one part of their duty must be internuncial between the viscera, and that they must be the agents of that consentaneous operation without which the functions of organic life could not long continue.

Order in time must be as necessary in the human microcosm as in the macrocosm of the universe. Were the processes of digestion, sanguifaction, respiration and circulation, to go on independently, the vital machinery would soon come to a stop. The heart beats at definite intervals, the respiration keeps a proportionate time, food should enter, and its residue leave the system at regular periods. But the rates are perpetually changing, from the variations of daily or even hourly life, and the changes must be announced from one part to another, in order that the requisite adjustments may take place. Without entering, however, more minutely into speculations as to the final cause of the intimate nervous connexions of the viscera, we may content ourselves with pursuing the way of exclusion. The cerebro-spinal nerves mixed up with the ganglionic nerves account for as much of sensation and emotional influence and reflex action, as we meet with in the viscera. The contractions of the muscular fibres of organic life, probably have special relations with true ganglionic fibres and ganglionic centres, uni-polar, bi-polar, etc. Still there are a large number of plexuses and nerves, entering and departing from those plexuses, whose function is scarcely accounted for, unless we infer that their office is to keep up a connexion of some sort between the viscera.

A general survey of the ganglionic system and the connecting nerves, impresses us with the unity of the whole system. But without the *à priori* assumption that organs so connected must work together, we have strong *à posteriori* evidence of the connexion in the events of disease. One organ, the variations of whose actions may be considered typical of the rest, is more easily observed than the others. I need not say that I allude to the heart. Much has of late years been done to prove the dependence of its rhythmical action on ganglionic centres. Since these researches, I think we can no more definitely understand than ever we could understand before, the readiness with which the rate of the cardiac pulsations is affected by disorders of any organs within the ganglionic chain. The intermittent pulse of indigestion, the alarming rapidity of the heart's action from inflammation of the peritonæum, or the depression of its action from injuries without loss of blood or severe pain—show that this regulating central power of the circulation, not only receives tidings of distant organs with great quickness, but also that it is seriously perturbed by them. There is every reason from analogy to infer, that, although we have not such ready means of noting the influence of other ganglionically connected organs on each other as in the case of the heart and the other viscera, yet that communications are continually going on in health, and that the same links of communication produce the association of morbid phenomena. That the connexion in health is quite irrespective of sensation is obvious, from the fact that in perfect health individuals may live for long periods of time unconscious of internal viscera, and, therefore, that their harmonious action had not been indebted to sensation. The sensory nerves of the viscera seem to have no other function than that of denoting unusual states of the organs. But if this be the function, the question arises, What possible good can accrue from the transference of the sensation from the sick organ to one that is well, or comparatively so?—a common enough occurrence in sympathetic disorders. The good would certainly appear to be beyond our discovery; but we may endeavor to learn the course of transmission. Do the sensory fibres convey the impression direct to the sensorium? and, before its conversion into a sensation, is the impression transferred to another part of the sensorium, related with sensory ganglial fibres from some other viscus? Is the impression which has been made on the gastric nerves by a lump of ice, allowed to pass over the encephalic cells related with those nerves without being converted into a sensation, while on reaching the cells related with the sensory fibres of the first branch of the fifth nerve, it causes a state which is felt in this nerve as neuralgia? Is this the probable route? or is there not another equally probable? If we admit that impressions are exchanged between the different ganglia, may we not conjecture that the impression made by the ice on the gastric nerves, instead of running along the nearest *rami communicantes* to the spinal ganglia, and thence up the sensory tracks (whatever they are) to the sensorium, or, if you please, by the more direct course of the vagus; instead of either of these courses; I say, may we not conjecture that the impression takes its route up the chain of sympathetic ganglia, making no disturbance till it reaches the ophthalmic ganglion, which, being in a susceptible state, undergoes a certain change, which change, on being imparted to its sensory nerves, excites a sensation of pain referred to the parts over which the sensory nerves of that ganglion are distributed? In this hypothesis I assume the ophthalmic ganglion to be predisposed to disorder, because an organ sympathetically affected with disease, implies a readiness for disease in some part of its structure. A sympathetic disorder is, so to speak, not forced on the sympathizing organ—it is invited by the readiness of the latter to take offence.

Let us endeavor to apply some of these considerations to another particular instance bearing on our immediate subject. A gentleman had for many years been liable to attacks of headache on the slightest provocation. Long-continued intellectual exertion, the excitement of an agreeable party, a journey, any error of diet, would inevitably lead to an attack of headache. During the same period he suffered at different times from pain in his teeth, which decayed rapidy, and at last were removed, and replaced by false ones. This change happened more than a year ago, and since that time he has been almost exempt from pain of the head. In other respects, his health and mode of life have been unaltered. What was the connexion between the diseased teeth and the headache? The morbid impressions on the ganglionic fibres of the fifth pair might, without any stretch of hypothesis, be reasonably presumed to induce a morbid state of the Gasserian ganglion, whether the impressions on the sensory fibres did or did not reach the sensorium, and induce a painful sensation referred to the teeth. The Gasserian ganglion is connected by marked fibres with the cephalic ganglion, from which a large number of nerves pass to the cerebral arteries. The cephalic ganglion probably partook of the morbid condition of the Gasserian, and hence might have arisen so susceptible a state of the ganglionic nerves of the brain, that they may have become disposed to ache under the influence of impressions which, without the predisposition in the nerves, produced in the manner I have described, would have had no effect. It seems to me that if this explanation cannot be accepted, there is no alternative but the supposition that morbid impressions on the dental nerves (not creating pain in those parts) arriving at the central extremities of the nerves, are passed on to that part of the sensorium which is related with the sensory fibres of the ganglionic nerves of the brain, and maintain in that part of the sensorium a morbidly susceptible condition; and that this condition is brought into such action as constitutes pain, whenever the said part of the sensorium receives impressions transmitted from nerves which have been offended by causes acting directly upon them in the brain itself, (as in over-study or anxiety,) or when it has received like disturbing impressions from the nerves of other parts of the ganglionic system.

I confess that the first of these views, which I have ventured to propound, is the one that seems to me more admissible. The course of communication traced in that survey is more direct, and involves less complexity of causation.

I should feel myself to be taking an unwarrantable liberty, were I to occupy the time and attention of such an audience as that which I have the honor of addressing, with hypothetical suggestions if the case would admit of a theory. We have not the means of subjecting ganglia and their nerves to satisfactory experimental observations in reference to sensation. When the vivisector has made his way to the base of the cranium, externally and internally, though he might obtain satisfactory information as to the conduits of motor influence, I know not how it would be possible, in the midst of such tearing and severing of tissues, to determine degrees of sensation, which can only be demonstrated by cries or quiverings.

Allow me to try the hypothesis by another example. A gentleman, after playing at bowls one evening, awoke in the night with slight hæmoptysis. The source was referred by his own feelings and his physician's examination to the upper lobe of the right lung. Whether or not a tuberculous nodule was the nucleus of the disease, there was evidence for a short time of consolidation of a portion of the lung in the right infra-clavicular region, and in the same part pain was felt for a long time afterwards, when the patient laughed vehemently, or made any strong muscular exertion. But after awhile he observed that pain would occasionally come on in the same part whenever his stomach was at all deranged, and that it would subside after a slight eructation of wind. What was the chain of events in the production of this obviously sympathetic pain? Was the morbid impression, which had been made by gas in the stomach on the gastric nerves, transmitted by sensory nerves to the sensorium, and instead of being transformed into a sensation in the part of the sensorium related with the gastric nerves, passed on to the central cells related with the sensory nerves belonging to that portion of lung which was formerly diseased? Or is it not more probable that the gastric impression was transmitted by ganglionic nerves to the pulmonary plexus belonging to the spot of lung in question, and that the impression, acting on the morbid susceptibility left in that plexus by old disease, excited in it another impression, which, having been transmitted to the sensorium, produced a sensation referred to the lung?

There is, however, another element to be considered in that curious process, by which, when impressions are being made on many sensory nerves, some in particular will be converted into sensations, and which may easily lead to a fallacy. It is a familiar fact that, amid the din of confused sounds in a street, or at a dinner party, the only auditory impressions which become sensations, that is, which are presented to the consciousness, may be the words of a friend in colloquy. The cause of this appears to be simply the predominance given to one cluster of impressions by the desire, or the compulsion of the will, in other words, the expectant attention. If of two impressions, starting severally from the gastric ganglionic centre, and its related pulmonary centre, one is more intense than the other, it will become the sensation perceived in the sensorium. Or supposing the intensity to be equal, the prevalence may be determined by the expectant attention. If, for example, the pulmonary impression has been linked with apprehensions as to the nature of the illness, or with previous suffering, it will be perceived to the exclusion of the gastric.

This subject of morbid sympathy as to sensation may, perhaps, be still further elucidated, if we consider the sympathy between the tegumentary surface and the internal organs. What, for instance, is the order of events in catching cold, in the common occurrence of a catarrh? A person, liable to such attacks, may have been standing at a corner of a street, or at an open window, conversing with a friend. According to the degree of interest in the conversation, he may or may not feel chilly; but in twelve or twenty-four hours he has the symptoms of incipient catarrh. What has happened during that period of incubation? What did the current of cold air do to him? It may be said that the insensible perspiration was interfered with, and that matters were thereby retained in the blood which ought to have been eliminated; and that the blood, thus contaminated, excites disease in any predisposed organ—the Schneiderian membrane, for instance. This may be the case; but another view might present itself. It might be inferred that an impression has been made on the nerves belonging to the cutaneous blood-vessels, reaches the ganglionic centres, and in its further diffusion, extends to the grey ganglionic nerves belonging to the blood-vessels of the Schneiderian membrane, and, through them, so alters the circulation and secretion of the membrane, as to produce what is called catarrh. The one view seems at least as likely as the other. But suppose the cold to have operated on some limited portion of the body, say the scalp; or suppose the outward cause to have been damp ground operating only on the feet. It is not easy to presume, in this case, a general defect of elimination. The more probable presumption is, that an impression is made on the nerves belonging to the blood-vessels in the skin, and that they transmit the impression to the visceral ganglia, and disturb such parts as are most prone to disorder.

Or we may take another case for investigation. I have a patient who frequently suffers from severe attacks of headache, and who tells me that one of the most frequent causes is driving in an open carriage in a cold wind. She has no general chill, for she is well clothed, but some morbific impression must be made on the face. In a few hours, the headache comes on. There is here no general interference with elimination, but an impression is made on the skin of the face, which may be transmitted to the Gasserian ganglion, and thence to the cephalic; or it may travel by the route of the arteries to the nerves of the brain. But the road which the sympathy travels may be traced in the reverse direction. Thus a patient is the subject of headache, attended with heat of the scalp, dilated and with throbbing temporal arteries. Cold is applied to the forehead and scalp, and the pain is lessened or removed. How is it thus sedative to the nerves of the cerebral vessels? Its operation on the vessels of the scalp is to cause their contraction; and I presume that this is effected by its operation on the vascular nerves. The impression on them, transmitted to their ganglion and plexus, must reach the nerves of the brain. We cannot think that the cold penetrates the bony case, and so reduces the vascular disturbance within it; or, should any one think that this is not impossible, let us suppose that instead of cold lotions, leeches have been applied. No influence is in this way transmitted through the cranium; the quantity of blood lost is too small to affect the heart or the general circulation, and it is a clear case of sympathy. The relief is afforded here unequivocally through the blood-vessels and their nerves, the latter nerves being the only media of communication between the vessels of the scalp and the vessels of the brain and *pia mater*, whatever communication may be traced between those of the scalp and the cranium and *dura mater*. An analogous train of argument might be pursued, as to the action of anodynes applied to the surface.

The phenomena of a *coup de soleil* also illustrate the principle; for, unless it be thought that the sudden elevation of temperature in the scalp is extended to the cranium, and thus directly irritates the vessels of the membranes and the surface of the hemispheres, there seems no other way of tracking the influence than that which I have already pursued, along the nerves of the external blood-vessels to those of the carotidal plexus.

Cerebral Circulation.—Having thus far considered the relation which painful affections of the head bear to impressions on distant nerves, let us turn our attention to their connexion with the cerebral circulation.

The intimate connexion of the nerves of the brain with its blood-vessels, enables one to account for the difficulty which is often presented to our diagnosis—that of distinguishing the pain which is purely nervous, from that which is the effect of vascular disorder. The peculiarity of the intracranial circulation enhances the difficulty; for though the researches of Dr. Burrows have settled the question as to whether the quantity of blood in the vessels can vary, having proved that it is variable, yet the nature of the outward barrier indicates plainly that the compression of the vessels, and consequently of the nerves, must at times be very considerable. Suppose the nerves be in a normal state, the pressure of distended vessels may occasion pain, as in the inflammation of the pulp of a tooth. Or suppose the nerves to be hyper-æsthetic, very slight changes in the force and the volume of the circulation will distress them. The aggravation of a nervous headache by palpitation, by interruption to the venous circulation, in coughing, straining, or other muscular exertion, must be familiar to most persons. I have just supposed the case of the nerves being in a normal state, and then distressed by the hydrostatic pressure; but, on consideration, I doubt whether pain is ever due to simple vascular distension, unless the latter has been extreme and long-continued.

It is difficult to find a combination of circumstances exactly like that of the vessels and nerves of the brain. Some resemblance may be found in a limb compressed by an article of dress, as a foot by a tight shoe, which in the evening, under the influence of heat and long dependence, begins to swell. The pressure of the shoe makes the swelling painful; but here, again, there is a want of strict similarity. For the pain comes from the superficial nerves of the skin, which are directly compressed. The pain is not like that which ensues when the vascular nerves only are affected. That mere compression of healthy nerves by their distended vessels should occasion pain, is rather negatived by the freedom from pain during violent muscular exertions and strains, which put the vessels of the head under the highest degree of pressure. And again when the accumulation of blood takes place in the other direction, whether attracted into the capillaries by unusual functional exercise, or injected into them by emotional excitation of the heart, or by the more composite influence of alcohol, there is often an entire exemption from pain, and in the latter instance a feeling of great enjoyment. I think it may be inferred that when fulness of the vessels gives rise to pain, there must be an accompanying or preceding unhealthy state of the nerves of the part, or that the congestion must have been long continued enough to beget such textural disorder as the nerves will necessarily partake of. In inflammation the nerves are for the time injured by the changes in which the process consists, as well as by the first action of the exciting cause.

The headache which follows an epileptic paroxysm, and that which attends or ensues on prolonged dyspnœa, or violent fits of coughing, might seem to be examples of pain occasioned by simple disturbance of the brain-circulation; but the latter cases are exceptional; that is, it so often happens that both long-continued dyspnœa and cough occur without producing headache, that we cannot but infer that some nervous element must be introduced into those cases in which pain attends upon difficult breathing and cough. As to epilepsy, though we have much to learn respecting its pathology, yet a paroxysm presents unambiguous evidence that the vessels are enormously strained; and the pain after the attack and the functional disturbance of the brain bear a direct ratio to the severity and frequency of the attacks.

It was while I was considering the relation of headache to altered states of the circulation in the brain, and speculating on the possibility that the duration of a fit of headache might be connected with the time requisite for the adjustment of the disturbed balance, and especially for change in the distribution of the cerebro-spinal fluid, which has been supposed to play an important part in such adjustments—it was with the view of confirming or correcting this supposition that I determined to make some experiments on animals. In the performance of these I had the valuable assistance of my friend, Mr. Michell Clarke, a gentleman well versed in anatomy, and expert in operating.

If the cerebro-spinal fluid replaces blood withdrawn from the intracranial vessels by gravitation, or if, conversely, the recession of the fluid makes room for congestion of those vessels, it appeared to me that were an animal kept some time in a certain position, the cerebro-spinal fluid ought to be found accumulated either in the cranial or in the vertebral cavities; if erect, in the ventricles and subarachnoid space of the brain; if inverted, in the theca vertebralis.

Three rabbits were selected. One was suspended by its ears and fore-legs, another by its hind-ledgs, a third was kept for comparison.

The two suspended rabbits were poisoned with prussic acid after about half an hour, their positions having been strictly maintained. The post-mortem examination was made immediately, and with the bodies kept steadily in the same position.

In the rabbit inverted during suspension the eyes were very prominent, and the membrana nictitans was congested (but there had been no manifestation of distress during life). The vertebral canal was first laid open. All the tissues in the lumbar and dorsal regions were bloodless; those of the neck and cranium were gorged with blood; the membranes and the substance of the spinal cord in the lumbar and dorsal regions were quite pallid. In the cervical portion of the theca there appeared a slight accumulation of sanguinolent serum. On opening the cranium, the bony tissue was full of blood; the meninges were highly congested, and the puncta sanguinea in the cerebral substance were numerous and strongly marked. There was no serious fluid in the ventricles, and none external to the convolutions.

The rabbit which had been suspended by its ears and forelegs was of course examined in an erect posture. The tissues of the head, neck, and back were exsanguine; but those of the lumbar region were in an opposite condition. When the cavities were laid open, the membranes of the spinal cord in the same region were found injected, while those of the brain and the vessels of the brain substance were extremely pallid. Serous fluid was anxiously and carefully looked for in the ventricles and between the convolutions, and at their base, but none was found.

The third rabbit was poisoned with prussic acid, and examined immediately in the horizontal position. There was a pretty equal distribution of blood, and nothing remarkable was observed, excepting the absence of anything like cerebro-spinal fluid, either in the cranial or in the vertebral cavity.

These experiments, while they entirely confirmed the observations and conclusions of Dr. Burrows, as to the production of an increased or lessened quantity of blood in the brain by gravitation, were negative as to any adjustment affeoted by cerebro-spinal fluid.

I thought it well therefore to repeat the experiment on two full-grown and very strong rabbits. They were suspended for an hour. The rabbit which hung by the ears and forelegs was found dead. The other, quite vigorous, was poisoned with prussic acid. They were examined with the same results, excepting that in the inverted animal there was not the same appearance of sanguinolent exudation in the cervical portion of the theca vertebralis which had been found in the first case. The rabbit which had died suspended by his ears and fore-legs was examined with particular care and interest. No serous fluid was discoverable in the ventricles, and none beneath the pia mater—none in any part of the cranial cavity. But the membranes and brain-substances were absolutely blanched. The appearances corresponded with the plate in Dr. Burrow's work, representing those of a rabbit bled to death.

As this observation was interesting in proof of the fatal effects of a posture which diverted blood from the brain, it was repeated on two other rabbits. One was suspended by the ears and forelegs, the other by the ears only. Death occurred a few minutes earlier in the former than in the latter case. The appearances within the cranium were similar to those described in the former instance.

In order to meet the supposition that the animals might have been distressed by the weight of the body, and the strain on the ligaments of the neck, the weight had been taken off by a support applied to the haunches.

Thinking it desirable to ascertain whether the inverted position would occasion death, if continued longer than in the cases of the animals suspended in that posture, a strong middle-sized rabbit was hung up by his hind-legs, and kept in that situation for more than four hours. After being cut down, he looked for a moment astounded, then gave his head a shake, and seemed to have recovered his self-possession. He was in fact quite well, and began to feed quite heartily.

Rabbits are known to hold their lives on a slighter tenure than do many animals. We thought it right therefore to try the effect of the erect posture in one of another order. A cat, being proverbially tenacious of life, was selected. Mr. Clarke found it difficult to keep her head constantly upright. She died in three hours and a half. The brain and its membranes were perfectly bloodless, but there was no fluid in the ventricles, nor under the pia matter, nor in any part of the cranial cavity. Neither could more than a very few drops be discovered in the vertebral cavity.

Whatever may be the physical agency by which the brain deprived of its blood is still capable of filling its bony case, the proof given by the experiment of the degree to which blood may be drained from the cerebral vessels by posture only, has, it seems to me, some considerable value. We see that, independently of the neutritive relation between blood and neurine for the performances of brain function, one of the conditions under which this function is performed is a certain amount of pressure, a greater or less amount of which may produce distressing or even fatal results. In the rabbit the circulation is not so arranged as to maintain even for a short time an adequate supply to the brain under the disadvantageous circumstances of erect posture. While the egress of blood is helped by gravitation, the afflux is to be kept up by the exertion of a heart not vigorous enough to overcome the difficulty presented by gravitation. The heart soon becomes further weakened by the reflected influence of that very state of the brain which its own incapacity has engendered, and fatal syncope is the result.

The fact teaches us also how important to man, with his "*os sublime*," is a sufficiency of strength in his left cardiac ventricle. It may be that in him there is a larger amount of cerebro-spinal fluid normally existent, and that he is furnished with adjustments for his greater variety of postures. But still nothing is more to be apprehended than the result of diminished afflux and pressure, from a failure of the cardiac impulse. And the observation is, I think, confirmed by all clinical and pathological experience.

I need scarcely say that in these experiments it was a surprise to Mr. Clarke and myself that no appreciable quantity of fluid should have been found corresponding to the cerebro-spinal fluid, though it was carefully looked for. Mr. Clarke was at the pains of examining under every precaution the vertebral and cranial cavities in a cat, and in dogs poisoned with prussic acid, but with the same negative results. The examination of a dog was made in the presence of Mr. Henry Clark, an accomplished anatomist, and myself. The whole quantity of fluid collected on bibulous paper from the theca vertebralis, the ventricles of the brain, and the cranial cavity, amounted at the most liberal computation to no more than seven drops.

It is not difficult to reconcile these negative results with the fact that one finds more or less serous fluid in the cranial and spinal cavities of the human subject, because the latter is always examined several hours after death, and when there has been abundance of time for the transudation of fluid in these as well as in other serous cavities. And, *à priori*, a larger proportion might be expected from the exceeding vascularity of the pia mater, which represents sub-serous cellular tissue. But it is more difficult to explain the absence of the fluid in the animals examined by us, when we consider it in relation to the experiments of Magendie, Cruveilhier, Ecker, and others, on the living animal. It would be presumptuous, as it is unnecessary to question the accuracy of such practised observers. And without other evidence I should come to the conclusion that the presence of true cerebro-spinal fluid is confined to the living animal, and that when the quantity of blood in the vessels is diminished by death, the fluid passes into them by endosmotic action.

But I have before me the notes of an experiment made by Mr. Clarke on a living dog, previously rendered anæsthetic by chloroform, which fully corroborated those of Magendie and Cruveilhier. The dog was of smaller size than that which was examined after death, yet nearly sixty drops escaped from the puncture through the occipito-atloid ligament, though from a dead animal of the same size the sum total of serous fluid obtained from ventricles, cranium, and theca vertebralis amounted at the outside to seven drops only.

I have already said that it also remains to be explained how the brain all but emptied of blood, still fills the cranial cavity. Two cats were killed, especially with a view to this point. The brain was drained either by posture, or by division of the cervical vessels, but on opening the cranium the dura mater was found as tense as if the vessels had been full of blood, and yet no serous fluid was discoverable. As there is no ground for attributing resilience to the cerebral substance, we are forced to speculate upon the possibility of some interstitial fluid or serous halitus. As the experiments have proved that the cerebro-spinal fluid of the living animal is not to be found after death, can it be that this fluid instead of being extra-membranous becomes interstitial? Or is there some aëriform substance in the vessels, such as prevents the coats of many of the arteries from collapsing after death, though they no longer contain blood? I have already suggested some experiments to Mr. Clarke in elucidation of this subject, some of which he has already performed, while others are still in progress.

EDITORIAL AND MISCELLANEOUS.

Artificial Rupture of the Amniotic Sac during Labor.—In our last March number we offered a few critical remarks on this interesting subject, dissenting in toto to the published propositions of Dr. B. F. Richardson, of Cincinnati—that "the function of the liquor amnii *does not cease* until the head is properly adapted to the pelvic orifice;" and that "complete flexion, and adaptation of the sub-occipito-bregmatic circumference of the head to the pelvic brim, is by far the most *tedious* step in the mechanism of a prima para labor; usually requiring many hours for its accomplishment." In the late June number of the Cincinnati Lancet and Observer, Dr. R. publishes a "Review of a Criticism," in reply to our objections to his theory. Inasmuch as we prefaced our remarks with a disclaimer of any intent to wound the author's feelings, we feel doubly sorry to see in his Review the evidences of displeasure. This, however, and all remarks which have no bearing on the subject under discussion, we cannot allow to claim further consideration.

So far from admitting that "the function of the liquor amnii *does not cease* until the head is properly adapted to the pelvic orifice," we maintain that the liquor amnii has nothing at all to do with this adaptation, and our observation (we waive all speculation on the subject) teaches us that, in the great majority of strictly normal labors, flexion of the head, and consequent adaptation to the inlet of the pelvis, occurs *after* the waters have been evacuated, and when the womb has, consequently, been allowed to bear upon the child with full force. Notwithstanding Dr. R.'s mathematical data in relation to the liquor amnii and the child—viz : that the former is but 24 ounces, the latter 112 ounces, and of greater specific gravity—we maintain that the child lies within the liquor amnii; that when the uterus contracts, it expends its force on the fluid surrounding the child—not on the child itself—and, consequently, notwithstanding the child might sink if placed in a sufficient quantity of liquor amnii *out* of the body, still, in the consideration of the phenomena of labor, it is to all intents and purposes, "floating in the liquor amnii." We feel sure, however, that Dr. R. understands our meaning, or that he will do so when he recovers from the shock of being reviewed. We repeat, then, that while the liquor amnii is present, the uterus cannot act on the child with full force, and, that, once the os uteri is dilated, if the membrane be artificially ruptured, the labor will be safely expedited by allowing the uterus to press the head firmly against the pelvis, and produce that flexion which is necessary to the proper adaptation of the head to the pelvis. And here we think we can point to the reason why Dr. R. has formed his theory. If a man will sit by and *wait* on a labor, when the os uteri is dilated, he will doubtless find that "complete flexion and adaptation, etc.," is a slow, and "tedious" process; but, with the os uteri dilated, if he will rupture the membranes, (granting that the vagina, vuloa, etc., are in proper condition,) he will then find that "complete flexion, adaptation, etc.," is a *speedy* process. Such is to our mind correct theory in the premises; and we call on the hosts of the profession to say whether it is not proven in practice. When we commenced the practice of our profession, we used to wait on nature, even when the os uteri was dilated, and a "tedious" and unsatisfactory business it was. For years we have pursued the opposite plan, and we have never seen evil result to mother or child; and we have invariably had the pleasure of seeing labor safely expedited.

But Dr. R. asks us some questions, inasmuch as he "prefers facts and reason to mere dogmatism."

1st.—"In the generality of first labors, is flexion *complete* until the plane of the sub-occipito-bregmatic circumference is in relation with the *pelvic brim?*" Certainly not: though we do not see why Dr. R. introduces the word "first" into the question. In the name of common sense, how does a *first* labor differ from a fortieth, in all that constitute the mechanism of labor?

2d.—"Is not flexion a condition of restraint? Is it not produced by the clonic and maintained by the tonic contractions of the uterus meeting with resistance at the pelvic orifice?" Flexion is produced by the clonic contractions of the uterus, in conjunction with the pelvic resistance. While the head is *above* the brim of the pelvis flexion is a condition of restraint, partially maintained by the tonic contractions of the uterus, in conjunction with the resistance offered by the pelvis below; but the moment the adapted head enters the brim, flexion becomes more permanent and *reliable*, because it is, as a condition of restraint, now greatly maintained by the pelvic brim surrounding the proper circumference of the head; and it is now that we see the incalculable advantage of having the liquor amnii out of the way, for, with it present, the uterus cannot expend its force properly on that portion of the child above the brim, and thus do its share towards maintaining flexion.

3d.—"Is tonic contraction of the uterus abrupt or gradual?" We do not think it is either, but regard it as, in normal labors, partially *persistent.*

4th.—"Will the *usual* amount of liquor amnii prevent the head from *gradually* undergoing flexion and descent, whilst the cervix and os uteri are dilating, and the 'bag of waters' protruding?" Yes; we have frequently met with cases, in which, with "the usual amount of liquor amnii," the head has remained extended above the brim, and as soon as artificial rupture of the membranes has been resorted to, flexion has taken place, and the labor has been speedily and safely terminated. That there are cases occurring every day in which nature does "gradually" effect flexion and delivery, we admit; but it is just here that we contend for the value of the services of the accoucheur. Observation teaches us that the rule is for flexion and adaptation to occur *after* the evacuation of the waters, and until we see cause to change this highly important practical view, we can never consent to be inactive after the os uteri is dilated and labor is tedious; because, as we believe, the presence of the waters prevents (or retards, if the term is preferred) that flexion of the head which is indispensable to speedy delivery.

We have now done with the subject. We feel that we have the mass of the profession on our side of the question. In our daily walks we find not one man willing to give up the practice we advocate, and we believe that the principle will stand with time. Again we regret that Dr. R. should have seen proper to exhibit any but the kindest spirit towards us. We found his theory floating along in the great current of medical literature; the subject interested us; we read his paper, (too long far to be inserted in our journal, as he thinks would have been but fair); we thought his reasoning fallacious, and, as journalists, we thought proper to say so. Should Dr. R. write again (and we hope he will, for he writes well,) and we think it necessary, we will again review him; but he shall always find us as ready to praise and approve as to condemn.—D. W. B.

Messrs. Editors.—It affords me sincere pleasure to call the attention of your readers to the able and interesting article by Professor Robin, on different points in the pathological anatomy of the uterus. The various conditions and affections of this organ are still comparatively obscure, especially those connected with the function of generation, including the relations of the fœtus to the mother, and every true lover of the science must receive with pleasure any accession to our previous stock of knowledge on this subject. It is a curious coincidence that your journal commences this day a series of publications which resume on these subjects his accumulated labors of years, and complete the picture, a sketch of which you may elsewhere find in different publications. Take, for instance, Cazeaux's Midwifery, a treatise which represents, better than any other, the present state of our knowledge on this branch—(an excellent translation of it, by-the-by, has lately appeared by one of our confreres, Dr. Bullock, of New York)—and you will find the names of Robin and Coste at almost every page relative to the development of the ovum, and the modifications of the uterus during pregnancy. Now the present memoir is certainly destined to fill up many a gap in the physiology and pathology of the uterus. If the present memoir prove acceptable to your readers, I propose to continue a series of regular articles from our distinguished correspondent; and if I can contribute, by my humble efforts, to spread on this side of the Atlantic, the recent progress of microscopical science, and cultivate a taste for that invaluable instrument, then indeed shall my labor be amply rewarded. It is a remarkable fact, that various scientific discoveries, though they may have originated in the old world, yet when transplanted across the water, have seemed to thrive with a fresh vigor, in many instances becoming simplified, and always more practical: our progress in the arts and manufactures certainly warrant the assertion; and I need scarcely refer to discoveries, which, from Franklin down to Morse, have become *household words.* But a few days ago, I have welcomed on my desk a new work, in which the applications of the microscope to clinical medicine are treated by a master-hand—I mean Bennett's Clinical Lectures on Medicine, published by Messrs. Wood, of New York. The great value of this work is, that it embodies, in a clear and concise manner, all the applications of the microscope to practical medicine. It is, in fact, a perfect manual on this subject, and as such must be welcomed by all who consider diagnosis as the most important element in the study of disease. Let me add that the book, by its typography and excellent plates, is a magnificent specimen of American printing, and reflects much credit on the publishers. Now, I have simply adverted to this book to show, that in England the new science is flourishing with rapid and vigorous growth, and taking at the same time a practical character, which it has scarcely assumed in France itself. Bennett, I believe, received or completed his medical education in Paris; but other English writers, as Golding Bird, Beale, Quekett and several others have written works which amply testify to the utility and importance of this instrument. It is a singular fact, that, while Golding Bird's manual has so simplified urinary diseases, that any one with a few practical lessons on the microscope, can soon diagnose the most complicated urinary deposits, in French there is no similar work; and in Paris, Dr. Rayer is perhaps the only physician who applies the microscope to the diagnosis of urinary affections. It is time, then, to call the attention of the medical profession in the United States to all those contributions which testify to the value of general anatomy, a science which can only be cultivated by means of the microscope. It is certainly, then, a source of pride and gratification to me to lay before the readers of the New Orleans Medical News and Gazette, these productions of one of the most eminent savants of the day; of one who spent many years in the special cultivation of this branch, and who is undoubtedly the greatest microscopist of the age. In order to present him to our readers still more intimately, I shall translate the following letter, which, though not intended for publication, may still prove interesting to many with whom his name is familiar; at all events, it will appear that the translator has some work cut out for him; and were it not a labor of love, it would almost frighten him from the undertaking. I had forgotten to mention that Dr. Robin has been recently elected a member of the Academy of Medicine of Paris.

Section of Pathological Anatomy, by a very large majority—40 votes against 20. However great the distinction, it could scarcely elevate him: he is as much honoring as honored.

A. PENISTON, M. D.

PARIS, May 15th, 1858.

My Dear Friend.—On receiving your last letter, I was about to send you the accompanying memoirs, which I have been preparing for some time past, and which I had already announced to you when you were with me last summer. I would have sent them ere this, had I seen in your journal the article I gave you, "*On Organized Substances, and the means of distinguishing Coagulated Fibrine from the different blastemas.*" I consider this subject as exceedingly important, although, perhaps, it may appear abstruse to some of your readers; still its connection with both physiology and pathology will amply repay its perusal. I am now suffering from a phlegmonous swelling in my right hand, caused by a dissecting wound, so that I shall not be able to give you much news from Paris. In your branch, Bernard has just published two important volumes on the Functions of the Nervous System. I have not sent them to you as you will find them in New York, and have probably received them by this time. Brown-Sequard has published two numbers of his *Journal de Physiologie.* It contains several useful, I may say indispensable, articles on physiology. I shall also recommend to you an excellent thesis, *On the Structure and Development of the Teeth*, by one of my pupils. My new edition of Nysten will not be complete before the end of July; it takes up a great deal of my time, and delays my work on General Anatomy, which you are so anxiously expecting. But I shall send you from time to time the most important parts, which may be interesting to the readers of the journal. These and other manuscripts are at your disposition for the future. The Memoir on the Mucous Membranes is not yet finished, but will be by the time you are ready for it. I have received your journal very regularly, for which I am much obliged to you. I am also authorized to thank the editors officially for the same on behalf of the Society of Biology. Its contents are regularly analyzed and reported upon by the secretary. Let me now congratulate you upon the prospects of the School with which you are connected. Notwithstanding all the difficulties inseparable from an enterprize of that kind, I have no doubt you are destined to occupy a distinguished rank among the institutions of your country:

but you must persevere in your labors and your devotion to science. In one of my previous letters, I spoke to you of a memoir which I have been preparing on the nature and structure of various tumors: let me know when you will be ready for it. Some of the articles which I have sent you have never been published before, and of those which have appeared already, only a bare outline, at best, has been published. But as my writings will be entitled to any claim of priority just as much if published in New Orleans as in Paris, I shall cheerfully send them to you first; and in this I only follow the prompting of my heart and my affection to you.

Adieu, my dear friend; be the interpreter of my best fraternal wishes to your colleagués of the school. Tell them that I wish them success in their efforts in behalf of medical education.

Sincerely yours,
CHARLES ROBIN, M. D.

DR. A. PENISTON, New Orleans.

RESIGNATION OF DR. THOMAS PENISTON, AND ELECTION OF DR. AUSTIN FLINT, OF BUFFALO, N. Y., TO THE CHAIR OF CLINICAL MEDICINE AND AUSCULTATION, IN THE NEW ORLEANS SCHOOL OF MEDICINE. It is with real sorrow that we announce the resignation, by Dr. Thomas Peniston, of the Chair of Clinical Medicine, etc., in the New Orleans School of Medicine. We are recording an event which only gives us pain, and we shall do it in as few words as possible. Impaired health has forced our worthy and most highly esteemed colleague to tender his resignation, and he has gone across the waters to seek repose from his labors and that restoration for which he shall have our heartfelt prayers. Of few men can it be said, that they have done their whole duty in any enterprise whatsoever, for it is man's nature to neglect himself and the world around him; but towards the institution from which our colleague now so reluctantly severs himself we can in all candor say, he has left no part of his duty undone. Dr. Peniston has been thoroughly identified with the interests of the New Orleans School of Medicine from its inception, and has contributed his full share towards its rapid advancement; he has been a faithful and a most popular teacher—the best teacher of Clinical Medicine we ever saw—and, while his absence will be deeply regretted by all who knew him in this most useful sphere, there will be many a heart throbbing warmest wishes for his speedy restoration to health and happiness, and his return to the home of his choice.

The Faculty have been forced to part with one of their most valuable colleagues, but they congratulate themselves and the rising generation of physicians in this region, in having been so fortunate as to fill the vacancy with a man of the eminent abilities and distinguished reputation of Dr. Austin Flint. In the prime of life and possessed of a vigorous constitution—a laborious student, with a mind peculiarly fitted for analytical research—familiar with the sciences generally and fully posted in the progress of modern medicine—an author whose works are unsurpassed by any medical writer in this country—and a teacher of enlarged experience and a happy delivery—Dr. Flint will bring to our aid every accomplishment requisite to insure the success of this young and flourishing Institution.

The literary labors of Dr. Flint have placed him in the front rank of medical writers in this country. He was the founder of the Buffalo Medical Journal and conducted it for many years with signal ability. At the meeting of the American Medical Association in Richmond, May, 1852, Dr. Flint was the successful contestant for the Prize Essay, and wrote "*On Variations of Pitch in Percussion and Respiratory Sounds, and their Application to Physical Diagnosis*," an essay of great ability and displaying a perfect knowledge of auscultation.

The same year he published his "*Clinical Reports on Continued Fever*," a volume of 390 pages, displaying powers of analytical research equalled only by the celebrated Louis, of Paris, and a model of clinical instruction.

In 1856 he published his great work entitled "*Physical Exploration and Diagnosis of Diseases Affecting the Respiratory Organs*," a quarto volume of 636 pages. This is a practical work of great value both to students and practitioners.

At the last meeting of the American Medical Association, which took place in Washington City, May, 1858, Dr. Flint was awarded the first prize for an "*Essay on the Clinical Study of Heart Sounds in Health and Disease.*" This will appear in the forth-coming volume of Transactions.

All these productions go to show that Dr. Flint has devoted his chief attention to the very subjects he is coming here to teach, and that he has already achieved eminent distinction. The prosecution of the laudable objects of his ambition has led him to make two or three visits to Europe for the purpose of receiving instruction from the greatest teachers of the day.

In addition to what has already been stated, we must not omit to mention that Dr. Flint has been a Teacher of Medicine for the last fifteen years, most of the time in the Medical College of Buffalo, but during four winters in the Medical Department of the University of Louisville, Ky., where, as we are informed by those who attended his lectures, he gave entire satisfaction.

With such recommendations as these, we feel assured that we have filled our vacant chair with the best man for the place in the United States, and with the facilities afforded by the great Hospital and Dispensary at our door, we are confident that Clinical Medicine and Auscultation will be better taught in the New Orleans School of Medicine than in any Medical College in America.

New Exchange.—We are in receipt of the first number of the "Maine Medical and Surgical Reporter," a monthly journal of forty-eight pages, edited by Drs. W. R. Richardson and R. W. Cummings, of Portland, Maine. We are much pleased with the contents and appearance of the number and welcome it to our exchange list.

Convention of Medical Colleges.—The following is the resolution reported by Dr. Hamilton, chairman of the Committee of Delegates from Medical Schools at the recent meeting of the American Medical Association:

"*Resolved,* That we recommend to all the medical colleges entitled to a representation in this body, that they appoint delegates, especially instructed to represent them in a meeting to be held at Louisville, on Monday, the day immediately preceding the Convention of the American Medical Association for the year 1859, at ten o'clock, at such place as the committee of arrangements shall designate."

Clinical Instruction.—We beg leave to call attention to the following extract from the Report of the Special Committee on Medical Education, at the recent meeting of the American Medical Association:

"In addition to the present requirements of our schools, there is one *which the time has come for the profession to demand.* We allude to attendance upon a clinical course in a hospital.

"*Clinical instruction* must be considered the great *desideratum* in any system of medical education. It affords the only means by which the student can *rapidly* apply the knowledge which he has obtained from the schools to the practical realities of his professional life. At the bedside, and under the immediate instructions of his clinical preceptor, he systematizes his knowledge, and familiarizes himself with all the details of practice.

"FROM OUR HOSPITALS ALONE *can men go forth to the responsible and perplexing duties of the medical profession, thoroughly prepared from the first to the most trying exigencies.*"

This is what we have urged from the time we first shed ink in the cause of medical education, and the idea is more and more thoroughly pervading the mind of the profession throughout the land. It is this very clinical instruction that our young men who have the means and the ambition have been crossing the Atlantic for; but the day has now come when it is being acknowledged that our own hospitals afford ample material for demonstration, and that men are now to be found who devote themselves to clinical teaching. In our humble opinion, New Orleans affords greater facilities in this way than any city in the Union, and the school to which we are attached will leave no stone unturned in her endeavors to procure the best teachers and fully develop this inexhaustible mine.

CORRECTION.—The June number of the Medical and Surgical Reporter says, that Prof. T. G. Richardson has received an appointment in the New Orleans School of Medicine.

This is a mistake. The only new appointments in this school are those of Dr. Austin Flint to the Chair of Clinical Medicine, and Dr. Grall as Demonstrator of Anatomy.

PROFESSORIAL.—We have received a note from Prof. S. G. Armor, of Cincinnati, requesting us to announce his resignation of the Chair of Pathology and Clinical Medicine in the Missouri Medical College of St. Louis.

Dr. B. L. Jones has been appointed to the vacant Chair of Chemistry in the OglethorpeMedical College.

BOOKS AND PAMPHLETS RECEIVED.

"Medical and Physiological Commentaries. By MARTYN PAINE, M. D., A. M. In two volumes. New York, Collins, Reese & Co.: London, John Churchill. 1840."

"Essays on the Philosophy of Vitality, as contradistinguished from Chemical and Mechanical Philosophy, and on the Modus Operandi of Remedial Agents. By MARTYN PAINE, M. D., etc., etc. New York, Hopkins & Jennings: 1842."

In our last number, we noticed the receipt of PROFESSOR PAINE'S "Institutes of Medicine." We have now to make our acknowledgments for the foregoing valuable works, which are most welcome to a place in our library. We only regret that the size and objects of our journal preclude our giving a more extended notice of the whole of these valuable works.

"Quarterly Summary of the Transactions of the College of Physicans of Philadelphia. From August 5, 1857, to February 3, 1858, inclusive. Philadelphia: Collins, Printer. 1858."

"Life: An Essay. By ETAM EZEL. 1858." We have not had time to peruse this little pamphlet. It comes with the respects of its author (G. D., of Texas). We will look at it.

"Sixteenth Annual Catalogue and Announcement of Lectures of Rush Medical College, for the Session of 1858–59."

LIST OF PAYMENTS TO JUNE 25, 1858.—Drs. J. A. Owens, vols. 3 and 4, $10; G. Hughes, vol. 5, $5; L. H. Burgess, vol. 4, $5; R. W. Burney, vol. 5, $5; Van Allen, vols. 4 and 5, $10; A. L. C. Magruder, vols. 4 and 5, $10; A. J. Thompson, vol. 5, $5; E. S. Drew, vols. 3, 4, and 5, $15; W. C. McGower, vol. 5, $5; J. J. Hulse, vol. 5, $5; J. N. Allison, vol. 5, $5; G. W. Wier, vols. 3, 4, and 5, $15; J. B. Dungan, vol. 5, $5; Kells (N. O.), vol. 5, $5; G. A. Nott, vol. 5, $5; Wedderstrandt, vol. 5, $5; T. R. Finlay, vol. 5, $5; H. Curtius, vol. 5, $5; W. B. Lindsay, vol. 5, $5.

Dr. R. L. Seale should have been duly credited in our May number with $5, for vol. 4.

EXCERPTA.

A CASE OF SUCCESSFUL ADMINISTRATION OF THE HYDRATED PEROXIDE OF IRON AS AN ANTIDOTE FOR ARSENIOUS ACID.—*By Wm. G. Meachem, M. D., Warsaw, Wyoming County, N. Y.*—On the afternoon of Saturday, Jan. 2d, I was hastily summoned to the side of Mr. H. C., of this village, æt. 27, who, as the messenger informed me, was strongly suspected of having poisoned himself. Upon reaching his house, I found him prone upon the floor, his body arched in that curve which unmistakably evinces incipient or confirmed spasmodic contraction of the muscular coat of the stomach and intestines, or of the diaphragm and anterior abdominal muscles, or of both combined. As I took him by the hand and called him by name, he instantly became so convulsed that three able-bodied men who were present could with great difficulty restrain him. The paroxysm soon subsided, its duration not exceeding thirty or forty seconds. Immediately coupling the intimation that the messenger had given me with the initiative cramp of the stomach and bowels, and the rapidly succeeding universal convulsion, confirmed the suspicion of poisoning. His wife informed me that he had told her there was something in his vest pocket which would acquaint her with what he had done to himself. I had the pocket searched, and in it was found a paper superscribed with "Poison, arsenic," and containing the brilliant crystals of arsenious acid.

Being unprovided with either of the prompter emetics, I prepared and administered to the patient an emetic dose of *antimonii et potassæ tartras.* By reason of the extreme rigidity of the facial muscles then existing, I found it next to impossible to introduce the spoon between his teeth. When the spasm had relaxed sufficiently to admit of free deglutition, copious draughts of milk were also prescribed. This done, I sent to procure the arsenical antidote, the hydrated peroxide of iron, and, after an appropriate interval from the former administration of the tartar emetic, which had proved ineffectual in establishing emesis, I repeated the antimonial. The convulsions continued to recur with considerable regularity every fifteen or twenty minutes, persisting each time from thirty to forty seconds.

While awaiting the arrival of the antidote, and in an interval of quiet, I questioned the patient relative to the circumstances of the rash act. With faint utterance he replied that he had purchased the arsenic at one of the drug stores in the village, and had taken a half-teaspoonful of it three several times. Some unsolicited expressions which he made use of, full of a consciousness of his perilous condition, confirmed his direct avowal of having ingested a deadly dose.

He suffered intensely in the stomach and bowels, not simply when the spasms were upon him, but in the quiet interims as well, as was no less plainly evinced by his repeated pressure of the abdominal walls with his hands, by the forward inclination of his body (a position assumed manifestly with the design of relaxing the abdominal tension,) and by the frightful anxiety of his countenance, than positively asserted by his own lips. The convulsive paroxysms were attended with wild, staring, bloodshot eyes, with imprecations and fearful ravings, and with maniacal fury. The previous free indulgence of the patient in intoxicating drink, may have served to intensify the delirium which was a marked symptom of the paroxysms; and yet the arsenic, doubtless, was itself competent to the institution of the most uncontrollable frenzy. When the convulsions left him, he invariably sunk into a temporary prostration, that so nearly resembled his anticipated death, that none but a medical man could suppose him still alive. His pulse, during the interludes, was irregular, uneven, intermittent and accelerated. Now and then, an ineffectual effort at vomiting was made.

The preparation brought was *the solution of the tersulphate of the sesquioxide of iron,* from which, with the aid of *the solution of ammonia, the pulpy, precipitated peroxide* was soon obtained, and its administration in tablespoonful doses every five or ten minutes, commenced. The convulsions still recurred at their usual intervals; but, at length, after repeated doses of the antidote had been given, the agonizing pain in the stomach and bowels was apparently somewhat assuaged, for the patient exhibited less anxiety of countenance, and applied his hands to the abdomen less frequently and nervously, and soon thereafter copious emesis ensued, and general convulsion ceased. The administration of the antidote was still continued at intervals of half an hour; for I was well aware that, though the immediate danger was obviated, it was the wiser plan to persist in the use of the iron; the arsenic remaining, the counter-poison was quite harmless, even in the largest quantities. A second vomiting occurred about an hour subsequent to, and was quite as free as, the first. The ejected matter was apparently little else than the milk, which had been imbibed and colored with the reddish-brown particles of the ferruginous antidote; the unusual quantity of liquid, however, far exceeding the bulk of that which was ingested in the milk. The patient informed me that he had omitted his dinner that day, purposely, no doubt, to insure a speedy and effectual action of the drug, for he had evidently acquainted himself with the preliminary steps requisite to the fatal deed. In confirmation of this it may be remarked, that he had made an attempt upon his life with the same fearful, irritant poison, only three days previously, but then he took so much that his stomach immediately expelled it. This time he had gone to work evidently with a scientific comprehension of what he was purposing, securing an empty stomach to facilitate absorption, and dealing out to himself the arsenic in moderately-divided doses.

The antidote was repeated once or twice after the second vomiting, and then suspended. Cataplasms of mustard were now applied to the epigastrium and soles of the feet, and *infusum ulmi, ad libitum,* was prescribed as a demulcent drink. The excruciating pain in the stomach and bowels began to subside almost simultaneously with the former emesis; and within half an hour subsequent to the latter, the patient expressed himself free from pain, excepting a severe headache. He still continued delirious, but the delirium was of a much more quiet character than that which had attended the convulsive seizures.

At 9, P.M., having been with him since 5 of that afternoon, I left him for an hour and a half, and on my return found him quite talkative, and loudly clamoring for bread. He now declared himself to be in all respects comfortable, excepting a general soreness (the result of his violent spasms,) and an inordinate hunger distressed him. His pulse had resumed a comparatively normal state, and he was evidently rescued from a fearful death. Dealing out a few anodyne powders to be taken *pro re natâ*, enjoining upon the patient the frequent and free ingestion of *infusum ulmi* or mucilage, and instructing the friends to prepare for him some bland farinaceous diet, such as sago, tapioca, or rice-water, should he demand nourishment, I left him for the night.

At my next visit, at 8, A.M., the succeeding day, I found my patient sitting up with his family, but gloomy, morose and irritable. He complained much of sore lips, mouth and throat, a circumstance explained by the fact that he had *licked* the arsenic from its containing paper. Enjoining upon my patient strict attention to diet and regimen, and recommending a continuation of the free use of diluents, I took my leave until the evening, when I again visited him, and found him in social converse with his family and relatives, and willing to engage with me in a lengthened and earnest chat, that manifested a condition of bodily and mental exhilaration and satisfaction with life, the very antipodes of the sullen despair and misanthropic hate of the preceding day.

January 6th.—H. C. is now apparently as well as usual.

The points in this case worthy of especial note, are *the length of time that intervened between the ingestion of the poison and the administration of its antidote, and the speedy subsidence of the irritation and congestion, and, it may even be, the inflammation of the mucous membrane of the stomach.* The interval between the taking of the drug and the first dose of the iron must have been, at the very lowest estimate, *two hours.* There is one other case on record, reported by Mr. John Robson (*Am. Jour. of the Med. Sciences*, xx. p. 522, from the *Lon. Med. Gaz.*, Nov. 5, 1836,) in which the amount of arsenic swallowed and the time that elapsed before the administration of the hydrated peroxide, are identical with the corresponding amount and time in the present case. The milk enveloping and diluting the poison and sheathing the coats of the stomach, in the first place, and the counter-irritation and the use of diluents subsequently, were undoubtedly more or less operative in retarding and blunting the powerfully irritant effects of the deadly dose.

I give publicity to this case, not simply to add to the gradually accumulating evidence confirmatory of the antidotal value of the hydrated peroxide of iron in instances of poisoning by arsenious acid, but also, and more especially, with the design of impressing upon my fellow-practitioners in the country, the propriety, the necessity of being always provided with the respective antidotes of all the deadly drugs; for, whenever a physician is summoned to a poisoned individual, *means ready at hand* are both life to the one and reputation to the other. Every practitioner in the country should regard the supplying of himself with these antidotes as sacred a duty as the acquainting of himself with their existence, their properties and their actions. The importance of the duty must be evident to every one who duly considers how poorly conversant and poorly supplied with these counter-poisons are the generality of country druggists, and how lengthy an interval of time must often elapse, by reason of distance, even under the most favorable circumstances, before the needed assistance can be rendered to a poisoned individual in the rural districts. Let every physician, therefore, whose eye shall fall upon the record of this case, learn from its auspicious result *to be prepared* (if he be not already) to meet every similar emergency, and to conduct it to an equally happy issue.

I have narrated the history of this case, both antecedent and subsequent to taking the patient in medical charge, somewhat *in extenso,* that the reader may be fully convinced that a poisonous dose of arsenious acid was really ingested, and its fatal consequences actually averted by the antidotal energies of the hydrated peroxide of iron.—*New York Jour. of Med.*

ON THE INVESTIGATION OF THE CAUSES OF PUERPERAL FEVER.—*By Professor A. Martin,* (*Monatschr. f. Geb., October,* 1857).—Prof. A. Martin, Director of the Lying-in-Hospital in Munich, enters into an elaborate discussion of the causes of puerperal fever, taking for the basis and motive of his inquiries an epidemic which prevailed in that institution, between December, 1856, and July, 1857. The facts in the Munich epidemics are remarkable. The hospital in which the last epidemic appeared was a new building; the construction was carefully adapted to its purposes. Cleanliness and ventilation were scrupulously enforced. Dead bodies were always speedily removed to a distant building. The discharges, placenta, etc., were immediately placed in air-tight vessels, pending their removal. The syringes, catheters, etc., used in the sick wards, were strictly confined to the use of the patients in them. The sick women were isolated in separated chambers. The attendants were enjoined to wash their hands in chlorine water. As in the Dublin Lying-in-Hospital, the wards were used in rotation, some being always empty to allow of time for cleansing. In short, no care seems to have been spared, Yet the disease spread. From the 1st of October, 1856, to the end of July, 1857, 1,090 patients were admitted. Of these, 88 suffered from puerperal fever, and allied pathological affections, as metritis, phlegmasia alba, phlebitis, etc. Of these 88 cases, 47 recovered, and 37 died, leaving 4 under treatment. One circumstance deserves mention, although it cannot have exerted material influence in the production of the disease. The building was lighted with gas; some of the gas-pipes leaked, and for a time all the wards were tainted with gas-stench. Some of the attendants who slept near the pipes complained of vertigo, headache, prostration and diarrhœa; and as similar symptoms appeared among the patients, it was determined for a while to discontinue the gas-lighting. The most probable cause appears to have been cadaveric affection. During January and February no serious illnesses had appeared. Suddenly, on one and the same day, two puerperal women were seized with fever. Both had been delivered normally nearly at the same hour. In neither case could any cause be detected. At last it appeared that an assistant, without the knowledge of the directors, had examined the body of a child in the remote dead-house, having, indeed, as he declared, carefully washed with chlorine water, and had immediately aftewards examined these two women, and no others. No other women were affected at the time. The guilty assistant confessed that the same thing had happened to him at the outset of the fever in December, when also the patients examined by him were the first seized. The sickness thus originated in December. Several cases of puerperal fever occurred up to the end of February, more or less severe, and it required a space of sixteen to twenty-one days to restore more satisfactory relations. It is further observed that the clinique of the hospital was held every morning from ten to eleven o'clock, when a great number of practitioners proceeding direct from the neighboring cliniques, where they had visited typhoid patients, and often even from the anatomical theatres, came in. Students also engaged in microscopical pursuits visited the wards. The hospital was shut up at the end of April. When first reopened, the cases were milder in form, and less frequent. But when the clinique was reopened in July, and students attended, several severe and rapidly fatal cases occurred. These ceased when the clinique terminated at the end of the session. [This article may be considered a strong corroboration of Dr. Semelweiss's theory of cadaveric infection. Still, in the lying-in-hospital of Bonn, where the writer was formerly an assistant for two years, the students as well as myself often went from the dissecting-table directly to the lying-in-wards, for examining parturient women, without even taking the precaution of washing our hands with chlorine water. But we did not induce a case of puerperal fever, and I know from Prof. Kilian himself, that he scarcely remembered to have seen the disease in his wards for eight or ten years. E. N.—*New York Jour. of Med.*

DIARRHŒA IN THE FIRST YEAR OF LIFE.—Calomel, one-sixth of a grain every third hour, is a remedy, where there is no fever, in the diarrhœa of infants under one year of age. Such is the experience of Dr.

Schuller, of Vienna, in regard to infants who are, or have been, nourished by breast-milk; experience in hospitals, on children artificially fed, gave a negative result. Calomel has the curing effect which has just been stated, only when the diarrhœa is beginning. If it has not been successful in stopping diarrhœa within 24 or 48 hours, it is of no use, and has to be discontinued. Vomiting, while calomel is given, yields no contra-indication. —*Jahrbuch für Kinderheilkunde und physiche Erziehung*, 1858, iii. 164.

RAW BEEF IN THE "DIARRHŒA ABLACTATORUM."—Seventeen years ago, J. F. Weisse, physician of the Children's Hospital of St. Petersburg, recommended for the first time raw beef, scraped, as the best remedy against the diarrhœa setting in after weaning. He repeats his recommendation as warmly now as it has been given in former years, referring to his own experience and the approval of a great many well-known observers. It is a remarkable fact, and worthy of being communicated, that many of the children cured by the use of raw beef, had subsequently tænia solium, which is not usually found at St. Petersburg, but which, according to D. von Siebold (*on Nematodes and Cystici*, Leipsic, 1854) is introduced into the Russian capital by Podolian oxen.—*Journal für Kinderkrankheiten*, Jan. and Feb., 1858, p. 60.

CASE OF PURULENT OPHTHALMIA IN THE FŒTUS—*By M. Rivaud-Landrau.*—M. Rivaud-Landrau, of Lyons, while on a visit to a neighboring town for the purpose of performing some operations on the eye, was shown an example of an affection which, as far as he is aware, is unnoticed in works on ophthalmology—viz: a case of indubitable purulent opthalmia occurring in the fœtus. The child was two days old, and born blind. The upper eyelids were closed, and slightly reddened and swollen at their dorsal edges; and they were glued together by dry purulent sanies. On raising them, the conjunctiva was found red and inflamed, and covered with numerous close-set granulations, bathed in well-marked purulent serosity. The globes of the eyes were atrophied, and appeared as two little red balls, granulated like a strawberry, at the bottom of the orbits. There were no traces of cornea or iris. The infant was in a very emaciated condition.—*Annales d' Oculistique, tom.* xxxvii. *p.* 68.

On Anders, a Disease communicable from the Bovine Species to Man, and probably from one Human Being to another.—In the provinces of Limousin and Auvergne there is according to M. Lemaistre of Limoges, a disease among horned cattle, which has the name in the former province, of *endaï*, and in the latter of *anders*. The appearances in cattle differ somewhat from those in man.

In horned cattle the first sign of the disease is a pimple of the size of a grain of wheat; this discharges a reddish yellow fluid, which hardens into a crust somewhat like impetigo. This scab is easily raised, and the skin beneath is dry, tumid, and somewhat wrinkled. Round the first other pimples appear; they do not suppurate, but dry up and desquamate. The ring thus formed has a greyish white color; it goes on increasing by the formation and desquamation of fresh pimple, and destroys the hair in its course. Several rings may exist in the same animal, in various stages; they rarely exceed a five-franc piece in size. The eruption is attended with much itching; it is mostly confined to the head, face, and neck, and always to one side. Each ring of eruption passes through its stages in about a month; but a succession of them may reappear during a considerable time. The disease is generally more obstinate in winter than in summer. In recovery the skin remains bare for some time, but at last the hair grows gradually. The disease is purely local, and is popularly believed to be contagious among cattle, in support of which belief several instances are adduced. A young animal is more liable to the disease than an old one: if a cow has *anders*, the calf will inevitably be affected. Nothing satisfactory seems to be known of the treatment.

In man, M. Lemaistre has not seen the eruption in its earliest stage. When fully developed, it appears at a distance as a bright red round patch, slightly projecting above the skin, not exceeding a five-franc piece in size. On close examination it is found to consist of small red pimples, slightly desquamating on the surface; they are generally redder towards the circumference of the patch than in the centre. There is much itching in the parts affected towards evening. It is altogether local, and principally confined to the face, fore-arms, and backs of the hands. It lasts from one to several months, and always ends in recovery. In some instances the appearance of the eruption has been traceable to contact with the animals so affected: in others this evidence has been wanting, and the disease has rather appeared to have been communicated from one human being to another. In the treatment, sulphur-bath appears to have been most useful; the local application of nitrate of silver produced only a transient effect.—*L'Union Médicale,* 26 *Janvier,* 1858.

INCISION OF THE OS UTERI TO PROMOTE LABOR.—In the *Monatsichrift f. Geburtskunde,* 1857, 10, Dr. Kristeller communicates ten cases where this operation has been resorted to, and states, as his experience, that incision of the rigid os, resorted to at the proper time and place, is a remedy not superseded by ipecacuanha, opium, or chloroform, and connected with far less danger than has been stated by celebrated authors. He never saw convulsions arise from the operation, as has been alleged, nor has, in his experience, the wound been further lacerated during the progress of the labor. The rapid course which labor takes after incision is only apparently so, as generally, labor has been going on for a long time, and all the parts are fully prepared, when the operation is resorted to, by which the rigidity of the os is removed. Only once in his ten cases has the author seen any considerable hemorrhage, amounting to about twelve ounces. Suppuration and occlusion of the os after the operation have never been observed. It is indicated when, after the membranes have ruptured, a pathological state of texture or anomalous action of the vaginal portion prevent, in spite of the continuance of labor, the dilatation of the os. Of such pathological conditions may be enumerated parchment-like rigidity and firmness of the vaginal portion of the os, as occurring sometimes in primipara; a hypertrophied, fibrous or cartilaginous condition, from preceding inflammatory processes; fibroid formations, and cancerous infiltrations, and finally spasmodic contraction of the os. Before the knife is resorted to, if delay is not connected with danger, the usual other means may be used; but, as a general rule, there is more danger of resorting to the operation too *late* than too *soon.* The author prefers a curved bistoury, which is covered to about three-quarters " from its point with adhesive plaster. It is guarded by the index finger of the left hand; with two fingers of the other hand the os is pulled down somewhat; the knife is then introduced between the os and the fœtal head, the point carried in about one-half" from the margin of the os, and the knife quickly and accurately drawn inwards and downwards; 3—4 scarifications will always suffice. L.

STARVATION OF AN INFANT FROM PARALYSIS OF THE FACIAL NERVE. Professor Hohl has related, in his Report of the Lying-in Hospital at Halle, a case in which the death of a new-born child was brought about in a very unusual manner. A woman in whom labor was arrested, through complete absence of uterine contraction, was delivered by the forceps, the head lying in the pelvic cavity. In consequence of the pressure of the left blade of the forceps, there resulted a paralysis of the facial nerve of the left side, which resisted all treatment. By this the infant was hindered from suckling, since all the milk ran out of the corner of the mouth on the paralysed side. Everything, prior to being swallowed, had to be placed within the grasp of the muscles of the pharynx. The child was thus gradually reduced by atrophy, and died on the twelfth day.—*Brit. and For. Medico-Chir. Rev.*

A CASE OF INCISION OF THE LABIA MAJORA, WITH RUPTURE OF THE PERINÆUM.—In a previous report, we gave an account of Professor Scanzoni's proposed method for preventing the laceration of the perinæum in difficult labor, by making incisions in the labia majora. In a case in which this operation was resorted to by Professor Hohl in the Lying-in Hospital at Halle, although five incisions were made, the perinæum was torn notwithstanding. The incisions, says Hohl, had no other advantage than making so many wounds the more.—*Ibid.*

A CASE OF ADHERENT PLACENTA : PYÆMIA : DEATH.—A case recorded by Professor Hohl shows that in certain cases of adherent placenta, complete extraction is impossible or dangerous; it also illustrates the consequences of this accident. A patient in the Halle Lying-in Hospital, delivered of an immature fœtus, was seized immediately afterwards with violent flooding. Attempt being made to extract the placenta, it was found that about three-fourths of it adhered to the uterus. It was about half detached, when the patient suddenly moved so strongly that the hand grasping a portion of the placenta was thrown out of the uterus and vagina. As the patient would not permit another attempt and the flooding went on increasing, chloroform was administered. On examination, it was now found that the os uteri was closed by spasm, effectually preventing the passage of the hand. Nothing but small fragments, thickly studded with chalk-concretions, could be extracted. On the following day, a purulent, horribly offensive discharge began, and a strong shivering fit, followed by heat and sweat appeared. This was repeated on the second night, and again on the third. Collapse followed, with unquenchable thrist, severe headaches, dry tongue, delirium—in short, all the symptoms of pus-poisoning. On the fourth day there appeared, for the first time, pain on pressure in the left side; this increased with the exacerbation of the other symptoms. She died on the eleventh day, having first exhibited raging, subsiding into muttering delirium. Death set in with peculiar electric-like shocks.

Autopsy.—Considerable purulent exudation in the abdomen, and adhesions. In the uterus was a completely calcified piece of placenta the size of a walnut, intimately united with the uterine wall. The blood in the body was cherry read, and nowhere coagulated.—*Ibid.*

CASE OF THROMBUS OF THE LABIA—*By B. S. Woodworth, M. D., Fort Wayne, Ind.*—I was called to visit Mrs. C., aged 20, on the 22d Jan. last. She was over seven months pregnant with her first child. On returning home after a long walk, she immediately felt a *slight* pain in the groin, and an inability to stand up, and upon an examination, she discovered a large tumor, which alarmed her and her mother, and I was sent for in great haste. On examining it by the touch, I ascertained that it was not a hernia, which I very naturally pre-conceived it to be; but wishing to discover more accurately, I made an occular inspection, which revealed the true nature of it clearly.

Here was a large tumor of the left labia, of a hard feel and livid color, which contained something like a pint of blood, which I at once pronounced a thrombus. The patient had no pain at that time, nor subsequently, in the recumbent position. My plan of treatment was the following. I advised rest, and lotions of cold water, with moderate compression with a T bandage, and told the friends that I would probably open the tumor on the following day. On the next day the swelling had increased somewhat, and had extended around to the lower part of the other labia. The patient being unwilling to allow an incision to be made. I continued the treatment the two following days, when, as the swelling remained in statu quo, they consented that I should open it, which I did, making a free incision, and evacuated a large quantity of clots and some fresh blood, which soon reduced the tumor, so that in ten days thereafter, it had almost entirely disappeared. At the end of this time, the swelling again appeared, and attained to near its original size in four or five days; but instead of blood, the contents of the tumor was evidently pus. I now prescribed poultices and fomentations, and in three days the abscess opened, and continued to discharge for about one week, when it was entirely healed. No farther trouble was experienced by the patient after this, and on 13th of March, inst., she was delivered of a full grown, healthy child.

I record this case, not as anything very extraordinary, though I think that such cases are not *very* common, especially those that occur before parturition. Many authors say nothing about them, as occurring before parturition. I was somewhat surprised to find nothing in Churchill about them, except as occurring in parturition. Meigs in his lectures, also does not mention it in any other connection. Although in Meig's Velpeau, a very good description of thrombus is given, showing that they occur occasionally even in virgins.

Cazeau's midwifery, also, has a good description of it. Many surgical writers also, give a description of this affection; but most of them speak of it, as occurring only during parturition.—*Cincinnati Lancet and Observer.*

ANIMAL CHARCOAL AS AN ANTIDOTE FOR CANTHARIDES—*By M. Thouery.*—It is generally known that charcoal possesses properties which are most interesting; that it removes most of the metallic salts from water; combines with oil to such an extent that it cannot be separated by ether, and fixes certain of the vegetable principles. M. Thouery, in 1851 and 1852, made a series of experiments, from which he concluded that animal charcoal possesses real efficacy in combatting poisoning by cantharides. These experiments were 54 in number, and were performed on dogs. Lately M. Thouery has published the details of an experiment made on men.

During the night of 12th–13th of December, 1856, Antoine B. experienced very acute suffering, after having taken an infusion of centaurea from a vessel which contained powdered cantharides. Being called to see him, M. Thouery recognized it immediately as a case of poisoning by an irritant corrosive poison, but none of the liquid remaining for examination, he could not determine the nature of the poison ingested. He confined himself to the administration of general remedies, uniting, however, calcined magnesia and animal charcoal, and giving them in large doses. The condition of the patient did not seem to improve at first, but after two days of intolerable suffering, relief was obtained and health was restored.

Thouery afterwards found that the poisoning had been produced by cantharides. He does not doubt, then, that animal charcoal largely contributed to the cure; and he regards this observation as confirmative, in a certain measure, of the results of his previous experiments. The only objection which we can adduce against this theory of Thouery is, that it does not necessarily follow from the fact reported.* L. H. S.—*American Medical Monthly.*

The Abrahamic Covenant.—The sensation created in Austria by the refusal of a Jewish physician to have his son circumcised, on the score of the dangerous character of the operation, has given rise to a thorough scientific discussion of the subject in a Viennese medical paper, entitled *Austrian Gazette of Practical Medicine*, in which the following results are arrived at:—1. That circumcision, as a medical operation, presents no kind of danger whatever; that if it should be followed by any untoward consequence, it is owing to the unskillfulness of the operator. 2. That the mortality amongst Jewish children is not greater than that amongst the Gentile children of the same class. 3. That circumcision exercises no unfavorable influence whatever upon the Jewish constitution. 4. That it protects from various physical evils, which it either altogether averts or greatly mitigates.—*Abridged from Lien D'Israel.*

How to keep Rooms cool in Summer.—Lord Rosse has denied the absurd prediction, that the approaching summer will be an extraordinarily hot one. Still, it may be well that medical men should be forearmed with the means of cooling their own and their patients' rooms. A flat vessel filled with water, and on which are floated branches of trees covered with green leaves, is a very pleasant and efficacious means, and is much employed in Germany. The suspension of Indian matting, previously damped, at the open window, tends much to diminish the heat. This matting may be imitated by any kind of plaited grass.—*Ibid.*

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* We have translated this notice from the French, not because we consider the claims of charcoal as an antidote for cantharides to be established as a certainty, but with the view of calling attention to it, and obtaining from our own countrymen some additional facts bearing upon the subject of cantharides as a poison.

Puerperal Convulsions.—Dr. James M. Newman presented a report on this subject to the Buffalo Medical Association, at a late meeting, which is published in full in the Buffalo Medical Journal, comprising some account of 33 cases collected from various sources. Of these, 17 were primiparæ, 9 multiparæ, 7 not stated. Recovered, 24; died, 9. Described as anasarcous, 7. The urine was albuminous in 12 cases; not albuminous in 2; dark colored in 2; no secretion of urine in 2; and condition not noted in 19. Ether or chloroform was employed after bloodletting in 19 cases—and of these, 16 recovered and 3 died; employed without bloodletting, 9—and of these, 6 recovered and 3 died. Convulsive movements modified and controlled by anæsthetics, in 23 cases; convulsions not diminished by them in 6 cases. Two cases proved fatal in which chloroform was administered without previous treatment being indicated.

Committees of the American Medical Association.—At the late meeting in Washington, the following committees for the ensuing year were chosen:

Special Committee on the Microscope.—Drs. Holsten, of Ohio, Dalton, of New York, Hutchinson, of Indiana, Stout, of California, and Ellis, of Massachusetts.

Special Committee on Medical Jurisprudence.—Drs. Smith, of New York, Hamilton, of Buffalo, Crosby, of New Hampshire, Purple, of New York, and Mulford, of New Jersey.

Committee on Quarantine.—Drs. Harris, of New York, Moriarty, of Massachusetts, La Roche, of Pennsylvania, Wragg, of South Carolina, and Fenner, of St. Louis.

Committee on Surgical Pathology.—Dr. James R. Wood, of New York, chairman.

Committee on Diseases and Mortality of Boarding Schools.—Dr. C. P. Mallengly, of Kentucky, chairman.

Committee on the various Surgical Operations for the relief of Defective Vision.—Dr. Montrose A. Pallen, of St. Louis, chairman.

Committee on Milk Sickness.—Dr. Edward A. Murphy, of St. Louis, chairman.

Committee on Medical Ethics.—Drs. John Watson, of New York, Dalton, of Massachusetts, Emerson, of Pennsylvania, Hamilton, of New York, and Gaillard, of South Carolina.

SOME TOPICAL APPLICATIONS EMPLOYED AT THE HOSPITAL SAINT-LOUIS, PARIS, FOR ECZEMATOUS AND IMPETIGENOUS ERUPTIONS.

Dermatology has, perhaps, longer than any other branch of practical medicine, felt the effects of the doctrine of Broussais. Imbued with the opinions of this reformer, physicians have for a long time persisted in combating, by local antiphlogistic treatment, the eczematous and impetigenous eruptions, which constitute the greater part, and, as it were, the type, of skin diseases. So long a perseverance in an exclusive and eminently unsuccessful line of treatment, is doubtless naturally to be explained by the really inflammatory character which these affections exhibit during a certain phase of their course; but, if it be true that emollients are useful in this early period, their inefficacy is soon perceived after the disease has been some little time established. Hence the return to astringent and escharotic applications, much abused by former practitioners, and especially by empirics, but which have been too much neglected during a long series of years.

M. Gibert has, more than any other dermatologist, aided in restoring this mode of treatment, and in regulating and methodizing its employment. Among the first of the astringent remedies, so useful in the treatment of eruptions on the skin, he places the resinous and empyreumatic substances, which were so much employed by the ancients. Purified tar, combined with lard, in the proportion of from one to three parts to thirty of the excipient, is in daily use in the wards of Saint-Louis, as the best resolvent for scaly eruptions, and a valuable desiccative in chronic eczematous and impetigenous eruptions. Since the introduction of glycerin into therapeutics, he employs this substance as excipient, in preference to lard. To facilitate its use, M. Gibert applies the mixture, thickened with starch, in the form of a pomade, according to the method of M. Garot, and this has the advantage over the ordinary pomades, made with greasy excipients, that it can be easily washed off. The formula ordinarily employed in the wards is as follows: Glycerin, one fluid ounce; purified tar, half a fluid drachm; warm the mixture, and add enough powdered starch to make a homogeneous paste, of moderate consistence.

This preparation relieves itching, heals excoriations, dries up secretions and dispels redness. Hence, under its influence eczema rubrum, impetigo, intertrigo, prurigo of the scrotum and of the anus, acne rosacea, and sub-inflammatory mentagra, are modified in the most favorable manner.

M. Gibert also frequently employs another resinous product, well known within a few years past, the *oil of cade.* The empyreumatic properties which this resinous oil possesses, in a much greater degree than tar, are such that it can rarely be employed pure. M. Gibert commonly mixes it with the oil of sweet almonds, or with cod-liver oil. A mixture is employed in his wards under the name of *huile cadée,* composed of two parts of cod liver oil and one of cade oil, which possesses, according to this skilful practitioner, very efficacious resolvent and siccative properties. Under the use of this application, he has seen cases of eczema cured, in which the excoriated and secretory patches had remained stationary for many months, notwithstanding the employment of sulphur, both externally and internally.

In the obstinate pruriginous, papular and eczematous eruptions of the anus and genitals, which are so often the source of despair both to the patient and the physician, M. Gibert has especial reason to congratulate himself on the employment of the oil of cade. In such cases, he combines with it the assiduous use of cold hip baths, and, by way of modifying the diathesis upon which the eruption depends, the internal administration of the arsenical solution of Dr. Boudin, modified as follows: distilled water, Oiss.; arsenious acid, gr. i. Make a hot solution, to be divided into six phials; half a phial to be taken every morning, fasting, in a glass of chicory-water sweetened with honey. Under this treatment, M. Gibert states that he has seen eruptions cured in a few weeks which had lasted for several years, and which had resisted mineral waters and many other kinds of medication.—*Journal de Chimie Médicale.—Trans. for the Bost. Med. and Surg. Jour.*

REMARKS ON THE MEDICINAL HYPOPHOSPHITES.*—*By William Proctor, Jr.*—[In our last number, we presented to our readers, a striking article from Dr. Churchill, on the use of the Hypophosphites, in the treatment of tubeculosis. We here transfer to our pages a paper in which these several salts and their mode of preparation are described.—EDTS. SOUTHERN MED. AND SURG. JOUR.]

The recent researches of Dr. Churchill into the therapeutic character and value of the hypophosphites in tuberculosis, some account of which will be found at page 143 of this number, have attracted much attention from physicians, and many inquiries have been made after these salts; and it is believed that a notice of the processes for preparing them, and some formulæ for their prescription, will be acceptable to the readers of the Journal, especially, as from their hitherto unimportant position among pharmaceutical chemicals, no mention is made of them in works most accessible. The salts which have been used are those of lime, soda, potassa and ammonia. In the sequel a notice is given of these, of the hypophosphite of sesquioxide of iron, and of hypophosphorus acid.

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* The Hypophosphites are manufactured in quantity by Hennell Stevens, of Philadelphia, who is successfully directing his attention to the supply of new chemicals for medical use, and fine chemicals in general.

The hypophosphites, according to Gmelin, are mostly crystallizable. They cannot exist without a certain proportion of water, which is equally true of the acid itself, which in its most concentrated form contains three equivalents of water, one of which is replacable by bases. When heated till decomposed, these salts emit phosphuretted hydrogen. They are permanent in the air as regards oxidation; but when heated in solution, especially if free alkali is present, they are decomposed into phosphates and hydrogen gas. They are nearly all soluble in water, and several of them in alcohol, and readily reduce the soluble salts of silver and gold.

Hypophosphite of Lime is the most important of these salts, as it not only, by oxidation in the economy, will afford phosphate of lime in a nascent state, if needed, but its reaction with the carbonates of the alkalies give a ready means of obtaining the alkaline hypophosphites. When phosphorus is boiled with milk of lime it gradually disappears, with evolution of spontaneously inflammable phosphuretted hydrogen, which explodes as it reaches the atmosphere with the formation of water and phosphoric acid. When the strong odor of phosphuretted hydrogen ceases to be given off, the liquid containes, besides the excess of lime, nearly half of the phosphorus as phosphate of lime, and the remainder, deducting the considerable portion which has escaped into the air as phosphuretted hydrogen, as hypophosphite of lime. According to Wurtz, more than one equivalent of water is decomposed, and the phosphuretted hydrogen is accompanied by free hydrogen. If this be true, the source of the super oxidation of so much of the phosphorus is traceable to the resulting oxygen; but Rose is of the opinion that this oxygen is derived from the atmospheric air in contact with the boiling liquid. When the process is conducted in a flask, it requires a constant ebullition of the liquid to prevent the explosion consequent upon the entrance of the atmospheric air. To avoid this result, it has been found safer to employ a deep, open vessel.

The constant evolution of gas and vapor, which keeps a froth on the surface, excludes the atmosphere in a great degree, so that the yield is not much diminished, whilst the safety and easiness of the process is greatly increased. The process should be conducted under a hood with a strong draught, or in the open air, to avoid the disagreeable fumes which are evolved.

Take of Lime recently burned	4 lbs. av.
Phosphorus	1 lb. "
Water	5 gals.

Slake the lime with a gallon of water, put the remainder in a deep boiler, and as soon as it boils add the slacked lime, and mix to a uniform milk. The phosphorus is now added, and the boiling is kept up constantly, adding hot water from time to time, so as to preserve the measure as nearly as may be, until it is all oxidized and combined, and the strong odor of the gas has disappeared. The mixture froths much, and but little of the phosphorous reaches the surface. Then filter the solution through close muslin, wash out that portion retained by the calcareous residue with water, and evaporate the filtrate till reduced to six pints. The concentrated liquor should now be re-filtered to remove a portion of carbonate of lime which has resulted from the action of the air on the lime in solution, and again evaporated till a pellicle forms, when it may be crystalized by standing in the drying room, or the heat may be continued with stirring till the salt granulates, when it should be introduced into bottles.

Hypophosphite of lime is a white salt with a pearly margarin-like lustre, and crystalizes in flattened prisms. Its composition, according to Wurtz, is $CaO, + 2HO, PO$, the water being essential to the salt. It is soluble in six parts of cold water, and not in much less of boiling water; it is soluble slightly in diluted alcohol, but insoluble in alcohol sp. gr. .835.

Hypophosphate of Soda is prepared by double decomposition between hypophosphite of lime and crystalized carbonate of soda.

Take of Hypophosphite of lime	6 oz.
Crystalized carbonate of soda	10 oz.
Water, a sufficient quantity.	

Dissolve the hypophosphite in four pints of water, and the carbonate in a pint and a half, mix the solutions, pour the mixture on a filter, and lixivate the precipitate of carbonate of lime, after draining, with water, till the filtrate measure six pints. Evaporate this liquid carefully till a pellicle forms, and then stir constantly, continuing the heat till it granulates. In this state the salt is pure enough for medical use; but if desired in crystals, treat the granulated salt with alcohol sp. gr. .835, evaporate the solution till syrupy, and set it by in a warm place to crystalize.

Hypophosphite of soda crystalizes in rectangular tables with a pearly lustre, is quite soluble in water and in ordinary alcohol, and deliquesces when exposed to the air. Its composition is $NO+2HO,PO$.

Hypophosphite of Potassa is prepared by the same process as that given above for the soda salt, substituting $5\frac{3}{4}$ ounces of granulated *carbonate of potassa,* in place of 10 ounces of crystalized carbonate of soda, and using half a pint instead of a pint and a half of water to dissolve it.

Hypophosphite of potassa is a white, opaque, deliquescent salt, very soluble in water and alcohol. Its greater tendency to absorb moisture renders it less eligible for prescription than the soda salt. Its composition is $KO+2HO,PO$.

Hypophosphite of Ammonia is prepared from hypophosphite of lime and sulphate or corbonate of ammonia.

Take of Hypophosphite of lime	6 oz.
Sesqui-carbonate of ammonia (translucent)	7.23 oz.
Water, a sufficient quantity.	

Dissolve the lime salt in four pints of water, and the ammonia salt in two pints of water, mix the solutions, drain the resulting carbonate of lime, and wash out the retained solution with water. The filtrate should then be evaporated carefully to dryness, then dissolved in alcohol, filtered, evaporated and crystalized.

This salt is deliquescent in the air, very soluble in alcohol and water, and when carefully heated evolves ammonia, and leaves hydrated hypophosphorus acid. The composition of this salt is NH^3+2HO,PO.

Hypophosphite of Sesquioxide of Iron.—This salt may be obtained in the form of a white gelatinous hydrate, by precipitating a solution of hypophosphite of soda or ammonia with one of sesquisulphate of iron. The precipitate should be well washed with water and dried at a moderate temperature. It is necessary to avoid using a hypophosphite containing any alkaline carbonate or the precipitate will be contaminated with free sesquioxide. Thus prepared, this salt is a white, amorphous, tasteless powder, like the pyrophosphate, soluble in hydrochloric acid, and in free hypophosphorus acid.

Hypophosphorus Acid.—So far as we are aware, this acid has not been employed in a free state by Dr. Churchill, but it is highly probable that it may come into use, should the favorable results claimed for its salts be substantiated by new observations. Any claims which phosphoric acid may possess as an agent to supply the waste of phosphorus and phosphates in the human economy, will be more than equalled by this acid. Hypophosphate of baryta is the salt which is most eligible for the preparation of this acid, but it is more convenient to prepare it from the lime salts, viz:

Take of Hypophosphite of lime	480 grains.
Crystalized Oxalic acid	350 grains.
Distilled water	9 fluid oz.

Dissolve the hypophosphite of lime in six ounces of the water and the acid in the remainder, with the aid of heat, mix the solutions, pour the mixture on a white paper filter, and when the liquid has passed add distilled water carefully, till it measures ten fluid ounces, and evaporate this to eight and a half fluid ounces.

The solution thus prepared contains about ten per cent. of terhydrated hypophosphorus acid ($HO+2HO,PO$), a teaspoonful representing six grains of the acid, which contains two and a quarter grains of phosphorus. The dose of this acid solution will probably vary from ten minims to a teaspoonful.

It is proposed to give several forms in which the hypophosphites may be conveniently administered, and a few hints to the physician in reference to prescribing them.

The soluble salts of mercury and silver are reduced by contact with the hypophosphites. All soluble sulphates and carbonates are incompatible with the lime salt, and should not be associated with it in prescriptions, if phosphate of lime is indicated in the case. The iron salt when dissolved by excess of acid is colored black by gallotannic acid and drugs containing it, but is not blackened by the tannin of cinchona, catechu and krameria; hence, any preparation containing it may be associated with Peruvian bark. The hypophosphites of soda, potassa and ammonia, are more or less deliquescent, and when prescribed in powder it should be with proper precautions to avoid moisture, as by association with a considerable excess of sugar of milk. The lime salt may be mixed with either this sugar or ordinary sugar. None of these salts are soluble in cod-liver oil; and if given with it, they should be dissolved in syrup, and mixed by agitation. Where lactin and glycerin are indicated in the treatment of phthisis or dyspepsia, and of these salts may be very elegantly associated in the form of syrup.

Syrup of Hypophosphite of Lime.

Take of Hypophosphite of lime, an ounce.
Water, nine and a half fluid ounces.
White sugar, twelve ounces.
Fluid extract of vanilla, half a fluid ounce.

Dissolve the salts in the water, filter, add the sugar, dissolve by aid of heat and add the vanilla. The dose is from a teaspoonful (three and a half grs.) to a tablespoonful (fourteen) according to the circumstances of the case, three times a day.

Compound Syrup of Hypophosphites.—The following formula has been made in view of the double purpose to which these salts are directed by Dr. Churchill, viz: the increase of nerve force, and the elevation of the tone of the several functions concerned in alimentation and nutrition; and will afford an agreeable means of testing practically their merit. The iron salt is presented in a form well adapted for entering the circulation, whilst the acid, besides exerting its solvent power, adds to the agreeable taste of the preparation.

Take of Hypophosphite of lime	256	grains.
Hypophosphite of soda	192	“
Hypophosphite of potassa	128	“
Hypophosphite of iron[*] (recently precipitated)	96	“
Hypophosphorus acid solution	q. s. or 240	“
White sugar	12	ounces.
Extract of vanilla	½	ounce.
Water, a sufficient quantity.		

Dissolve the salts of lime, soda and potassa in six ounces of water; put the iron salt in a mortar, and gradually add solution of hypophosphorus acid till it is dissolved; to this add the solution of the other salts, after it has been rendered slightly acidulous with the same acid, and then water till the whole measures nine fluid ounces. Dissolve in this the sugar, with heat, and flavor with the vanilla. Without flavoring, this syrup is not unpleasant, being slightly saline, and not at all furruginous. Any other flavoring may be used, as orange peel, orange flower or ginger. It is also suggested to physicians that glycerine may be used, wholly or partially, in sugar when indicated, six ounces and a half of glycerine being substituted for twelve ounces of sugar. The object of acidulating the saline solution is to decompose any alkaline carbonates which may be present, and which have been noticed by the writer in some of the commercial soda salt. The dose of this preparation is a teaspoonful three or four times a day. A teaspoonful contains two grains of the lime salt, one and a half of the soda salt, one of the potassa salt, and three quarters of a grain of the iron salt, besides a little hypophosphorus acid.—*American Jour. of Pharmacy.*

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[*] This quantity, 96 grains, of hypophosphite of iron is obtained when 128 grains of hypophosphite of soda dissolved in two ounces of water is decomposed with a slight excess of solution of persulphate of iron, and the white precipitate will wash on a filter with water.

ON LEAD COLIC—*By M. Briquet.*—A very interesting communication has been made to the Académie de Médicine, by M. Briquet, physician of the Charité Hospital. The object of the paper is the nature and treatment of lead colic. What is the seat of the violent pain which then exists? Is it the digestive canal—the muscles of the abdomen—the diaphragm? Is the spinal cord affected, as Laennec and Barbier admitted? Is the pain purely neuralgic, as maintained by Andral and Grisolle? M. Briquet affirms, from researches made on forty-four patients, that the pain is in the muscles of the abdomen, sometimes in one, sometimes in many. He says also, that there is in some cases a real hyperæsthesia of the skin in the neighborhood of the painful muscles. In some patients, however, the reverse exists: the skin is anæsthetic. He affirms that constipation has no influence whatever on the abdominal pain. He proposes in another paper to relate cases to prove that the application of galvanism to the painful muscles usually takes away the pain at once. We can assert that it is perfectly true that the pain may disappear immediately in cases of lead colic, after one application of an electro-magnetic current.—*Virginia Med. Journal.*

PRURITUS VULVÆ.—According to Dr. Hartmann (*Medic Neuigk.*), a very severe case of pruritis vulvæ was successfully treated by him with a strong decoction of white hellebore (the root) applied by compresses. He recommends the remedy highly in similar cases.

MORTALITY AND HEALTH OF THE CITY.

The number of deaths reported by the Board of Health from the 23d of May to the 27th of June amount to 676; being a weekly average of 135. This is more favorable than our last monthly statement, in which the weekly average was 140. The largest mortality was from Consumption and Bowel Complaints, the latter chiefly amongst children. The general health of the city has been very good.

Yellow Fever.—The city was quite startled at the beginning of last week by the occurrence of five or six cases of yellow fever. On Sunday, the 20th of June, a young Irish girl died with black vomit at the *Maison de Santé* (Stone's Hospital). She had been in the city since January, last and was employed as a servant in a Creole family residing on Barrack street, a considerable distance from the river. She is reported to have stayed close at home and been exposed to no sort of foreign influence whatever. A number of physicians saw the case and no doubt was expressed about the nature of the disease. She was taken sick on the 16th, and died the fourth day.

On the 16th a young man was admitted into the Charity Hospital, Ward 3. He had been about New Orleans irregularly for several years, but never had yellow fever. Was employed on a tow-boat plying between the City and the Balize when he was attacked. Some doubt has been expressed about the nature of this case, but we think the majority pronounce it yellow fever.

The young man still lives, after having hæmorrhage from the mouth for many days, and becoming deeply jaundiced. This case was followed by four others, admitted into the Charity Hospital, at different times during the last week, all coming from a vessel, the *Rosalie*, which came from Sicily, loaded with fruit. She arrived here on the 6th of June, all well, and having had no sickness on the voyage. She had touched at no port after leaving Marseilles. About fifteen days after getting here, the crew began to sicken with yellow fever. Four of them have entered the Hospital, and all have died save one boy, whose state is extremely precarious.

We have heard vague rumors of a few other cases in different parts of the City, but we attach no importance to them. Quite a panic has been produced by the occurrence of the above cases, but no new case has been admitted in any of our hospitals since the 26th of June, and we know of none at any other place. As before stated, the City is remarkably healthy at this time, July 1st, and there is not the least danger in persons visiting or passing through it.

NEW ORLEANS

MEDICAL NEWS AND HOSPITAL GAZETTE.

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MEMOIR ON SOME POINTS OF THE NORMAL AND PATHOLOGICAL ANATOMY OF THE MUCOUS MEMBRANE OF THE UTERUS.

By Doctor Charles Robin, Professor Agregé of the Faculty of Paris, Member of the Academy of Medicine, etc.

Written expressly for the New Orleans Medical News and Hospital Gazette, and translated by Anthony Peniston, M. D., Professor of Physiology, New Orleans School of Medicine.

PART SECOND.—*Continued from July No.*

Section 2. Description of the Uterine Surface of the Placenta, when detached.

The cotyledons which are found on the uterine surface of the placenta constitute a series of irregular projections with intervening furrows of various depth. But I do not know that it has ever been observed that, when the placenta is artificially separated, in case of a woman dying before delivery, as well as in natural delivery, the surface of the cotyledons is covered with a greyish, semi-transparent membrane, of a soft texture, and varying from ½ to 2 millimetres in thickness, according to the subjects.

This membrane is sometimes smooth, and at other times presents a peculiar uneven appearance. It does not contain within its substance any vessels similar to the circular sinus which is sometimes found at the circumference of the placenta, nor are they similar to the sinuses of the serotina. I have elsewhere established the important fact that this greyish somewhat elastic membrane, and of a sticky, glutinous nature, passes continuously from one cotyledon to another, being somewhat thicker in the interstices into which it dips (See Researches on the gradual modifications of the villosities of the chorion and placenta in Memoirs of the society of Biology, 1854, p. 75.)

This coat or membrane detached from the serotina to which it adheres is derived from the inter-utero-placental epithelium of the mucous membrane of the uterus, and from the most superficial part of the decidua serotina. It is therefore principally made up of epithelial cells which have been considerably hypertrophied, both as to their nuclei and their substance. It also contains some mis shaped cells, and various curious modifications of these cells, which I shall describe more particularly in another portion of this memoir. It contains besides some amorphous matter and different molecular granulations. It can also be seen that the border of the placenta, as it approaches the decidua, is continuous with that portion of the latter which is adherent to the chorion, rather than with the surface of the decidua which has just been detached from the uterus. As this membrane is constantly met with, except when accidentally torn, its existence demonstrates the following propositions:

1st. That the placental villosities do not plunge immediately into the venous sinuses of the serotina.

2d. The cotyledons project towards the decidua inter-utero-placental, which on its own part enters deeply into the furrows which separate the cotyledons; but the substance of the latter, and the ramifications of the villosities which are traversed by the fœtal blood, are separated from the maternal blood by this greyish membrane on the one hand, and on the other by an exceedingly thin membrane which covers the sinuses of the serotina.

3d. Therefore it is that the exchange of nutritive materials between the mother and the fœtus takes place through the thickness of this membrane, and through the coats of the capillary vessels of which the villosities are constituted. We thus see that the transmission is not so immediate as is generally supposed; so that the contact between the chorial and placental villosities is intermediate, and not immediate.

4th. Its adherence to the cotyledons is of the most intimate molecular nature, for we see that the placenta detaches the surface of the serotina, and carries it away, instead of becoming detached from it. Thus it is, that physiologically and anatomically speaking, the cotyledons, and in other words the placenta, are applied superficially against the large maternal vessels, and do not bathe immediately in the blood itself, in the arborescent form which is commonly ascribed to them; the latter being nothing more than a mere hypothesis to explain the endosmotic action and are not the result of observation.

It is consequently erroneous to say that the blood, in consequence of this hypothetical anatomical arrangement, is interposed or in some sort infiltrated between the ramifications of the placental villosities, as far as the fœtal surface of the placenta, and in contact with the chorion, where it could be seen transparently through the latter and the amnios. For, if as I have done, we take a placenta which is yet adherent to the uterus, and an incision be made through the entire thickness of these two membranes down to the surface of the cotyledons, no blood will be found in the interstices between the villosities, unless some of the latter happen to be cut, and then they will give out fœtal and not maternal blood. It is true that when we examine the placenta transparently through the chorion, and amnios, if these membranes happen to be intact, then the tissue of the latter presents a uniformly red color which may lead us to suppose that the blood is immediately in contact with the chorion; and this may appear confirmed by the fact that pressure renders them paler than before. But the former appearance is due to the fact that the villosities, the ultimate ramifications, of which are scarcely visible to the naked eye, are filled with fœtal blood, and therefore have the appearance of an uniformly red coat or substance when seen through the amnios and chorion. As to the paler shade, which the mass assumes when the finger is lightly pressed on the fœtal or amniotic surface of the placenta, it is explained by the fact that slight pressure will drive the blood into the adjacent villosities. Moreover, a successful minute injection into the uterine sinuses does not penetrate into the substance of the placenta, except in case of rupture, which is easily seen, nor does the injecting matter come in contact with the chorion, or infiltrate the placenta.

Section 3. Nature of the connections between the Placenta and Uterus in some Mammiferous Animals.

I must here observe that the indirect and intermediate connections of the placental villosities with the maternal vessels are not at all peculiar to man for it is not true that the villosities of the placental cotyledons or of the chorion in mammiferous animals such as ruminants, carnivora and others, bury their extreme subdivisions in the tubular glands of the uterine mucous membrane, or its dilated vessels. Among rabbits, especially at their first development, the villosities enter deeply between the folds of the uterine mucous membrance which is extremely vascular at this point, but there is nothing more than an intercrossing or interlacement of these folds with the ramifications of the villosities, or even with the ramuscular fasciculi; so that at last there is nothing more than contiguity between the ramifications of the villosities on the one hand, and the folds of the highly vascular mucous membrane on the other. This simple contiguity or interposition becomes much more manifest at a later period, because it then becomes evidently intermediate. When we examine the uterus of a rabbit at the beginning of gestation we find bilobar placentas corresponding to each ovum, but these placentas lie immediately on a wide projection resembling the papular elevations in syphilis. When these placentas are detached, the surface of these papular elevations appears slightly depressed in the centre as if ulcerated, owing to the projecting margin which rises circularly above the rest of the papular elevation. These papular elevations are the maternal placentas; their section has a homogeneous carnified appearance of a greyish red color, it is principally composed of cellular tissue like the surrounding mucous membrane, but it has besides a considerable quantity of fusiform fibroplastic elements, and especially a homogeneous amorphous matter with fine granulations between the fibres. The vessels which are larger than in other parts of the mucous membrane have not however the appearance of sinuses. I have not found in this tissue, the thickness of which, is mainly owing to this amorphous matter and to these fibro-plastic elements, any follicular glands similar to those of the mucous membrane. The surface of these elevations, or maternal placentas is puckered and presents a considerable hypertrophy of the conical vascular villosities which are found over all the mucous membrane. These folds and villosities are interlaced with the villosities of the fœtal placenta, but by simple juxtaposition without any penetration of the one into the other.

The folds of the maternal placentas are covered with a greyish coat in which are found some few fragments of villosities from the fœtal placenta which have been torn in separating the latter from the uterus. We also find large epithelial cells with numerous nuclei, forming the sheaths of the villosities found in the hypertrophied mucous membrane. The greyish coat is principally composed of these enlarged cells with numerous nuclei. (1) These cells which I propose to describe more fully hereafter are similar to those which I have spoken of before as belonging to

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(1.) It is now established beyond a doubt, that the internal membrane of the uterus in animals is never exfoliated, and that it is only in women that the decidua is really detached.—*Raciborski on the Physiological and Pathological Exfoliation of the internal membrane of the uterus.* Paris. 1857. p. 32.

woman also. In the rabbit this greyish coat has an opaque dipstieritic, semi-membranous appearance, as if gangrenous, being soft and friable ; it is interposed between the *fœtal placenta,* and the thick popular elevations which we have called the *maternal placenta.* This soft, friable, epithelial coat is the only portion of the uterine mucous membrane which is really deciduous in the rabbit, and as we see it is only composed of epithelial covering interposed between the real uterine mucous membrane and the fœtal placenta; which interposed membrane gets gradually thicker as gestation advances : It corresponds to the epithelial coat of the decidua inter-utero-placental in woman. This epithelial covering in animals is sufficiently thick towards the end of gestation to constitute a sort of cushion or greyish coating, which can be easily detached from the real mucous-membrane beneath. It is carried away during delivery by the fœtal placenta to which it is more adherent from being interlaced with the folds or filaments on the surface ; nevertheless it can be easily separated from the latter. This greyish, friable, non-vascular coat, is composed of the epithelial cells above mentioned ; but at the beginning of gestation it is composed of a single row of cells, and acquires the appearance above described as it gradually gets thicker. This membrane is intermediate in all the exchanges of blood between the fœtus and the mother, for it is through it that the materials, which are derived from the vascular portion of the uterine mucous membrane, penetrate by endosmosis, molecule after molecule, until they reach the placental villosities, and on the other hand the effete products of the fœtus are transmitted outwards in the same manner by exosmotic action.

This membrane, on account of its friability, and slight adherence, renders the expulsion of the placenta exceedingly easy. It is probable that the expulsion of the fœtus takes place when the transmission of materials from the mother to the fœtus is either entirely suspended or becomes insufficient on account of the thickness of this membrane.

In the dog we find a greyish coat, similar to the one above described in the rabbit, but it is much more adherent to the mucous membrane, and a portion of it remains attached to the latter several days after parturition.

In the sow the mucous membrane of the uterus is loose, and movable over the subjacent coat ; but in pregnancy it becomes thicker and looser than before fecundation. It then becomes conjested over its whole extent and presents at different intervals, various carunculi or fleshy projections, which are formd by a duplicature of the mucous membrane, and which are generally in an œdematous condition, having the appearance of small cysts. The whole surface is abundantly lubrified with mucus, in order that the fœtal envelopes may glide over each other. The different fœtal sacs are not adherent by any point of their surface to the mucous membrane of the uterus, simply resting upon it. It results from this disposition, that at any epoch during gestation, the different fœtal sacs can be withdrawn one after another with the greatest facility; for a simple pression from above downwards on a level with the constricted portions which separate the sacs would produce this result. During parturition the fœtal envelopes simply glide over the mucous membrane of the uterus, and nothing is really detached from the latter; therefore, there is no rupture of any membrane whatsoever. The vessels of the mucous membrane are larger at the point of contact with the egg than elsewhere, but they cannot be said to be real sinuses. The papilla of the mucous membrane are also larger than in other places.

With these slight exceptions the mucous membrane is entirely in the same condition in the point corresponding to the attachment of the ova as in other portions. The soft, greyish, pulpy coat which remains on the surface of the allantois, is analogous to that of the rabbit, but much thinner, and composed of larger epithelial cells, than in other portions of the uterus, with a large nucleus and a nucleolus within the latter. These cells are sometimes isolated, sometimes united together in layers or sheaths retaining the shape of the vascular papilla of which they form the covering.

We also find some sheaths of nucleolar epithelium from the tubular glands or mucous follicles, which are covered by this peculiar variety of epithelium. In the sow, the allantois, into which ramify the branches of the umbilical artery of the fœtus, and which afterwards becomes the placenta, is formed as in the human species; the only difference being that the tissue is much paler and more transparent; the nuclei are wider apart, because the cells which have become blended together to form them are larger; but the nuclei are somewhat smaller and paler. In the cow this anatomical arrangement is exactly similar.

The allantois of the sow is loaded with small papilliform elevations, which are wide, flattened and hollow; traversed by a single flexuous capillary of which the arterial and venous branches are widely separated from each other. The capillaries themselves, in the allantois have exactly the same structure as these vessels in adult life. In the sow, the allantois has generally a coating of epithelial cells, prismatic in shape, regular and finely granular, having each a little nucleus, either spherical or avoid, but without a nucleolus. In different places, on the allantois, soft, pulpy elevations may be seen, giving a sort of marbled appearance to the surface of this membrane, they are formed by an accumulation of prismatic cells similar to the preceding with other spherical cells having large nuclei, similar to those which line the internal surface of the uterus. This soft, pulpy, epithelial coat represents in the sow the greyish, friable, inter utero-placental coat, which we have described in the rabbit during gestation, and through it also, the exchange of nutritive materials between the fœtus and the mother, take place by endosmotic action as above described. Here also the relations between the fœtus and the mother, are those of contiguity and not continuity; the exchange of liquids between these two beings, takes place through the thickness of this tissue, by mediate action, and not by direct action.

In the cow, the chorion and its villosities are formed of cells which are in juxtaposition, but which may yet be easily separated from each other, until the fœtus is about 18 centimetres in length. In the chorion itself, they are applied against the layer of laminar tissue which is interposed between the chorion and the amnios. On the villosities they are directly applied over the large capillaries of which they are composed. These cells are easily detached from the capillaries, either retaining their sheath-like form from their juxtaposition, or floating separate from each other. These cells measure from 15 to 25 thousandths of a millimetre in width; they are polyhedral in shape, with rounded angles from mutual compression. Their lines of demarcation are pale, greyish and regular when isolated; many are quite spherical in shape. Their outlines are regular, of a greyish color, and finely granular. They contain one or two spherical nuclei, measuring from 8 to 10 thousandths of a millimetre, with no nucleoli, but some large, dark granulations. Together with these isolated cells, in the field of the microscope, we also find some free, detached nuclei, similar to the above. The capillaries which are found in the villosities, or, rather, which constitute, with their epithelial covering, the whole substance of the villosities, are of the first variety—that is to say, they have a single coat, as seen in the rounded terminations in the placental villosities of the cow. The coat of these capillary vessels is homogeneous and transparent, interspersed with long ovoid nuclei, of a pale color. The terminal villosities of the placental cotyledons of the fœtal calf, which are intervolved, as above described, with the villosities of the uterine cotyledons of the mother, are therefore in simple relation of contiguity and juxtaposition with the latter, which are highly vascular; but there are really no sinuses or large accumulations of blood. These are peculiar to the human species, and, perhaps, to some varieties of the monkey tribe, in whom the uterus is rigid, thick and muscular. So that, after all, we see that the vascular organs intended to nourish the fœtus, are simply applied against other vascular organs, on the part of the mother, though the exact mode of application or adherence may vary according to the arrangement of the mucous membrane of the uterus, the existence or absence of maternal cotyledons; but there is certainly no penetration or communication of vessels, as is sometimes asserted.

There are no other utero-placental vessels than those which belong to the folds or projections of the uterine mucous membrane, and which dip into the villosities of the chorion, or of the placenta, in mammiferous animals, and which, in the human species, extend into the shallow grooves which separate the placental cotyledons.

Section 4. State of the Uterus at the Insertion of the Placenta, after this Organ has been Detached.

If we next examine the state of the uterus on a level with the insertion of the placenta, we find that it still retains the serotina, less the thin superficial coat which has been detached from it and carried away by the placenta; so that the serotina, which is still supplied with numerous vessels, so distended as to have the appearance of sinuses, has not really become deciduous, at least not immediately. This can be explained by the fact that its vessels are continuous with the muscular tissue of the uterus, and nutrition is kept up by means of this vascularity. In dissecting these organs, this peculiarity appears the more extraordinary when we see how exceedingly thin is this membrane, in comparison with the large sinuses or blood-vessels between which it is interposed as the only separation and protective covering. This tissue is of a red color, somewhat darker than the real decidua. And lastly, an important fact is that between the serotina and the muscular tissue of the uterus there is no new membrane similar to the thin coat which replaces the exfoliated mucous membrane in other portions of the uterus to which belongs the decidua vera. The decidua serotina is only gradually detached and exfoliated during the lochial discharge. It is then replaced by a gradual closing in of the thin new mucous membrane which takes the place of the decidua vera. The serotina has therefore a very different appearance when examined in the uterus of a woman dying between the seventh and ninth month of pregnancy, where the placenta is not yet detached, from what it would present between the second and tenth day after delivery. In the former case, the serotina is thin and just as wide as the placenta, its sinuses are wide and flat, except when distended by artificial injection; the smaller ones however, are somewhat more cylindrical. Its surface is not perfectly smooth, and it afterwards becomes still more rough. After delivery, the contraction of the uterine walls greatly diminishes the extent of the serotina, so that it is soon reduced to six or eight centimetres in width, and continues gradually getting smaller. Its shape, which was at first circular, becomes afterwards irregularly oval with the long diameter following that of the uterus, and its edges become serrated and torn; so that this membrane gains in thickness what it loses in width, on account of the contraction of the uterus upon itself. At the same time, the surface becomes plaited, rough and mammilated; its tissue becomes darker, and it gradually softens, taking a mucous pulpy consistency. On a woman dying a few days after delivery, the remains of the decidua inter-utero-placental or decidua serotina have from 15 to 18 milimetres in thickness, or even more. The irregular projecting edges of this coat are applied against the internal surface of the uterus, are intimately adherent to it, and continuous with the then new mucous membrane which covers the other portions of the uterine cavity. The latter has generally a rosy, smooth or even brilliant appearance, except where covered with blood or purulent bloody mucus; on the other hand, the surface of the thick coat which forms the serotina is rough as if tuberculous, and irregularly mammilated. It has a soft, pulpy appearance, and can be easily scraped off; its color is a dark, greyish brown, sometimes nearly black; at other times the summit of these elevations, or even the entire surface, takes a greyish look, in consequence of mortification.

We sometimes find on the surface of this tissue the orifices of vessels which have been closed by fibrinous clots having a dark brown appearance. If these be followed and dissected out with scissors, the operator soon penetrates into the sinuses of the muscular coat, and there the vessels are much larger than elsewhere. In fact, on account of the numerous anastomoses between these large vessels when opened, the whole tissue has a peculiar areolar, cavernous appearance. It will also be seen that its thickness, and its projection above the internal surface of the uterus are principally due to the clots which fill these sinuses. When they are emptied, the intervals between them are very slight, consisting of a thin tissue which is intimately adherent to the internal face of the uterus, but as it is very soft it can be easily scraped off and detached with a knife.

In making the post-mortem examination in cases of puerperal fever, I have seen this rough coat, with a black grumous surface, mistaken by persons unacquainted with these facts for a portion of the placenta adherent to the uterus, and in process of decomposition. The remains of the decidua inter-utero-placental gradually disappear after delivery, and its traces are fainter in proportion to the time which has thus elapsed. But even when completely obliterated, the place which it once occupied can still be recognized, for this mucous membrane of new formation is always more uneven, and thicker there than over other parts of the uterus.

We next pass to the changes which the epithelium of the body of the uterus undergoes, in consequence of and during gestation.

(*To be Continued.*)

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SOME ADDITIONAL REMARKS ON THE TINCTURA ANTACRIDA, A REMEDY FOR DYSMENORRHŒA, AND CONSEQUENT STERILITY.

By E. D. Fenner, M. D., Prof. Theory and Practice of Medicine, New Orleans School of Medicine.

In my article on the above subject, which appeared in the last number of this Journal, I mentioned the unexpected coincidence of my publishing at the same time, and on the same subject, with the celebrated Professor Robin, of Paris. I had long contemplated publishing my observations on the use of this medicine in dysmenorrhœa, but postponed writing, until, at last, the paper was prepared for the press before I saw the translation of M. Robin's admirable and invaluable essay. The hope expressed in my previous article that the researches of M. Robin would give us more satisfactory information, both in respect to .the true nature of the disease and the modus operandi of the remedy, has been fully realized. Being on the very eve of departure from home, and expecting to be absent at least a month, all that I can do at present is, to endeavor to impress on the reader the importance of carefully perusing the highly scientific observations of M. Robin, which demonstrate most conclusively to my mind, that *dysmenorrhœa proceeds from a clot in the cavity of the uterus, and that the tinctura antacrida affords relief by preventing the formation of that clot, or dissolving it when formed.*

The medicine is an admirable *alterative and deobstruent,* and experience has proved its special determination and happy effects in the disorder under consideration. Whilst my last paper was going through the press, and after I had obtained from M. Robin the clue to the true pathology of dysmenorrhœa, I had a case that afforded a beautiful illustration of its correctness. A delicate married lady, aged about 25 years, and the mother of one child, was seized with severe uterine pains, cramps and a tendency to syncope—there was at the same time a discharge of coagula from the uterus. As she had missed her last catemenial period she thought herself pregnant and was about to have an abortion. We had no satisfactory evidence that such was the case. The hæmorrhage was slight, but continued to recur at intervals, and with every pain there was a discharge of small coagula. Now, said I to myself, if this be of the nature of dysmenorrhœa, my medicine will afford relief by causing the discharge to become fluid and free. I therefore ordered 20 drops to be taken morning and night. On the following day I found the patient relieved, the discharge fluid and moderately free. This appeared to demonstrate the *methodus medendi* of the medicine. I have since conversed with some of my cases and found that in every case the difficulty was dependent on coagula in the uterus. At the same time I can readily suppose that there are cases in which engorgement of the uterus and construction of the cervical canal would demand the use of leeches and the bougie.

I have much more to say on this subject, but am compelled to stop for the present. I find, on inquiry, that this medicine is extensively prescribed by physicians of the city whose attention I called to it several years ago. More anon.

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CASES OF DROPSY.

By A. S. Woolley, M. D., Shelby County, Ala.

Messrs. Editors—Should you deem the following cases worthy of your consideration, or of any benefit to your numerous subscribers, you can give them a place in your valuable journal:

Case 1.—A. C., aged 11 years, was put under my care with anasarca—no cough—bowels active—liver and lungs apparently healthy—urine scanty and highly colored—morbid appetite—skin dry—tongue coated with a thick yellow fur.

I prescribed pil. hydrarg. grs. v every night until copious dark bilious stools were produced. Then tr. digitalis, tr. iodin., and mur. tr. iron, aa ℥j, one teaspoonful to be given three times daily. This was taken during six days with no apparent effect. I then added squills for three days longer, and still no beneficial effect; but I found my patient had copious watery and lead colored discharges. I then discontinued all the previous medicines and gave her plumbi acet. grs. iij, and pulv. opii. grs. ss. every four hours to check the diarrhœa, and four days afterwards I found the bowels checked and the water passing off rapidly by the kidneys; and in two weeks, under the same treatment, she was perfectly well.

Case 2.—I was called to A. W., aged 9 years, with ascites—hot dry skin—pulse 120—tongue thickly coated—urine scanty—no cough, and liver and lungs in good condition—bowels very much relaxed, with bloody mucous discharges. I gave pil. hydrarg grs. iv, every night until dark bilious operations were produced. I also gave tannin and opium to check the diarrhœa, which proved effectual at the time. I then gave various diuretics to cause the water to pass off, but succeeded only partially, and in the mean time the diarrhœa became worse than before. I then gave pulv. opii. grs. ss. and plumbi acet. grs. iij. every four hours, to check the diarrhœa. This proved effectual, and at the same time the water was passing off rapidly from the kidneys. I did nothing else aad my patient recovered.

Case 3.—T. G., aged 60 years—with anasarca—tongue coated—pulse very quick—with a violent and almost unceasing cough—had to sit up day and night on account of difficult respiration, and nearly exhausted for the want of sleep and from fatigue, I gave pil. hydrarg. grs. viij, every night for three nights. This was followed by free bilious evacuations. Also gave syr. scil. comp. ℥ij, tr. opii. camph. ℥j, ♏, one teaspoonful every three hours, which relieved the cough in about two days. I then gave syr. fer. iod., tr. digitalis, and spts. nitre, for six days without any effect in removing the water. From my experience in the other two cases, I began to think there was some virtue in sugar of lead, and as my patient was very old and feeble and the case almost a hopeless one, I thought I would try it again, but with a different view this time, as my patient had no diarrhœa, but was, on the contrary, rather costive. I gave him grs. iv, every four hours, and, as he was suffering for sleep, added half a grain of opium with each dose. In ten days my patient was walking about, and all the water gone out of his system, and he seemed perfectly well with the exception of debility, which was soon remedied with bitter barks and brandy.

Now what say you cured my patients? Was it the mercury? If so, why did it not show some sign of it sooner? Was it the diuretics and astringents given before the sugar of lead? If so, why did they not begin sooner, for they had ample time in all three cases? As for myself, I shall always believe it was the sugar of lead, until farther experience teaches me to the contrary.

TWO CASES OF DISLOCATION OF THE LENS, WITH REMARKS.

By C. Beard, M. D., Visiting Physician Charity Hospital.

Wm. Burns, aged 30 years, general healthy appearance, presented himself at the Dispensary of the New Orleans School of Medicine to be treated for defective vision of the right eye. He is blind in the other, from suppurative inflammation, which occurred when he was young.

He states that last September, whilst fighting, he was kicked violently in the eye by his antagonist. The lid was slightly wounded, ecchymosis and swelling ensued, and the eye was closed for a few days. On opening it for the first time after the accident, he found his vision was very much impaired.

He entered the Charity Hospital, ward No. 10, in January last, was treated with blisters to temple and orbit, remained some time, and was finally discharged without any improvement having taken place.

On examination in the early part of the present month, the eye appeared perfectly healthy, with the exception that the iris presented the tremulous condition often indicative of softening of the vitreous humor. The pupil responded properly to light, and vision, although very much impaired, was not that of an amaurotic patient. On dilating the pupil with atropine, defective sight was at once accounted for. Lying horizontally behind the iris, its anterior surface turned upwards, was the lens, partially opaque and imbedded in the vitreous humor. It moved slightly in its new situation with the movements of the eye. It was not changed in form by absorption, although it had evidently occupied that position for over nine months.

The diagnosis being made, the treatment was not difficult. A bi-convex glass cured his amaurosis, and he went his way much pleased.

Case 2.—Gaspar Baer, a native of Germany, aged 57 years, entered my ward in the Charity Hospital, in May, 1857, blind of the right eye. On examination, he was found to be affected with hard cataract. I operated on the patient, by couching, with good result. The lens was easily depressed, and remained in its new position. Some inflammation followed the operation, but it yielded to an appropriate treatment. The patient was discharged in the following July, seeing very well.

Early in May, 1858, one year after the operation, he called at my office to be prescribed for. I found that the lens had remounted again and passed into the anterior chamber, where it then lay. It appeared unaltered by absorption. I advised the patient to go to the Charity Hospital, whither he went. He did not again fall under my observation.

These cases naturally suggest an inquiry as to why, other things being equal, absorption of a lens, displaced accidentally or by couching, should be so slow in some cases, while it is rapid in others. I have seen a moderately hard cataract, after partial depression, disappear by absorption in less than six weeks; while in the two cases cited above, although the lens was placed apparently under the most favorable circumstances for removal, absorption was resisted nine months in one, and twelve in the other. Malgaigne mentions the case of an old man upon whom couching had been repeated several times, the lens rising up again at longer or shorter intervals; finally, after the lapse of nine years, as the result of a fall, it passed into the anterior chamber, whence it was removed by extraction, apparently but little altered.

Can the lens be depressed with its capsular envelope unbroken? Let us examine its anatomy.

Robin describes the capsule as being composed of two delicate homogenous layers, of considerable strength; the anterior layer nearly double the thickness of the posterior, which is exceedingly thin; the change in the character of the two layers takes place abruptly at the periphery of the lens, where their difference in thickness is marked.

The capsule, according to Græfe, is closely applied to the surface of the lens. The liquor morgagni being nothing but a post-mortem production, as he has very well demonstrated.

Brücke, in his admirable monograph of the eye, describes the lens and capsule as fitting into the concave anterior surface of the hyaloid body, (fossa hyaloidea,) the posterior layer of the capsule being inseparably connected with that portion of the hyaloid membrane investing the fossa.

Hyrtl also speaks of the indissoluble connection of these parts.

Besides this attachment of the capsule to the hyaloid, there is that of the zonula of Zinn, according to Hirschfeld, a prolongation of this delicate membrane, which, after arriving at the outer circumference of the ciliary body, with which it contracts adherences, becomes thickened, folds itself inwards, divides into two plaited layers, which, to a certain extent, enfold the capsule.

Now, considering this intimate connection of the capsule with the adjacent structures, and reasoning solely from an anatomical point of view, one can scarcely help concluding, with Hyrtl, that depression of the lens, with its capsule perfect and unbroken, is an anatomical impossibility, and that it would be easier to rotate the whole vitreous body upon its axis than to accomplish any such manœuvre. It is, however, astonishing, that this author, one of the most distinguished anatomists of the day, systematic in pointing out the mistakes of others, should explain as he does the nature of that envelope, which is certainly observed in lenses which have long remained unaltered after depression. He says, that which is mistaken for the capsule in these cases, is nothing more than the external layer of the lens, which has remained transparent whilst the internal portions have become opaque. The microscope has, however, too clearly demonstrated the difference in structure of the capsule and the lens, to permit of one being mistaken for the other.

It may, then, be set down as a fact, that the lens, with its capsule entire, may be dislodged from its natural position, and that its removal by absorption in cases of depression is dependent in a great measure, if not entirely, upon the condition of this envelope.

Remounting of the lens after couching is one of the most frequent and unpleasant accidents which can follow the operation; it occurs usually soon or immediately after the removal of the needle from the eye. This is due probably in every instance to the fact that the capsule has been depressed with the lens, and that its peripheral connections have not been entirely severed. Hence the propriety of carefully lacerating the capsule as thoroughly as possible in every case in which the needle is used.

HOSPITAL REPORTS—A CASE OF YELLOW FEVER, WITH CLINICAL REMARKS.

By Professor E. D. Fenner. Reported by Theogene Chachere, Student of Medicine.

Charity Hospital, ward 17, bed 252; admitted, July 8th, 1858; Thos. Stone, aged 18 years, native of England, resident of New Orleans the last ten months, occupied on a towboat.

Previous History of the Case.—Was attacked about 4 o'clock in the morning of July 8th, with chillness and pains in the back and limbs, soon followed by hot fever and severe pain in the head and lumbar region. Entered Hospital 8½, P. M., same day, and was seen by the Assistant House Surgeon, who prescribed a hot mustard foot-bath and an ounce of castor oil.

July 9th. Was seen for the first time by the visiting physician of the ward, Dr. Fenner.

Present State.—The patient is a healthy looking youth, of ordinary size; countenance flushed; skin hot and sweating freely; tongue coated in the centre with a yellowish fur, the tip and edges bright red; it is rather broad, thick and moist; the gums covered with a white pellicle; thirst urgent; stomach quiet; no appetite; abdomen soft and flaccid with slight uneasiness when pressed; bowels moved four times by the oil; urine free; pulse full and soft, beating 98 to the minute; pains in the head, back and limbs, somewhat abated; breath fœtid; respiration somewhat accelerated.

Dr. F. remarked that this was a fair specimen of ordinary, mild yellow fever, and as the young man had come in at a proper time, (a thing so seldom done in this disease,) it would afford an excellent opportunity to observe the course of the disease. He prescribed the following:

℞. Neutral mixture, ℥iv.
S. Tablespoonful every two hours.
Orange-leaf tea for common drink.
Lumps of ice occasionally.

July 10th. Patient says he feels better—passed a quiet night—pains in head and back much relieved—still has some fever—pulse 92—skin very moist and warm—tongue broad and moist, the fur whiter, edges and tip red—thirst abated—says he feels very weak—stomach uneasy, vomited some bilious matter this morning; abdomen soft—one stool since last visit; urine free and highly colored.

Treatment.—Continue neutral mixture, and have some rice or barley water.

Dr. F. remarked that the case was progressing as favorably as possible, and cautioned the patient against raising his head from the pillow for any purpose whatever. Ordered the bed-pan to be used in case the bowels acted. He also remarked privately that the case was getting now into the critical stage, and his fate would be decided within the next twenty-four hours—that he probably would require a little brandy in the course of the night, as the fever goes off, and instructed the resident student to that effect.

July 11th. Patient quiet—passed a tolerable night—slept occasionally—took a little weak brandy and water—vomited some light, colored, flocculent matters, which, on being tested, were found to be extremely acid. His skin is now cool and moist—tongue cleaner and edges paler—but little thirst—stomach uneasy, with slight nausea—abdomen soft—no stool since yesterday morning—urine free—pulse eighty-eight—considerable prostration.

Treatment.—Ordered a tablespoonful of the Sydenham mixture every hour or two, if awake. Sinapism over the stomach—very light nourishment.

Dr. F. remarked that we had here a beautiful illustration of the favorable crisis of yellow fever. That the mixture ordered was composed of a little phosphate of lime and gum arabic dissolved in orange-flower water when properly made, and was expected to act as a demulcent and antiacid to neutralize the great tendency to acid elimination that always occurred at this period of the disease.

July 12th. Patient much better—passed a quiet night—skin cool and moist—tongue cleaning off—some appetite—stomach perfectly quiet—one stool since yesterday—urine free and high colored, containing a considerable amount of albumen—pulse eighty-four.

Treatment.—Take a little more of the mixture, and have chicken water.

July 13th. Patient pronounced convalescent—pulse seventy-four—complains of debility—but little appetite. Was allowed to change his clothes and to take a little porter and soup.

July 14th. Still improving—pulse seventy-two.

July 17th. Was discharged from the Hospital at his own request. His pulse had fallen to fifty-eight on the 15th.

Dr. Fenner finally remarked that we had here seen a mild attack of this fever, in a very favorable subject and treated in good time, yet carrying the patient to the verge of black vomit, before recovery commenced. Moreover, it is the only case that has yet been discharged from the Hospital this season. He thought it indicated the commencement of a severe epidemic.

SPECIAL SELECTIONS.

GULSTONIAN LECTURES FOR 1858, AT THE ROYAL COLLEGE OF PHYSICIANS.

By JOHN ADDINGTON SYMONDS, M. D. F. R. S. E., Consulting Physician to the Bristol General Hospital, etc.

ON HEADACHE. LECT. III.—*Continued.*

The treatment of Neuralgic Headache scarcely needs special consideration, after what has been said of the treatment of nervous headache. It is sufficient to observe that this disorder is often of such severity and pertinacity as to require the boldest exhibition of remedies: full doses of opium or aconite at the time of pain, and large quantities of quinine or arsenic in the interval. These are the cases in which iron, and especially the carbonate, has so often answered well. I can also speak favorably, from experience, of large doses of muriate of ammonia. I ought not to omit to mention my sense of the great utility of aconitine ointment as a palliative. The endermic use of morphia, sprinkled on a blistered surface, has likewise been one of the most powerful of anodyne resources. On what principle relief is afforded by a ligature round the head, it may be difficult to explain; but the fact is undeniable, and the remedy of ancient repute. Shakspeare makes Prince Arthur say to Hubert,

> "When your head did but ache,
> I knit my handkerchief about your brows,
> * * * * *
> And with my hand at midnight held your head," etc.

A benumbing influence is exercised on the superficial nerves; and this would seem to be reflected on the internal nerves; which are the seat of pain. I have in my own person felt temporary relief to neuralgic toothache, by firm pressure of the cheek; and in this case it is unquestionable that the operation must be of a reflex nature. After all, I doubt if the action be more mysterious than that of sedatives applied externally; the mode of which action has been already discussed (a).

Our remarks on treatment have thus far had reference to the headache of congestion, and to that which is a purely nervous affection. There is a mixed form of headache, to which we adverted in a former lecture, and in which there are both these elements, usually with a predominance of one of them. These cases are most difficult to manage; for while they require a certain amount of the measures suited to both kinds of disorder, it is very easy to go wrong by excess. In our anxiety to keep down vascular turgescence, we may be tempted to the use of remedies which, though they afford temporary relief, are almost sure to add to nervous irritability. Such are local depletion, blisters and purgatives. Yet if the neurotic derangement seems uppermost, and we employ nervine tonics too freely, there is a risk of aggravating the vascular disorder. This, however, is the slighter risk, unless our remedies are very powerful. Every case will need a separate consideration and an individuality of treatment. On the one hand, we may find that a few leeches or a blister to the nucha, or leeches to the hæmorrhoidal vessels, or mustard pediluvia, or dry cupping along the spine or on the temples, with shower-baths, and cold sponging of the spine, followed by diligent friction, and tepid or evaporating lotions to the forehead and scalp, with occasional aperients in the morning, diaphoretics at night, and a well-regulated diet, will meet the congestive element without recourse to any of the more energetic antiphlogistic measures; and, on the other hand, if we cannot apply to the heroic doses of quinine, we shall derive great assistance from zinc, especially the valerianate, as well as from most of the pure nervines. Of the reinforcers of nervous power, I have had reason to think well of the action of minute doses of strychnia and of nux vomica, when I could not apply to quinine or iron.

Phosphorous I have only employed in the form of phosphoric acid and of phosphate of iron, not without hints of advantage, but not with such signal good results as to make me dwell upon them. But I have been informed by others that they have attached great importance to this agent.

For the relief of present pain in these cases, anodynes must often be administered. Opium, or morphia combined with alteratives and salines, succeed with some; others are more profited by henbane, camphor, and Indian hemp, or by the extract of poppy.

(a) My friend, Dr. Williams, whose opinion is of great weight in all questions of pathology, is inclined to refer the alleviation produced by external pressure in some kinds of headache to some operation exerted on the bloodvessels.

Treatment of Sympathetic Headache.—If we were right in our conclusion that pain is rarely forced on the nerves of the head, that is, that it rarely occurs without some readiness of the nerves to ache when any offence is transmitted to them, it will be evident that the management of cases of this class, while it consists in warding off such causes of irritation as may come from distant organs, is equally concerned in lessening if not in removing the cerebral susceptibility also. The treatment of gastric headache will not be confined to the cure of dyspepsia, of whatever form, though this is the main object; for in diminishing the frequency of attacks dependent on gastric irritation, you are in one respect acting on the primary fault, because, by diminishing the habit, you allow the nerves time for growing into a less irritable condition, since frequent returns almost inevitably confirm the habit. But the correction of dyspepsia produces effects co-extensive with the whole economy, and therefore if a patient has by medicinal courses, or by regulation of diet, or by hygienic methods lost his dyspepsia, and with it his tendency to headache, it does not follow that he has merely put away the sympathetic origin of his complaint. He will, from the same system of treatment, through the remote influence of improved digestion, have acquired healthier blood and a sounder tone in his nervous system. Remarks of a similar bearing might be made on the influence of an improvement in the functions of the uterine system in patients whose cephalalgia has disappeared after such improvement, for in these cases also there is the positive advantage of amended general health, as well as the negative good belonging to the removal of a source of irritation. Still we have to bear in mind, when reasoning upon such examples, that although the cerebral nerves may be so irritable as to derive sympathetic disturbance from any organ whose function is deranged, yet that the specially sympathetic headache implies a tendency to respond to irritation from one part more easily than from another. Given the same susceptibility of the nerves of the head, in one person it will only be provoked by gastric irritation, in another only by uterine. But independently of this occasional communication of disorders, there would appear to be sometimes established a permanent morbid *consensus* between the cephalic ganglia and the semilunar in the one case, and between the cephalic and the ilio-lumbar in the other. And so the subsidence of one will allow the other to subside also. It is not often, however, that of the three great ganglionic centres, two only suffer together, with exemption of the third. More frequently they are more or less involved in the painful consequences of their alliance.

As to gastric headache, I have generally found that when an attack of this nature has been once set up in consequence of disorder of the stomach, it will take its course. Now and then a remedy may tell upon it, if employed at the very onset. Thus, as one of the most common causes of irritation is an excessive secretion of acid fluid, the patient has been directed to take a teaspoonful of carbonate of soda or magnesia, in a tumbler of water, with a teaspoonful of sal volatile, to which I have sometimes added a teaspoonful of common salt, with a view of assisting the stomach to eject its contents by vomiting, if so disposed. And sometimes this potion has stopped the pain by neutralizing the acid matter and its impression on the gastric nerves. But more commonly it is the state of the membrane which gives birth to this secretion that is the source of irritation, and cool mucilaginous drinks, with prussic acid and alkalies, must be continued for some hours, with synapisms to the epigastrium; for gastritic dispepsia is more commonly the cause of headache, than atonic indigestion. But, whether it be the one or the other, the irritable nerves must be soothed; and this purpose is often magically answered by a cup of strong coffee. The preventive treatment of the dyspepsia I cannot, of course, undertake to discuss on this occasion; and I seem already to have said too much. For a like reason, I do not speak of duodenal indigestion, and its bilious headache. I will only mention, as a contribution from my experience of such cases, that long periods of exemption from returns of these headaches have occurred to patients who have faithfully observed my direction that they should drink a tumbler of common salt and water every morning, an hour before breakfast; and that I have, in other instances, been led to assign great value to the use of pills of rhubarb, ox-gall, and nux vomica, at dinner-time.

The uterine headache has not in my hands received treatment very different from that which is applicable to neurotic headache in general. But of the anodynes for immediate relief, I have perhaps oftener selected camphor and belladonna, and conjoined with them sinapisms to the sacrum, and the use of the hip-bath. A full dose of chloric ether or of chloroform has also acted very happily in these cases. Of course the remedies must be varied according as the catamenial flow is defective or in excess.

Treatment of Toxæmic Headache.—On that form of secondary headache, which is an all but constant symptom of continued fever, it is obviously unnecessary to trouble you with any lengthened remarks. That although probably excited by the altered condition of blood which belongs to the fever, it is, in part, dependent on the congestion of the vessels, is indicated by the measures which relieve the pain. A cool pillow, removal of the hair, refrigerating lotions, leeches to the temples, and sinapisms to the nucha or epigastrium, so well known in the routine of fever treatment, are remedies for vascular congestion; yet they by no means displace the use of sedatives. Small doses of Dover's powder, or henbane, with a mercurial, are very helpful. And though so many of the efficient remedies are antiphlogistic, yet that the pain is not purely vascular, may be inferred from the fact that there is a natural tendency to its subsidence after a few days, and that had the vascular disorder been commensurate with the pain, worse results must have ensued, in the way of inflammatory exudation. In former times the answer to this remark would have been that the delirium, hebetude, and sopor, which follow the headache, indicate such consequences of the prior congestion or sub-inflammation; but this, again, is contradicted by the negative results of post-mortem examinations in persons who have died at this period of the disorder. The disturbing action of the febrile poison was once brought prominently before me in the case of a woman who sank after violent maniacal excitement of five or six days' duration, and in whom I suspected typhoid fever, only because two or three others of the family had been seriously affected about the same time and in the same house. A careful inspection of the brain detected nothing but turgescence of the membranes, but the follicles of the intestines were frightfully ulcerated, even at that early stage, and showed the intensity of the poisonous action in that part of the body coincident with a no less deleterious process in the brain, which however did not reveal itself in textural changes.

The secondary headache belonging to rheumatism requires that discrimination, to which I formerly alluded, of the pain which is merely a symptom of febrile movement, from that which depends on a profounder action of the blood-poison, whether on the dura mater or on the brain itself. And a like remark applies to acute gout.

Chronic rheumatic pain of the head may be often traced to cold, and may be cured by a warm nightcap, or by discontinuance of cold ablutions.

The headache of chronic gout I have found amenable to most of the remedies which are useful in the headaches which have not this complication. But colchicum often comes in with marvellous power. I am informed by Mr. Spencer Wells, that having seen how great a control quinine exerts over this form of cephalalgia, he has been disposed to inquire whether its action bears any relation to some curious observations of Ranke on the diminution of uric acid in the urine of persons under its operation. As far as my own observation goes, I should incline to the belief that the cephalalgia of the gouty diathesis is not the direct product of a gouty poison, but only a neurotic disorder belonging to that state into which the nervous system has grown in these subjects, analogous to that which occurs in other derangements of the constitution. Were it excited by a chemical materies morbi, we might expect such changes in the intra-cranial fibrous tissues as we find in the joints where there have been actual deposits of gouty matter.

And I take the same view, *mutatis mutandis*, of the nervous or neuralgic headache consequent on the syphilitic constitution, and of its appropriate treatment.

If we try to sum up one or two practical conclusions, apart from such speculative discussions as I may have too much indulged in, I think it will be found that they are mainly these :

1. That headache has an importance of its own; and that, whatever connexion there may be between this affection and the disorders of other parts of the body, the head itself is to have the chief consideration.

2. That headache, whether primary or sympathetic, considering the frequency of its occurrence, is rarely dependent on disorder involving danger, or serious impairment of the function of the brain.

3. That symptomatic headache, even when belonging to grave lesions, may be benefited by the treatment applied to the primary forms.

4. That the primary headache, whether caused by an original susceptibility in the ganglionic nerves of the brain or by a morbid sensibility resulting from previous disease or weakness, is to be relieved by sedatives, and to be cured by tonics and nervines. He will be the most successful practitioner who can best wield those two great weapons, Quinine and Arsenic.

5. There is a natural tendency in this disorder to decline with advancing life.

On comparing cephalalgia with pain in other parts, and with those forms of pain which bear most analogy to it, more especially in the paroxysmal character, and in the accompanying conditions and symptoms, I think it would be difficult to find any affection to compete with it, either in frequency of occurrence or in the duration of the liability of attacks. Certainly we may exclude from such rivalry, angina pectoris, gastralgia, enteralgia, and neuralgia of the extremities. Toothache, as we have already observed, is too obviously related with structural changes, to be put in the category. But there remains that pain, of which we hear so much in females—dolor dorsi. Ladies have been divided into two great classes: those who have a back and those who have not—a sufficient indication of the frequency and chronicity of pain in this situation. It resembles headache in its mode of coming on and in its remote causes, and it resembles it equally in its pathology. Like headache, it may be related with structural changes and with congestive or inflammatory states; or it may be purely nervous, and probably it is far more often of this kind, and as such it may be dependent on original or acquired sensibility of the uterine and ovarian nerves, wrought upon by abnormal states of the functions of those organs, or by disorders in other parts of the system. The sympathetic production of this pain, which makes so large a part of female misery, might be traced to radiation in the sensorial centres, or to transmission from ganglion to ganglion, in the same manner which was attempted in reference to the production of headache; and I believe that an energetic and persevering use of like remedies would be of equal use, and certainly might in many cases supersede the use of distressing local applications.

I will not enter upon the analogy of gastralgia, as it would occupy too much of our time; but I cannot forbear adverting, for a few moments, to sternalgia, commonly known as angina pectoris.

Far less frequent than headache, it is often associated with serious and perilous disease. It is true that there are many cases in which the seat of pain is the same as in the more formidable cases, and yet in which the pathology is very different and the prognosis by no means grave. For in the latter cases the irritation starts from the gastric nerves, and the reflected pain takes the course of the same nerves as are involved when the irritation has sprung from the heart or the aorta. In hysterical cases it is probable that the irritation begins either in pelvic nerves or in the emotional centre. But these gastric and hysteric cases stand in the same relation to the true angina pectoris as the sympathetic headache to the true nervous headache, the pathology of which begins and ends in the cerebral nerves themselves. But the primary angina is dangerous, while the primary cephalalgia is a slight malady, and the reason is obvious. The nerves of the brain, having no other obvious functions than those of transmitting impressions and of regulating capillary circulation and molecular nutrition, may be disturbed in this as in many other organs, without serious detriment. But who can say to what extent those ganglionic nerves, which regulate the rhythmical motion of the heart, and the life of which is the life of the heart itself, who can say how far they can be safely interfered with? It is true, they may be sympathetically affected without injury. The varying rate of the pulsation in a thousand disorders, and the irregularity in others, denote that reflected peturbation does not necessarily imply danger. But when such perturbation begins in the heart itself, whether from degeneration of its substance or from disease of its nutrient arteries, and even when the disease is only in the aorta, it betokens a profound lesion, the influence of which may at any time be fatal. When the change takes place in the life-giving nerves of the part, who can say? Extreme disease may have been ascertained, and yet very little trouble may be occasioned, because that which, after all, is most vital, has not been touched. When we discover, after death, disorganization to the greatest degree, we are often compelled to admit that amidst all that change and wreck, there is nothing visible that tells why the instrument stopped its action just when it did, rather than months or even years before.

Statistical Memoranda.—In order to obtain some numerical data as to the frequency of certain presumed causes of headache, and as hospital registers would not supply information respecting cases which only casually come within their scope, I distributed some circulars of queries, for obtaining replies to which I am much indebted to many of my professional friends. They were queries which were easily answered, and that the answers were for the most part given thoughtfully and carefully, was obvious from their consistency in themselves, and from the general agreement between the different respondents. Though I cannot bring forward any very new results from the answers, yet they afford confirmation to many of the remarks which I have ventured to make upon the disorder under consideration.

We may first direct our notice to those points which indicate a neurotic susceptibility on the part of the subjects of the disorder.

Of 90 cases 76 were females. These numbers establish more strongly than I should have expected, the fact, which is testified by most of the old writers, that females are more frequent sufferers. If we endeavor to ascertain whether this liability of females bears relation to their sexual functions, we do not find much to support this suppositon. Of the 76 females, 40 were single ; out of the 76 there were 35 who referred to the periodical health as having been in some way connected with their attacks ; very few dated from puberty—but a large number from early life. Of the 22 out of 90 who referred to previous illness, none specify illness referable to pregnancy or parturition. In 11 loss of blood is mentioned, but while some speak of it as artificial, none refer to menorrhagia, or to the floodings of abortion or childbed. Now, every practitioner must have noticed that women who have suffered from such losses have been very liable to headache, but it is clear from the replies in general that the specially feminine proclivity to headache is independent of these casualties. But we have said that 35 did refer to the periodical health, and the result of my cross-examination in many of these cases brought me to the conviction that the menstrual function only presented occasions of offence, exciting causes as we often call them, but that the earlier antecedent was a constitutional liability not dependent on this function.

Putting aside the catamenial function, we inquired into the mode of life, and found nothing to lead one to suppose that this was importantly concerned in the production of female liability.

The predisposition more probably originates in the nervous system ; and it is needless here to adduce proofs of what is admitted on all hands, that nervous mobility, irritability, hyperæsthesia, or by whatever other name we choose to designate susceptibility of nervous disorder, is oftener found in the female than in the male subject. It is likely to exist in organisms which evidence a capability of so much fineness and delicacy of perception, united with so much proneness to emotional excitement, and in which also we observe the functions of organic life to be so readily wrought upon by passing states of thought, sensation, and emotion.

Of the exciting causes, I find that emotional disturbance has the highest number. Out of 90 cases, 53 declared this to be one of the causes of their attacks; 48 also considered that atmospheric states were to be blamed; and 25 specified thunder.

It would be little better than a platitude to say that it is far more difficult to trace the origin of a predisposition superinduced on the system, than to indicate causes nearer at hand. Accordingly, in these answers, although the questions afforded sufficient opportunities of stating circumstances in the mode of life, occupation, habits, climatic influence, etc., which might have begotten the tendency, yet very little light is thrown on this subject by the answers. And I incline to the belief, that the cause of this tendency lies too far off to be reached by reference to the experience and consciousness of the individual. It lies in the original mould of the nervous system. This conclusion is supported by the fact that 44 had suffered from very early life, and a large proportion of these used the formula "as long as they could remember." An additional presumption appears from the proof of inheritance of the liability. In 19 cases the mother is mentioned, in 9 the father, and in 12 both parents; in all, 40 give explicit evidence of hereditary predisposition, and a few others mention cases in collateral branches.

The negative evidence as to *diet* is curious. Out of the 90 cases only 19 blame their diet, and these, if they specify any at all, only mention some article of food which has been at times an apparent and immediate cause of disturbance; of the remainder, 62 deny that diet has anything to do with their attacks; and 9 make no response, or a very indefinite one.

For these negative results as to diet, there are two obvious reasons: first, that people are very slow to observe the gradual or habitual influence of any system of diet into which they have fallen by custom or association; and secondly, that if food does not produce marked disagreement in the first stages of digestion, they are ignorant of what passes in the duodenal and subsequent processes.

As to the influence of *climate* in augmenting the predisposition, more evidence is given. Thirty-nine either say that climate is a matter of indifference, or they make no answer. But 29 seem very clear that they are least liable to attacks of headache in places where the air is dry and bracing; 6 commend cold atmosphere, 6 condemn it; 8 praise warm atmosphere, 3 dislike it; 6 are in favor of sea-air, and 4 are averse to it. I do not attach any great importance to the paucity of answers as to marine air, because it was not particularized in the questions. Still, if sea-air, *quâ* sea-air, had made any great impression, either as a friendly or as a hostile agency, I think it would have come out in the answers.

Fatigue is mentioned as an exciting cause by 32, though, from inadvertence, the queries contained no pointed reference to it.

Some of the questions were framed with the view of ascertaining how far numerical statements would bear out the prevalent notion of a connexion between headache and disorders of the digestive organs.

Out of the 90 cases, 33 said that *vomiting* or nausea occurred in their attacks; 12 reported *nausea only;* 35 experienced neither; 10 made indefinite answers, or none. Of the 45 cases of vomiting and nausea, 33 declared that the vomiting came on after the accession of pain. Of the rest, 6 spoke of the sickness as simultaneous, and 6 gave no evidence on the subject.

To the question whether the pain had been relieved or removed by sickness, 18 of the 33 said they had been relieved sometimes or partially, and 2 said the pain had given way after prolonged vomiting. Of these one used the expression, "after bringing up pancreatic juice."

These answers would scarcely lead to the inference that the headache depended on the presence of anything in the stomach; for though 18 of those who had vomited spoke of being relieved, this term is very vague, and it is all but impossible to seperate the mere effects of the act of vomiting on the circulation and the nervous system, from what might be due to the ejection of food or morbid secretions from the stomach.

Not more satisfactory are the replies to the question as to the action of the bowels. Of the 90 only 12 distinctly affirmed any relation between their headaches and the action of the bowels; 54 denied any connexion; 17 gave ambiguous answers, and 7 were silent.

And in reply to whether they had derived relief from aperients, only 15 gave distinctly affirmative answers; 17 said "sometimes," or used other qualifications; 6 were doubtful; 3 were certain that they were made worse by aperients; 36 declared that they were not relieved; and 13 gave no distinct response. These replies do not tell much either way. When aperients have been taken the pain generally subsides spontaneously before the bowels have acted; and therefore even with all the disposition which most patients manifest to attach undue importance to this function, they would not assign the relief to the action if the action occurred subsequently to the relief of pain. And yet we know theoretically that they might do so, since the artifical impulse given to the function of the small intestine may have removed sources of irritation. On the other hand, the affirmative respondents are as likely to have unconsciously set their replies into consonance with their preconceived notions of the value of aperients.

To have asked my catechumens whether their headaches had anything to do with bile or bilious disturbance would have, I well knew, called forth a chorus of "Yes." But as I should have been none the wiser, I thought it better to limit the question to the "accumulation of bile manifested by the skin."

Of the 90, 65 were obliged to say "No;" 14 gave a doubtful or qualified "Yes;" and 11 were mute. When asked if they were liable to indigestion, 39 answered in the affirmative, but with qualifying terms, indicating that they were not quite certain about the matter.

I will not trouble you with further details, but as far as these replies bear upon the relation between headache and disordered digestion, the evidence is, to say the least, ambiguous and unsatisfactory. But if any decided inference is to be drawn from it, we must conclude rather against the existence of such a connexion in many of these cases, and we do not advance beyond the fact that when there is the disposition to cephalalgia, the digestive organs may afford occasions of excitement to that disposition.

Two of the questions were intended to elicit facts as to the dependence of the attacks on hyperæmia. Of the 90 cases, 38 reported that their headaches were increased by the recumbent position, 27, that they were relieved, 5 were doubtful, and to 17 it was a matter of indifference. Of those who were relieved, some said that the position at first was painful, but afterwards, that it became comfortable; and others appeared to think that the rest was quite as important an item as the recumbence. Change of posture, of course involving motion, almost uniformly aggravates the pain; and we may fairly suspect, that of those who say that the pain is increased by laying the head low, some have been deterred from fairly trying it by the first discomfort of the change.

To the question whether the pain was relieved by stimulants, 35 answered in the affirmative, 30 simply denied, 8 reported it to be absolutely increased, and the remainder were doubtful, or indifferent, or had never tried such remedies. I do not think that much value can be attached to these answers. A headache may be associated with passive congestion, and may be relieved by stimulants. It may be purely nervous, and yet aggravated by them, because in so tender a state of the nerves the slightest increase of vascular disturbance will increase the suffering, whether produced by exciting drinks, or as we have just noticed, by slight changes of posture.

The inquiries as to the operation of these and other agents were made, chiefly to obtain information as to the pathology and etiology of the disorder. But one general question was put as to therapeutics:—"From what kind of remedies have you derived most relief?" The question was too wide; for, while some enumerate the remedies which have done good at the time of pain, and also those which have lessened the frequency of returns, others have confined themselves to the treatment which has relieved the attack. Twenty-six specify benefit from *tonics*, *quinine* being mentioned oftener than any other; 15 mention *sedatives;* 11 insist on profound *quiet;* 4 speak well of *cold applications*, 2 of *warm;* 1 ascribes relief to *leeching*, and 1 (the same respondent) to *blisters;* 3 are so unenterprising as to have tried nothing; 12 have been altogether unsuccessful, and they now rely only on time and patience. The rest give vague answers, not requiring further comment.

As far, then, as the information is positive, we may say that the evidence is in favor of tonics and sedatives.

It is curious that, in the whole series no mention is made of phlebotomy or cupping; and leeches are noticed only by one person. Had the same set of questions been answered five-and-twenty years ago, I presume that antiphlogistic remedies would have received more notice.

Before closing this statistical attempt, I may mention that of the 90 cases, 43 report that the attack comes on very early in the day—most of them on *waking*. It would be easy to speculate on the cause of this general fact. Some would blame the position in bed (*); others the long interval after food had been taken; others the state of the nervous centres belonging to sleep; others the diminution in the intensity of terrestrial magnetism in the morning. For my own part, I wait for further illumination on this as on many other topics belonging to the whole subject.

EDITORIAL AND MISCELLANEOUS.

Health of our City.—We have delayed the issue of our number, in order to procure the latest data in relation to the health of the city. Yellow fever having made its appearance here about the middle of June last, of course there was much concern felt in relation to its extension; indeed, many were firmly convinced that we should very soon run into a true epidemic of this dire scourge. Much, if not nearly everything, however, has to be learned yet in relation to the mode of origin and progress of the disease; predictions in relation to either, amount to mere surmise, and he who makes them is almost as desperate as the purchaser of a lottery ticket.

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* I was curious to see if there was any coincidence between the fact of accession of pain in the early morning, and increase of pain by recumbency. Of the 38 cases who reported such increase, I find that only 16 report their attacks to have been of early occurrence in the day. If this is of any value at all, it would be adverse to the notion that position has anything to do with early accession

We are just from the office of the Board of Health, where, through the courtesy of the worthy Secretary, Dr. Baldwin, we had full access to the mortuary reports of the past week. We find recorded twenty-six deaths by yellow fever, against twenty the week previous; which, taking the intense heat, which we have endured recently, into consideration, is certainly a very trifling increase—far below the anticipations of the least anxious. Again, and contrary to general rumor, we find that the disease is almost wholly confined to the laboring class of people, and, of these, to those most of all exposed to the vicissitudes of weather, viz: individuals engaged on our levee—a large proportion being sailors of ships arrived here comparatively recently. We have made diligent inquiries of our professional brethren, and it is the rare exception to find one who has a case of yellow fever under treatment. For ourself, we have seen one case in private practice. This is a negro man who has spent two summers here, but never had the disease. He is engaged as porter, ect., about a commission house, and attends to receiving bills of lading from the steamboats. He was taken on the 22nd inst., after having felt a little badly for a day or two previous. His attack has been decided, but uncomplicated, and at this moment, (26th) he gives every promise of recovery.

Yellow fever is not anything like epidemic, then, in New Orleans, and we only hope that it may continue thus limited in its operations. And yet we cannot say that our city is healthy. The reader will see that the weekly mortality is entirely too great. Still, the physicians find it "too healthy" for their own immediate good, as the mass of the sickness and mortality is in a sphere which affords but little "practice." For weeks past there has been a continuous exodus of the "passing" population, until the "residence portions of the city" look almost wholly deserted. Will the reader blame us, if, in very desperation, we throw pen, ink and paper to the dogs and "go over the lake" too?

WILL VERATRUM VIRIDE PRODUCE ABORTION?—An esteemed correspondent writes us as follows: "I wish you would give your experience with veratrum viride in pregnant women, in the next number of your journal—whether or not it acts like ergot on the uterus. I give you a case.

"I was called to see Mrs. ——, aet. 32—said to be about six months advanced in pregnancy, with her seventh child. She had never miscarried. I arrived at the house about 10 o'clock, A. M. She had been confined to her bed about eight or nine days. Had had no physician attending her. I diagnosticated her disease typhoid fever—prescribed veratrum viride in the usual doses during twelve hours. When I returned to her, she told me she felt some pains in the lower part of the abdomen, like labor pains. As she had never aborted, but little attention was paid to it. I continued the veratrum, together with two grain doses of calomel. At 9, A. M. next day I was called in haste. On my arrival, I found that she had miscarried. She then told me that she had felt pains like labor pains after taking the second dose of the veratrum.

"I, also, have a friend who gave it to three negro women laboring under pneumonia in the first stage. They were all enciente, and all aborted. I, also, heard an old and experienced physician say that he had met with a similar misfortune in a case of typhoid fever."

We are sorry that we cannot speak from experience in regard to the administration of this remedy in the class of cases indicated. We have never seen the remedy administered to a pregnant woman. The written testimony furnished in our journals is conflicting, but the mass of evidence, so far as we are aware, is against the proposition of the remedy acting like ergot. According to our reading, it has always appeared that the abortions might be traced to other causes, or to the fact of the medicine having been given so as to produce excessive nausea or vomiting, to which latter the abortion might be attributed. A patient freely nauseated or vomited by ipecac or tartar emetic will sometimes abort, but surely not as a consequence of the specific effect of these medicines on the uterus.

We do not think that either of the typhoid fever cases mentioned by our correspondent will stand as evidence of the specific power of veratrum viride over the uterus. Women affected with typhoid diseases during pregnancy are exceedingly apt to abort, and we think all the probabilities are in favor of coincidence, rather than cause and effect, in the cases cited. It is well known that an attack of yellow fever almost certainly produces loss of the fœtus—*certainly*, if the disease is of an adynamic form.

Of the results in the cases of the three negro women with pneumonia, we cannot express our opinion. We should have been pleased to have seen accurate and complete data. A thing is *propter hoc* merely because it is *post hoc.*

SHELBY MEDICAL COLLEGE.—We have at last received the announcement of the Shelby Medical College, Nashville, Tennessee. This new institution goes into operation in the fall, and with the following gentlemen composing the Faculty, viz: Dr. John F. May, Professor of Surgery; Dr. E. B. Haskins, Professor of Theory and Practice; Dr. John P. Ford, Professor of Obsteterics, etc.; Dr. S. L. Maddin, Professor of Anatomy; Dr. John H. Callender, Professor of Materia Medica; Dr. R. O. Curry, Professor of Chemistry, etc.; Dr. D. F. Wright, Professor of Physiology, etc.; Dr. H. M. Compton, Demontrator of Anatomy. The Faculty announce that they will be thoroughly prepared to teach medicine properly, and we wish them the fullest success in their enterprise.

PROFESSORIAL CHANGE.—Dr. Daniel F. Wright, of Memphis, Tenn., desires to announce his resignation of the chair of Physiology and Pathology in the Memphis Medical College, and his acceptance of the same chair in the Shelby Medical College, at Nashville. Dr. Wright was editor of the Memphis Medical Recorder, and now that he becomes a colleague of Dr. Richard O. Curry, the respected editor of the Southern Journal of the Medical and Physical Sciences, we should not be surprised soon to find on our table a fusion journal. Two such editors would create a stir amongst rivals.

NECROLOGY.—Dr. Robert Hare, Emeritus Professor of Chemistry in the University of Pennsylvania, died on the 16th of May, ult., at the advanced age of 77. Dr. Hare was one of the greatest chemists of his day, and contributed vastly towards facilitating experiments. He possessed an unusual mechanical turn of mind and readily contrived apparatus for any purpose whatever. He lectured in the University of Pennsylvania nearly thirty years—too long, we think, for his own reputation. We were amongst his latest pupils, but while we admired and revered the great old chemist, we could not think him acceptable as a teacher. Of late years he became a warm advocate of the greatest folly of the day, spiritualism, which was only another evidence of his declining mental powers.

On the 24th April, 1858, Dr. Wm. Gregory, Professor of Chemistry in the University of Edinburgh.

On the 23d April, 1858, in the 63d year of his age, Dr. Robert Harrison, late Professor of Anatomy in Trinity College, Dublin.

At Vienna, of Meningitis, in the prime of his life, Professor Manthner, Director of St. Ann's Hospital.

At Berlin, on 28th April, 1858, Johannes Müller, Professor of Physiology and Anatomy.

Report of Deaths in the City of New Orleans, from 23d of May, to the 25th July, 1858.—Men, 416; women, 158; boys, 381; girls, 344; adults, 574; children, 725; males, 797; females, 502; total, 1299. Under 1 year, 369; 1 to 2 years, 133; 2 to 5 years, 94; 5 to 20 years, 111; 20 to 40 years, 348; 40 to 60 years, 195; over 60 years, 49.

Diseases.—Consumption, 131; still born, 49; cholera infantum, 51; diarrhœa, acute and chronic, 62; scarlet fever, 15; convulsions, infantile, 81; congestive fever, 34; marasmus, infantile, 43; croup, 15; dysentery, acute and chronic, 60; typhoid fever, 23; yellow fever 64.

List of Payments to July 26th, 1858.—Drs. G. A. Nott, $5, vol. 5; J. P. C. Wederstrandt, $5, vol. 5; T. K. Finlay, $5, vol. 5; Curtius, $5, vol. 5; A. P. Champlin, $5, vol. 5; Preston Gibson, $5, vol. 5; J. B. McMillan, $5, vol. 5; Marsh, $5, vol. 5; A. V. Wolverton, $5, vol. 5; J. B. Cottman, $10, vols. 4 and 5; W. B. Erwin, $5, vol. 5; J. B. Morgan, $5, vol. 5; J. J. Gauthreaux, $5, vol. 5; Harney, U. S. A., $5, vol. 5; Bensadon, $10, vol. 3 and 4; J. M. Lewis, $5, vol. 5; Mr. Jas. Syme, $5, vol. 5.

Once more we appeal to our country friends to pay up all they owe us and make a new start. Surely they cannot now plead such a scarcity of money as existed a few months ago.

PAMPHLETS RECEIVED.

"Observations on Malarial Fever." By Joseph Jones, A. M., M. D. From the Author, and a paper well deserving a careful perusal.

"Announcement of the Philadelphia College of Medicine, for the Year 1858."

"ANNUAL CATALOGUE AND ANNOUNCEMENT OF THE ST. LOUIS MEDICAL COLLEGE. SESSION 1857, '58."

"SIXTEENTH ANNUAL CATALOGUE AND ANNOUNCEMENT OF LECTURES IN RUSH MEDICAL COLLEGE, FOR THE SESSION 1858, '59."

"ANNOUNCEMENT OF SHELBY MEDICAL COLLEGE, NASHVILLE, TENNESSEE. SESSION 1858, '59."

EXCERPTA.

ON QUININE IN PUERPERAL FEVER.—*Abstract of Remarks before French Academy of Medicine; by M. Beau.* (*From Am. Med. Monthly.*)—M. Beau, in reply to the request of M. Depaul, that he should make known the results of the treatment pursued at the Cochin hospital, for puerperal fever—that is, by the sulphate of quinine—said substantially as follows:

You know, gentlemen, there are two views in relation to the disease which constitutes the subject of this discussion. Some regard it as an inflammation analagous to peritonitis; others, as an essential fever, a pyrexia, in which the anatomical lesions are only accessory and secondary. M. Depaul adopts the latter, while I include myself among those who accept the former doctrine. Let us see what are the reasons which militate against the one or the other view; but first, let us remark that puerperal fever has been divided into two kinds, by Doublet: 1st, a light, ephemeral fever, continuing only a day or two, the milk fever; 2d, a malignant or severe fever. The discussion is upon this latter form.

The idea of considering puerperal fever as a pyrexia, is very ancient. Bichat, after numerous autopsies, revolutionized the idea of a pyrexia, and caused that of an inflammation to be adopted. Having especial reference to the anatomical lesions, which he most frequently met, he considered puerperal fever to be a peritonitis, and for a long time the affection which we are now discussing was designated only by the name of puerperal peritonitis. The progress of pathological anatomy, however revealed lesions common to phlebitis and lymphangitis; and puerperal peritonitis not being applicable to these different lesions, the old name of puerperal fever, which embraced all, was resumed.

I think that puerperal fever arises from an inflammation, and that this inflammation itself is under the influence of an inflammatory diathesis. I add, that the action of this diathesis, which is common, and so to say, habitual in the puerperal state, is singularly favored, exalted, by the epidemic constitution; another mysterious influence, which combines its action with the preceding, to arouse and develope puerperal inflammations. Diathesis is not a 0 in pathology, it is an x, which is very different. The diatheses are not symptoms in themselves; they are revealed by their manifestations. For instance, the development of tubercles is preceded by the tubercular diathesis, which is not known till tubercles are manifest. The danger, then, does not arise from the diathesis, for when it is present alone, death never ensues; it is the manifestation of it which produces death. The tubercular diathesis is inoffensive, but its manifestation, tubercle, is mortal.

Of all the manifestations of the puerperal diathesis, peritonitis is the most frequent. Ordinarily, it is present alone; sometimes it is accompanied by lymphangitis, phlebitis, etc.

This peritonitis, which is sometimes uterine, sometimes general, may be accompanied by a considerable tympanitis of the intestines, which alone, constitutes a most serious danger, by interfering with the respiration, and consequently with aeration of the blood, by crowding upon the diaphragm. Often a mortal danger results from polypoid concretions, which form in the heart. These concretions, more frequent than they are thought to be, are formed by the buff of the blood, by the fibrin, and give by themselves, to the disease of which I speak, an inflammatory character. This complication, which is not only frequent, but habitual in peritonitis, explains the anxiety and the general disturbance, which the intensity of the peritonitis will not explain. There exist great differences in the symptoms, according as the peritonitis is partial or general. The peritoneum, in all its folds, is an immense membrane, the extent of which is certainly more than double that of the skin. It can be understood that, from a more or less complete inflammation of this membrane, differences in severity may result, which give rise to differences in the nature of the disease. It is the same as in bronchitis, according as a single tube, or all the ramifications of the air passages are invaded. Differences in quality are mistaken for differences in quantity.

M. Depaul and myself agree that puerperal fever is preceded by something, and I have just tried to explain what I understand by diathesis. I hope that the great interval which separates diathesis from pyrexia, admitted by my opponent, will be understood without further explanation.

Let us examine if puerperal fever presents the characteristics of inflammation rather than those of pyrexia. Its debut is brusque, there is a chill and pain in the pelvic region; but pneumonia, which is an inflammation par excellence, commences in the same way.

In pyrexias there are always some eruptions; this is their peculiar characteristic; for example, the eruptive fevers and typhoid fever, which is always accompanied by the lenticular eruption; none exist in puerperal fever.

There are, it is said, some typhoid symptoms in puerperal fever. That is true, but let us not be deceived by this, for typhoid symptoms appear at the last stage of all inflammations which terminate fatally.

Other characteristics, still more convincing, are furnished us by the blood. According to MM. Andral and Gavarret, the fibrin never increases in pyrexias; the contrary is the fact in inflammations. How is it as regards puerperal fever? There are not many observations upon this point. Still I have found a few researches recorded by M. Hersent in his thesis, in 1845. The author, as he himself says, having preconceived ideas, believed it was a pyrexia, and that consequently he should find proofs of the liquidity of the blood; but he found no evidence of this condition in his experiments upon this fluid in puerperal fever.

After having stated that during gestation the fibrin was in greater proportion than in the normal state, M. Beau read different passages from M. Hersent's thesis, showing that in puerperal fever the fibrin was sometimes increased in quantity, but never diminished, even when this fever took the typhoid form. From this it is right to conclude that this affection is an inflammation, and not a pyrexia.

Its contagious character is adduced as an argument against considering puerperal fever as an inflammation. But contagious inflammations are not rare, certain inflammations of the conjunctiva, dysentery, etc., which are evidently phlegmasias, are contagious; and again, in favor of pyrexias, the fact of the general disturbance which produces death in the child, may be adduced. Cannot both individuals be subjected to the influence of the inflammatory diathesis?

The partisans of pyrexia find a difficulty to be overcome, in the fact that they admit two kinds of peritonitis—an inflammatory peritonitis, which no one doubts, and a febrile peritonitis. But how can they be distinguished? I do not know.

And now as regards the treatment. The following is my method: I first administer ipecac, whatever may be the form of disease. Then I give a gramme (fifteen grains) of sulphate of quinine, informing the patient in regard to the effect it will produce, so that they may not be a source of alarm, and then continue this medicament in 10 to 12 grain doses every eight hours.

The next day there will be less heat of the skin, the pulse will be less frequent, the patient will feel better, and the countenance will be less changed.

It is important to ascertain the degree of tolerance of the patient; as soon as the quininism diminishes, the dose should be increased. The sulphate of quinine should be administered several successive days. Sometimes the patients throw it up; another dose should then be given, and when the repugnance to it cannot be overcome, it should be administered by enemata.

This method of treatment is rendered difficult by patients being differently affected by it.

As accessory treatment, I put large blisters over the most painful parts of the abdomen.

Can all forms of puerperal peritonitis be cured by this means? Evidently not. Those inflammations of the peritoneum which are limited, those which can be called sub-umbilical, are cured; the supra-umbilical are not cured. The sub-umbilical cases, complicated with fibrinous concretions of the heart, are not cured. If the name of puerperal fever be reserved for the severe forms, I admit that I do not cure them any oftener than do others.

I am requested to give observations, facts, which demonstrate the efficacy of the method of treatment I have prescribed. These can be found in the thesis of one of my former pupils, M. Barbrau (Paris, 1857,) who zealously followed my researches at the Cochin hospital.

The treatment of puerperal fever by sulphate of quinine is not new. I do not claim any priority for it. I only observe that I have adopted a method which differs essentially from my predecessors, in that I employ the quinine alone, and in large doses; for I repeat that I regard puerperal fever as a disease depending upon an inflammatory diathesis, and the treatment to be applied to it is that which is proper for acute articular rheumatism, and all other affections characterized by an increase of fibrin in the blood.

ELECTRICITY AS AN ANÆSTHETIC.—*By F. Y. Clark, Surgeon Dentist, Savannah, Ga.*—As an anæsthetic, the virtue of this subtle fluid is as yet but little known. Since its introduction (but a few months back,) it has taken rapid strides, and is now fast becoming the harbinger of that much dreaded operation—to which it seems peculiarly adapted—the extraction of teeth.

Whether or no it will apply equally well to other branches of surgery, remains yet to be known; but we can see no good reason why it will not, for the effect produced on the nerve of a tooth must be the same as that on any other nerve of the body.

The principle upon which electricity acts as an anæsthetic agent, is very simple, and what is most surprising about it, is that it has never been thought of, or brought to light before. This, however, may be owing to the many mystified theories, in regard to its action upon the nervous system.

It may be readily ascertained, by connection with a battery, that the members of the body which grasp the poles, are the only parts of the system which receive any perceptible shock. The fluid of course traverses the body, but its intensity becomes less and its quantity more by the number of nerves or conductors over which it has to pass. What I mean may be very clearly and easily illustrated by the simple experiment of connecting one hand with the positive electrode, and then placing the finger of the other hand in a bowl of water highly charged by the negative pole. The finger thus placed, receives the same dead, benumbing sensation that the tooth does in coming in contact with the charged forceps.

We know that, if we make a simple circle by connecting the two electrodes of a battery with a very small wire, and then pass on the current, that the small wire, so placed, will instantly become ignited, because it cannot bear the intensity of the fluid. Thus it is with the finger or any similar member of the body, a current of electricity that would scarcely be felt in the system, would paralyze the finger. Thus from simple facts we can understand how parts of the body can be benumbed or shocked, while in the body itself the cause is scarcely perceptible. It is, then, simply by making immediate use of the shock or paralyzing sensation produced by the charged forceps, that we are enabled to perform the operation of extracting teeth with comparatively little, and in most cases no pain.

Certain animals, as the *Gymonotus Electrus Torpedo, Silurrus Electrus,* and several others, capture their victims, by paralyzing their nerves by electric shocks, from a peculiar battery with which nature has furnished them· Thus it will be seen that these animals have been practising by instinct for generations, what man is just beginning to learn by art.

Thus far we have used electricity in over fifty cases, and although now and then we met with unsatisfactory results, yet, on the whole, we consider it far preferable, and much more applicable to the extraction of teeth, than anything we have tried. Besides, it is certainly better to make use of an agent—particularly in the operation of extracting teeth—that admits the free and full action of our senses, than to inhale a subtle fluid that robs us for the time of all knowledge, and the result of which remains as yet in doubt, even with the best of us.

The mode of using or applying this agent consists in merely establishing a circle by placing one electrode in the hand of the patient, and then connecting the other with the handle of the instrument to be used. The hand of the operator should be protected by a silk glove. It is very necessary in all cases to ascertain what amount each patient will conveniently bear, for what will affect one, will scarcely be felt by another; for this reason all batteries should be constructed so that the current can be easily regulated. We think Sherwood's comes up to the requirements better than any that we have used. We get rid of the fumes of the acid, by having it placed in our laboratory, and wires running therefrom into the operating room.

In order to convey as correct an impression in reference to the use of this agent as possible, we submit the three following cases, which are very similar to all others in which we have used it.

Case I.—Was a young lady who wished the second left Superior Bicuspid removed in order to give room for the remaining teeth to form within the arch. We regulated the current as we thought would suit the system, and then extracted the tooth with apparently no pain. On interrogation, the patient observed that she felt no pain from the removal of the tooth, but that she felt the shock rather severe. No doubt in this case we had on a stronger current than needed.

Case II.—Mr. B. called according to appointment to have five fangs removed in order to make room for artificial teeth. After regulating the current, etc., we commenced the operation, but found on removing the first tooth, that we gave considerable pain. We then made the current much stronger and removed the other four without—as the patient observed—giving the least pain.

Case III.—Was a negro boy sent from the country to have a tooth extracted. As this boy knew nothing about electricity or its use in extracting teeth, we thought it a good case to truly test its effect. We therefore requested him to take a strong hold of the chair arms—around one of which had been previously stwisted one of the poles of the battery—and then extracted the tooth, after which he looked very much bewildered, and on asking him how it felt, he observed in true negro dialect, "I no know how him feel massa, but him no feel like him pulling tooth, him feel more like him foot asleep."

Now, were we to write pages, we do not believe that we could as clearly express the real feeling that this fluid ought to produce in order to use it successfully, as did this boy in the above very expressive negroism.

In conclusion, we might add, that the continued developments of this subtle agent, are truly wonderful. It is but a short time since Franklin brought it captive from the clouds, and although it has now almost become the vital power of every art, it is still veiled in mystery, and no doubt, as has been said by a journalist of this city, "were we better acquainted with its mysterious workings, the cause of the cholera, the plague, and similar diseases would be better known." Again, could we but lift the veil from before that great *bugbear* that has crazed so many brains, peopled so many mad houses, and turned religion to foolery by tipping tables, upsetting chairs, nonsensical rappings, etc., we would no doubt see this same subtle fluid as the motive power, instead of the spirits of another sphere. For the human brain is a better galvanic battery than any that has ever been constructed by man, and could we see its mysterious workings, we would behold innumerable and wonderful telegraphs operating from mind on mind, and from mind on lifeless matter, and thereby understand the mysteries that now confound us.—*Savannah Journal of Medicine.*

TYPHOID FEVER.—*Augusta, Ga., June* 15*th*, 1858.—Dear Sir,—Your request for my treatment of typhoid fever, is before me. The requirements of the case will not permit me to be very brief; yet, on the other hand, there being some limit to an epistolary communication, space may not allow me to particularize with absolute definiteness, so that what I may hereinafter say, you will please regard as *a basis of hints*, rather than a complete and thorough detail of management for this disease.

First: you must understand that this disease is of long continuance, and is also self-limited in duration. That is—start out to deal with it, under the firm and full conviction, that you *cannot cure it*; and this conviction, if honestly maintained, will save the life of many a patient. Let the patient and the disease alone, so long as they appear to agree together very well, without the latter taking advantage of the former by attacking him at any particular vital point, and you will find that generally, the patient will outlive the fever, *unless he started out to die.*

Secondly: should the disease attack with violence a vital organ, as brain, lungs or bowels; or to speak more properly, should these organs not possess sufficient force of vitality to resist the enervating and disorganizing influence of this condition, it will become necessary to fortify them by remedial means directed thereto, also by supporting the general strength, and thus assist the recuperative powers of the system.

Then, should the *brain* become the seat of congestion, you may drive it out, and keep it out, at the same time giving tone to the vessels, by the frequent, though not too severe, application of the *cold douche to the head.* Should the parenchyma of the *lungs* be the seat of engorgement, sufficient to embarrass their function, as manifested by partial or complete occupation of the air passages with mucus—as shown by auscultation and percussion, as well as by the extreme frequency of the respiration—treat the case according to Behier's plan, (see March number of Southern Medical and Surgical Journal,) viz., with *extensive and repeated dry cupping to the chest,* and use *turpentine* to give tone to the mucous tissue of the lungs, and prevent or suppress inordinate secretion or exudation. Should the *mucous membrane of the small intestines* give way, at, or in the vicinity of Peyer's glands, which sometimes happens, though not invariably (as is supposed by some), give turpentine as the best known means of arresting the disorganization of this tissue, and to stimulate its recuperative energy. This can be materially assisted by keeping a small blister—the size of the hand—open, upon the right iliac region.

But if you have not the forbearance and firmness to stand by a case of typhoid fever, which is running its course harmlessly, without interfering to diminish the patient's strength, and with it, his chance of recovery, you had better forego all the reputation you may expect to gain by your professional enterprise in this quarter.

Is typhoid fever, then, a disease *not* requiring the regular attendance of the physician? Far from it. It is one above most of diseases, demanding his especial supervision and untiring watchfulness.

1st. He must guard the patient, that he does nothing himself, and that nothing be done for him or with him, by others, to jeopard his chance of recovery.

2nd. He must see that he is placed and kept in the most favorable circumstances as regards bodily comfort, temperature, ventilation, etc., bodily wants—as diet properly and judiciously ordered, limited and adapted to the stages of the disease; also, as regards the application of stimulants and *when* to be used.

3rd. He is to watch and listen with a sentinel's eye and ear the distant approach of those insiduous complications, to which we have referred, and which are sometimes the result of this disease, in one or more of the vital organs; and begin early to assist the patient's constitution to withstand or overcome them.

The patient with typhoid fever seems to me to be poised with fearful precision between life and death. If he maintains his ticklish position through this protracted crisis, he is safe; but how awful it is, to see what a little influence may jostle him from it. He may not *drop off* suddenly, but that little influence, even *one* injudicious dose of active medicine, in the beginning of the attack, may prove the source of his gradual, but inevitable decline and final *fall.*

Therefore, beware of treating this disease *actively*, even in the beginning. Especially exclude *purgative medicines*, which are detrimental in various ways.

One of the first and the most prominent symptom throughout the disease, is *weakness*, or loss of vital energy. The vitality of the fluids, as well as of the solids, is below par. Depletion increases the impoverishment of the fluids and the approach to necræmia; and from this condition of the blood, probably, results the extreme loss of nervous power in this disease. The patient seems completely enervated: very slight muscular effort seems, often, entirely to overcome him, so that rising up in bed quickens the pulse, and if frequently repeated, will often seriously damage the prospects of the case. Therefore, the mere effort to get up to stool, frequently, should be avoided, and purgatives withheld for this reason, if other more valid objections did not obtain. I have seen a convalescent relapse for two weeks, from walking across the floor to change his bed.

Purgatives given early in typhoid fever are apt to determine to the bowels, in the latter stages, by deranging them and rendering them enfeebled and more susceptible to the influences which manifest themselves in this portion of the organism, as the most common form of complication or organic implication, occurring in this disease. And who can say, but that the bowel affection is in many instances determined by this treatment in the incipiency of the attack. If the alimentary canal requires clearing out, a simple dose of castor oil would probably accomplish it without irritation or depletion. But I would express here, my unfeigned disapprobation of the abominable plan of purging every patient, in every disease, as a necessary preliminary to any other treatment. In typhoid fever, it is productive of positive mischief, by precipitating the result of an existing predisposition, viz.—to intestinal disease; whereas, in paroxysmal fever, often, valuable time is lost in waiting for the operation of active medicines, to the delaying, or entire exclusion of the only important element of treatment.

Thus, you perceive, that as for the treatment of typhoid fever, it is a disease that requires more watching than active interference. But if there is one remedy in the catalogue of the Materia Medica, which seems to be adapted to this condition more than another—if there is anything which seems calculated to relieve the complications which generally arise in this disease, or to prevent them ere they make their appearance— it *does* seem that *that* agent is TURPENTINE.

Dr. Thomas Smith, of London, wrote ninety pages upon the good effects of turpentine, and some were disposed to smile at his credulity; but I rather think he was right.

The turpentine treatment of typhoid fever has been long in vogue. The name of Professor Wood, of Philadelphia, is particularly associated with this treatment, inasmuch as he recommends it highly, and advocates strongly its advantages above all others in certain stages.

I have used this treatment for six or eight years, and whether from its positive advantages, or from the benefit derived from the exclusion of all other medication, there has certainly been the greatest improvement in the success of my practice upon this disease during that period.

It sometimes happens that typhoid fever, in its incipiency, is mixed up with paroxysmal fever. These cases require quinine to rid them of this paroxysmal complication—which it does—leaving the continued fever to pursue the even tenor of its way. I sometimes give quinine in moderate doses, three times a day, and continue its use for a length of time, varying the quantity with the degree of necessity for its continuance—in those cases which have a tendency to congestion, or to passive hemorrhages—

because I believe that quinine acts upon the vascular tissue, to give it tone or contractility; and I have seen many good results from the continued administration of considerable doses of quinine under such circumstances.

The mode in which I have found it convenient to administer turpentine in typhoid fever, is by the following emulsion :

℞ Turpentine,........ ....℥ss.
Loaf Sugar,................
Gum Arabic,.............. } *aa* ℥j.
Comp. Spts. Lavender,.....
Bi Carb. Soda,...........
Camphor Water,.............q. s.
To make an 8 ounce emulsion.

Dose—For an adult, one tablespoonful three times a day (well shaken), and so continue.

To begin with the turpentine early in the attack, seems to lessen the danger to the bowels, and also to the lungs.

I have often seen the bowels, discharging large watery stools before the use of this emulsion, become even constipated under its use, by relieving the local affection. I have continued the emulsion in many cases from my first visit to a patient, until he was discharged.

If there should be much diarrhœa, and the emulsion does not check it, I give 5 grains sub. nit. bismuth in a spoonful of water, after each passage. The most convenient mode of administering the bismuth, I find is in an aqueous mixture, viz :

℞ Sub Nit. Bismuth,.............grs. 80
Water, ...ʒij.
Mix in two ounce phial.

Shake until well mixed at each dose, and give one teasponful after each passage.

If this does not have the desired effect, I add a small quantity of laudanum—from 10 to 20 drops. I dislike the too free use of opiates in typhoid fever : I think they are calculated to bring about coma or delirium in the latter stages.

The bismuth, in connection with the turpentine treatment, will generally check the bowels as promptly, or even more speedily than opiates. When there is diarrhœa, I invariably put a small blister over the right iliac region, and keep it open if possible, until this symptom disappears. I always examine this region, whether there is diarrhœa or not, and if I find tenderness upon deep pressure with the hand, or meteorism, I take it for granted that the diarrhœa is not far off, and apply the blister and put the patient upon the treatment above mentioned.

You will generally find that the comp. spts. lavender, in the emulsion, will prevent *subsultus tendinum* in a great measure; but if this phenomenon should appear, add to the course, a teaspoonful of the tincture of valerian (for an adult,) every three or four hours, until relieved, and repeat *pro re nata.*

I give this tincture also, to quiet restlessness, sleeplessness, and slight delirium, in these cases. Sometimes a Dover's powder (10 grains,) at night is necessary to induce sleep.

In the earlier stages, when the skin is very dry and hot, great benefit may be derived from the thorough application of lard ("bacon-rind") to the surface of the patient's body and limbs. It has a very cooling and soothing effect. This valuable fact I gained from my friend, Dr. P. D'L. Baker of Alabama. The patient may be greased all over, once or twice a day; or, whenever his skin becomes very hot and dry, or he seems very restless; that is, if it can be done without giving him the least fatigue, or causing the least muscular effort: for, recollect, that nothing should be done that requires the patient to rise up or to move. I have a patient now, who got his pulse up very high the other day, by simply fanning the flies off of himself, for a little while. I mention this forcible example to show you, how little muscular effort it would take, either by getting up to stool often, or otherwise, to kill a patient with this disease. Thus, a patient with typhoid fever, should never be allowed even to get up to stool, if it could be avoided; and there exist the best reasons for this, but time and space forbid my entering upon the pathology here. I will, therefore, cite you to my brother's contributions upon this subject, in his volume entitled "Essays on the Secretory and Excito-Secretory System," article, "Typhoid Fever."

If the bowels should remain constipated for too long a time, they should be relieved by warm-water enemata.

At a variable length of time from the beginning of the attack, sometimes ten days, or two or three weeks, the skin will become cool, or even sometimes alarmingly cold. It is sometimes proper to give stimulants before this stage comes on, when, for instance, the adynamic character of the disease is very apparent in the first stage, or if accompanied with passive hemorrhage—i. e. *brandy with quinine:* but stimulants are generally imperatively demanded, when the cold stage has set in. Some cases only require port wine, with which I often combine comp. tincture of cinchona, three times a day, in doses of from one to two ounces of the former, to two to four drachms of the latter for an adult, diminishing in proportion to the age of younger subjects. Other *extreme* cases require brandy almost without limit. It is astonishing sometimes to see what immense quantities of brandy may be poured down a patient in this very low condition, without his system appearing to recognize that it is a stimulant.

The dietetic and regimenial conduct of a case of typhoid fever is as important, and I believe, even more so, than the medicinal course. In the earlier stages the patient does not desire much nourishment—does not require much; and probably it would do him an injury to force him with much. Some light food, such as very fresh milk, well boiled with rice or arrow-root; grated cracker and hyson tea, etc., is as much as he will generally require; and care should be taken that he does not take too much even of these at a time.

In the latter stages, it is necessary not only to use stimulants, but a nourishing diet is demanded, both by the patient's improved appetite, and by his debilitated general condition. Sometimes the appetite is ravenous, and craving the most indigestible and injurious articles, the patient being often difficult to control, and this is a dangerous season on that account. The best form of nourishment under these circumstances is beef essense, and in the absense of this, chicken soup made with rice, seasoned with pepper. The patient should be allowed the free use of cold water—ice water, if practicable—but not to take too large a draught at a time. Sponging the body with cold water in the earlier stages, often affords great relief and improves the condition of the patient. He should never be allowed to lie upon the floor, or in an obscure corner of the room, inconvenient to be got at; but should always be supplied with a bedstead, placed in the most accessible part of the room—that he may be examined critically, in a good light, at pleasure, and without the disadvantages of a constrained position, or other embarrassments to the physician. He should be on a comfortable, soft bed, and should be turned over occasionally, or else he will have bed-sores—one of the most serious inconveniences attendant upon this protracted disease.

Above all things, he should be in a well ventilated room, and not near a fire, either in winter or summer. I believe many negroes die of this disease, from being kept in too close quarters, and exposed to the heat and smoke of a cooking-fire. The temperature of their apartments should be uniform and moderate.

When the patient is convalescent, do not allow him to indulge in any excess of diet, or exercise, for some time after his attack. I have been thus particular in dwelling upon this *minutiæ,* because in attention to these things, which are ordinarily considered minor points, the treatment of typhoid fever principally consists.

Hoping that the hints which I have thus, so hastily and unsystematically, thrown together here, may be of convenience to yourself and benefit to your patients, I have the pleasure to remain,

Very truly, your friend,

ROBERT CAMPBELL.

EXCESSIVE NAUSEA DURING PREGNACY RELIEVED BY CAUTERIZING THE CERVIX UTERI. *By Dr. Parks.*—Dr. Parks remarked that he gave the following abstract, in consequence of a somewhat similar case reported by Dr. Churchill, in the Dublin Quarterly for August last, and which was stated to be, so far as Dr. C. knew, the first instance in which the nausea of pregnancy had been treated by applications to the cervix uteri. Dr. Churchill's case, however, occurred in November or December; that now reported, in January, 1856, and was read at a meeting of the Society for medical observation, before the publication of Dr. C.'s case.

Jan. 17th, 1856.—Mrs. ———, aged about 27, consulted Dr. P. for excessive nausea during pregnancy. She was rather a delicate woman, though active and energetic. At one time, since marriage, she had uterine symptoms, which were referred to inflammation of the uterus by the practitioner under whose care she was, and who treated the patient locally, with the aid of the speculum. The result was successful, so far, at least, as the symptoms were concerned. In other respects she had been well, till the occurrence of a miscarriage, which took place a year or more since, she says, from some accidental cause.

The patient had not menstruated for three months, and had begun to enlarge as if from pregnancy. She had also, for a considerable time, been troubled with nausea, which had become almost constant, and so troublesome as to lead her to seek advice in relation to it. On examination with the speculum, an abrasion was found—marked, but not severe—at the os tincæ, which was cauterized with nitrate of silver. Two days after, the patient stated that the nausea was greatly alleviated immediately after the operation, and on the day following had almost entirely ceased, now only existing to a slight extent. A mixture, containing lavender, gentian and ginger, was prescribed. The patient went her full time comfortably, and had a safe delivery.

Dr. Storer mentioned two cases of this affection, which were relieved by the application to the cervix uteri of the saturated tincture of iodine. He had also tried the tincture of iodine internally, in doses of three drops, and found it even more effectual than creosote.—*Ext. Bos. Soc. for Med. Imp.*

ON THE VALUE AND ON THE INDICATIONS OF PERCHLORIDE OF IRON ADMINISTERED INTERNALLY IN THE TREATMENT OF ERYSIPELAS. (*Bulletin Géneral de Thérapeutique.*)—The use of perchloride of iron in the treatment of erysipelas has lately been brought again into notice by the publication of a thesis by M. Louis Mathey, and by some observations made by M. Aran, physician of the Hopital St. Antoine, in Paris. M. Mathey relates ten cases of erysipelas treated with this medicine, and his conclusions are contained in the following remarks:

The action of perchloride of iron on erisypelas is evident, and the course of the disease is modified a short time after its administration. In fact, on the second day, and sometimes even on the first, M. Mathey has seen the disease become limited and circumscribed, and its further progress arrested. As to the duration of the disease, the effect of the perchloride is still very remarkable: not only is the progress of the erysipelas sensibly modified from the first few hours which follow the administration of the medicine, but it is completely arrested; the radical cure of the disease is obtained in a very short time. It was observed that in ten rather severe cases of erysipelas, treated by the internal use of perchloride of iron, three were cured in two days, three were cured in three days, two were cured in four days, one in five days, and one in seven days. It cannot therefore be denied that erysipelas is advantageously modified by the internal use of chloride of iron; that the cessation of the symptoms proper to erysipelas, is sometimes very rapid after the administration of this medicine; that in a series of ten observations, made upon varied cases, this treatment never failed; that even where its efficacy may be doubted, it has never given rise to any bad symptom; and that when administered in the dose of thirty drops to a healthy subject, it has never given rise to any painful sensation, and has never produced any notable functional disturbance.—*Med. Chir. Review.*

REPORT ON CHEMISTRY AND MEDICAL JURISPRUDENCE.

By I. L. CRAWCOUR, M. D., Prof. of Chemistry and Med. Jurisprudence, New Orleans School of Medicine

ON CURARE: BY BAUDRIMONT.

The curare or woorara is one of those mysterious and terrible poisons whose action although rapidly fatal, when it enters the circulation, becomes innocuous when introduced into the economy through the stomach. It is this mode of action, which resembling that of venomous serpents, raises doubts as to its being the production of a vegetable, it having been stated that it is prepared from the juices of various plants of the order strychnos. All vegetable poisons, as is well known, act both by inoculation and when introduced into the stomach, the curare would be the sole exception, an exception inexplicable both on the score of its vegetable production as much as from its chemical composition. According to Humboldt, the Indians of the Orinoco, prepared this poison from the bark of a certain liana, which they call *Vejuco de Macarure.* By crushing a juice is extracted from this bark, which is filtered and subsequently concentrated by heat. As soon as the liquor reaches a syrupy consistence, another very glutinous vegetable juice is added to it, this is not venomous, but adds consistency to the other ingredients. The Indians use it in this form to poison their arrows. According to Humboldt therefore, the curare is purely vegetable, but more recently M. Goudon has discovered that the Indians of Messaya add to the juice of the plant, some drops of poison procured from the most venomous serpents. The latest investigator is M. Terral, and he derives his information from persons who had lived among the Indians of Brazil. The following is his account of the mode of preparation: A horse or buffalo having been killed is suspended in some place known to be the resort of venomous reptiles, which speedily attracted by the scent, bite frequently and persistently the prey, which is too large however, to form an article of food for them. In three or four days the carcase is saturated with poison, it is then removed and placed in large vases, and allowed to completely putrefy; it is then boiled in water, and when concentrated, the juice of the *Vejuco de Macarurė* is added to it. The liquid is then separated by pressure from the solid residue concentrated and placed in little gourds, which the Indians wear suspended at their waist. If this is really the mode of preparation, we can readily understand the special action of this terrible poison. It is difficult however to comprehend, how a set of savages, could have acquired the idea of concentrating in one product all the requirements for every form of poisoning, viz: vegetable poison, virus of putrefaction, and the venom of snakes, thus leaving behind at an immense distance the inventive powers of a Borgia, a Locusta, or a Brinvilliers.

CARBONIC ACID AS AN ANÆSTHETIC: BY HERPIN.

The anæsthetic effects both of carbonic acid and of carbonic oxide on man are incontestable, but for their use as medicinal agents certain conditions are necessary which must be taken into consideration.

Carbonic acid, pure or mixed with an equal volume of air is irrespirable, and produces a convulsive closure of the glottis and a feeling of suffocation; when placed in contact with the schneiderian membrane it produces a sense of stinging similar to ammonia; also, if applied to the eye, it induces so vivid an impression of heat and burning, that few can bear its application for more than a few seconds.

We deduce, therefore, that carbonic acid should be inhaled only when mixed with a large proportion of atmospheric air, and that all contact with the eyes and nostrils should be strenuously avoided. From experiments made in the Grotto del Cane at Naples, although the carbonic acid was diluted with much air and nitrogen, the dogs who had served once, invariably resisted when attempts were made to lead them again into the Grotto, and while in, it was necessary to hold them until consciousness had passed off. As soon as they recovered they fled with all their speed from the spot. The impression, therefore, that these animals experienced must have been a painful one, and consequently no such mixture could be employed to produce anæsthesia in man.

The effect of carbonic acid upon the higher animals differs, according to whether the action is produced primarily on the respiratory organs or on the brain and nerves. When the gas is pure or in large excess, suffocation ensues very rapidly; respiration is accompanied by crepitations, and convulsions; the mouth foams and the tongue is frequently bitten, from the convulsive movements of the jaws. Involuntary evacuations occur, the jugular veins become gorged with blood, the face is swollen and sometimes rupture of a small blood vessel occurs. As a rule there is an expression of extreme pain and suffering. In these cases, assistance, even if very prompt, is generally useless, death ensuing rapidly. After death, we find the lungs distended and of a violet color, both cavities of the heart, especially the right, filled with blood, but the vessels of the brain scarcely injected.

When, however, carbonic acid is mixed with a large quantity of atmospheric air, say eighty or ninety per cent., the results are very different. Unconsciousness occurs without suffocation, without pain and without any apparent disturbance, the action of the gas is confined chiefly to the brain and nervous system. The patient suffers at first from confusion and singing in the ears, soon succeeded by a dreamy and almost cataleptic condition. The pulse, at first quickened, is diminished in force and frequency; the palpitations of the heart become more and more feeble; respiration becomes infrequent and almost imperceptible. Insensibility and anæsthesia occur gradually in a more or less complete manner; but the features present no change and appear as in an agreeable and profound sleep.

In this condition, a person may be easily restored to life, even after a prolonged apparent death. After death, the vessels of the brain are much injected, the lungs are somewhat collapsed and pale, the cavities of the heart are almost empty.

In some experiments made at Pyrmont, a young man, who was engaged as an assistant, remained a little too long in the mephitic atmosphere, was found deprived of consciousness; pulseless, pupils dilated, and the cornea very brilliant; respiration very weak and at long intervals. After having been exposed to a free current of air for some time, well rubbed, and water thrown over the head, some slight convulsive movements occurred, he took a deep inspiration, and appeared to wake, as out of a profound sleep. He complained of a painful sensation in the head, but none in the chest. His walk was feeble and staggering, and it was not until the next morning that he was entirely recovered. The following is the account he gave of his sensations. "As soon as I lowered my head in order to pick up the instruments," said he, "I felt an agreeable sense of heat penetrate my whole body. I felt perfectly comfortable, although my head felt a little dizzy. I heard myself called, but the voices seemed to come from a distance, and they became fainter and fainter; while at the same time I heard the most exquisite music coming from afar and approaching me, and lights of the most brilliant colors danced before my eyes. The desire I had to walk and approach the voice which called me, was as it were chained, and I was withheld by an incomprehensible power, to which, however, I willingly submitted, being already half stupefied. From this moment till I awoke on the turf, I recollect nothing."

On being asked if he suffered any pain in the chest, he said, on the contrary, the loss of consciousness had been accompanied by a most agreeable sensation; but that his waking had been disagreeable, from the weak condition in which he found himself.

From these observations, it follows, therefore, that the first effects of the gas are produced on the brain and nerves of sense, and secondly upon those of motion; the power of the will over the muscular movements is first arrested, then entirely suspended; the limbs become incapable of motion, and remain in the position in which they first fell. There is an absolute loss of consciousness, but frequently the functions of organic life, and particularly those of respiration, continue their action, though feebly. Carbonic acid acts more promptly and energetically in sensitive persons, in whom the chest is large in proportion to the rest of the body, as in women and children. It also frequently induces premature labor or miscarriage in pregnant women. With regard to its application as a therapeutical agent, it is, probably, better to produce anæsthesia by chloroform, and then to continue it by dilute carbonic acid, in the proportion of ten parts to 100 of air. In this manner the danger of the continued employment of chloroform would be obviated, as the strength of the mixture of carbonic acid and air could be graduated at will, and the patient be kept insensible for an unlimited period, without danger.

TAPPING IN OVARIAN DROPSY.—*By R, McDaniel, M. D., of Laurens District, S. C.*—Mrs. B., 34 years of age—of strong constitution—the mother of seven children—was attacked with ovarian dropsy in 1841; died the 4th of January, 1858, making sixteen years from the commencement of her disease to the termination.

Mrs. B. was treated by several experienced physicians, for upwards of two years. The accumulation, still increasing, and the disease becoming more alarming, a consultation was called November 10th, 1843, and the operation of tapping was performed, for the first time, by Dr. R. C., with the evacuation of thirteen quarts of water. From that time to the termination of the case, she was tapped two hundred and nineteen times, with the evacuation of four hundred and ninety-five gallons of water.

Dr. R. C. continued to perform the operation of tapping, for a number of times, when the instrument was given up to the Rev. Mr. B., her husband, a very intelligent man, who performed the operations up to the time of her death, and who kept a correct statement of the number of times that she was tapped, and the amount of fluid evacuated at each operation.

Mrs. B. gave birth to two daughters during her illness—the first one, March 22nd, 1843, which lived and did well; the second, was born June 19th, 1845, which lived a few hours, and expired; the operation being performed twelve times up to this date.

Four years previous to her death a tumour made its descent into the vagina, filling the whole vagina, which interfered very much with the convenience of the patient in a setting position. A physician was called in—the tumor, or sack, was pierced, with the evacuation of eight quarts of water, to the great alleviation of the patient. During her illness, she was tapped twenty-nine times, per vaginum, yielding from five to twelve quarts of water at an operation.

For the last few years, being the family physician, and being called to see Mrs. B., who suffered much in the latter period of her disease, I found that, after the operation of tapping, inflammation of the sack had ensued several times, but was combated, successfully, by a strict antiphlogistic treatment—inflammation ensuing again after the two hundred and eighteenth operation, proved rapidly fatal.

At the earnest solicitation of the patient, she was tapped a few hours before she expired.

I report this case to the Profession, on account of the frequent number of tappings, and the aggregate amount of water that was evacuted. As far as my knowledge extends, such a case is rare, if it does not exceed any on record.—*Southern Medical and Surgical Journal.*

ON OBSTINATE VOMITING.—Various means have been proposed against vomiting during pregnancy, in hesteria and other circumstances. Several French physicians have lately tried with success, in many cases, some remedies either too neglected or quite new. During pregnancy vomiting has been stopped by the use of a portion of tincture of iodine (10 drops), iodide of potassium (10 grains), and 120 grammes (4 ounces) of water and syrup. This prescription has been employed by M. Becquerel. A physician of the south of France, M. Bacarisse, has made use of iodide of potassium alone, which he thinks to be more successful than tincture of iodine. Professor Buission, of Montpelier, has tried three remedies: tincture of iodine with alcohol (a remedy much spoken of in Germany); iodide of potassium alone, and with tincture of iodine. With the first of these three medicines he has seen an increase of the vomiting; with the second, vomiting has hardly diminished; with the third it has completely ceased. He has made this trial three times. Dr. Martin Magron, of Paris, has employed successfully dry cupping on the spine, and in a case of obstinate hysterical vomiting, he has succeeded at every fit in stopping the vomiting by this mode of treatment. In a case of paraplegia, with convulsive fits and vomiting, he has succeeded a great many times, in stopping at the same time the vomiting and the fit, by cupping applied on the spine and slight bleeding.—*Virginia Med. Journal.*

DURATION OF LIFE AMONG THE JEWS.—According to the observations of E. Gatters, the duration of life among the Jews is considerably longer than with Christians; even in infancy the mortality of the former is relatively less than among the latter. From his calculations it results that the average length of life is—for Israelites, 46.5 years; for Germans, 26.7; for the Croats, 20.2; for the Austrians, 27.5. Gatters attributes this superiority on the part of the Jews, in different climates, entirely to the influence of race, and suggests the advantage of paying attention to the ethnographic element in the etiology of diseases. It is very probable that the cause of the greater longevity of the Jews over Christians does not depend wholly on race, as Gatters thinks, but especially, if not entirely, on the fact that the Jews are more wealthy than Christians, and that their hygiene is superior to that of the latter.—*Brown-Sequard's Jour. of Phys.*

[The Jews in America cannot certainly be considered as superior to Christians in their hygienic condition; they are, in fact, far below the latter in this respect. We have no means of ascertaining the comparative longevity of the two races in this country.—EDITORS.]

YELLOW FEVER.—Dr. G. Van Arcken, of Port au Prince, contributes a valuable article on this disease in that place, to the May No. of the *Am. Medical Monthly.* According to his views, yellow fever may assume all "grades of violence, from a slight indisposition to the most fatal form," owing to the difference in mode of living, constitution, or peculiar susceptibility. There is generally a chill ushering in the attack, the duration and violence of which, often, is an indicatian of the character, etc., of the disease. Thus a complete rigor ushers in a slight and easily managed case; slight chillness, or none at all, portends a dangerous attack. Constant symptoms are "deep-seated and severe pain in the supra-orbital region, and weakness in the back and limbs, amounting sometimes to a spasmodic pain." When the fever has set in, the skin is hot and dry, respiration hurried, flushed face, red and watery eyes, tongue thickly coated, and sore throat occasionally. Generally, more or less vomiting supervenes, of a bilious character. In the more fatal cases, the gastric complication comes on later, and sometimes as black vomit. Sometimes, there is weight and oppression at the epigastrium, with pain on pressure. Bowels costive, and when moved, offensive. Mind disturbed, restlessness, and often delirium, giving way to deep coma.

These symptoms may continue from two to four days, and then the patient seems much better; "pulse natural, respiration calm," and pains almost gone. This, however, is temporary; the system has only ceased to struggle against the disease. By making pressure at this stage, over the epigastrium, we find great tenderness, and a lump pulsating. The eye changes from red to a yellow or orange color. The forehead and cheeks assume a pale violet or bluish appearance, from the stagnation of the blood. In a few hours "the pulse becomes frequent and irregular," with marked intermissions, the tongue brown, dry, with red edges, and sordes appear on the teeth and gums. Now the stomach rejects everything, and also ejects "a matter consisting of black flakes and particles, floating in a colorless or slightly yellow fluid, which ultimately becomes opaque and black."

The urine is scanty or suppressed, hemorrhage occurs from the mucous membranes, "gloomy indifference" takes the place of the restlessness, pulse ceases, hiccough sets in, the skin becomes cold and clammy, an offensive odor is exhaled, and death closes the scene.

In slight cases, after the fever passes off, the stomach gradually improves, perspiration is free, and the urine is dark brown, and convalescence takes place.

Of course, in various cases, we find symptoms of great diversity, and in very malignant attacks we have every symptom much increased in intensity. The peculiar vomit generally occurs about the third or fourth day, but fatal cases may occur without it. Dr. Van Arcken considers that it is entirely a distinct disease from either bilious or typhus, and that there is no reason to believe in the contagiousness of the disease.

The most unfavorable symptoms are "excruciating pains in the back, limbs and forehead, great feebleness of pulse, sometimes an entire absence, a bronze color of the face, bloodshot eyes, together with extreme restlessness, epigastric tenderness, black vomit, and suppression of the urine."

"The favorable symptoms are, first, the absence of any of the above mentioned; then a prolonged continuance of the primary fever, which subsides only gradually, and under the effect of a general diaphoresis, the passing dark-brown urine, salivation, a gradual clearing of the tongue, and a diminution of thirst, and epigastric tenderness."

The position of Dr. Van Arcken, as physician to the American Consulate, brought under his care all cases occurring to the crews of foreign vessels. These were generally robust, able-bodied men, from twenty to thirty-five years of age, and about half Americans.

When called to a case, he first decided whether it was inflammatory or typhoid. The former presented a full, strong pulse, and the usual appearances of high excitement. The latter, feeble and frequent pulse, etc.

In an inflammatory case he administers thirty grains of calomel, placed dry on the tongue, and allows a little water to swallow it. As a mercurial action is desired, he gives, after this, two grains every two hours for two days. As it is difficult to produce this action, from the want of absorption in yellow fever, he ceases, after forty-eight hours, to attempt it. He allows acid drinks, and if costive, purgative clysters. Next, having had several stools, the patient feels relieved, and if there is any tenderness at the epigastrium, leeches are applied around the anus, a tepid bath is given, and an emollient poultice applied to the stomach.

The patient goes on well, and next morning takes ten grains each of calomel and quinine. On the fourth day, ten grains of quinine with one grain of opium is given, to produce diaphoresis and sleep, and the action of the skin is kept up by means of liq. ammoniæ acetat. Next day weak soup is allowed, and occasionally, to keep the bowels free, citrate of magnesia. In eight or ten days he is able to be at his business.

Sometimes, when the progress is more unfavorable, on the third day the tenderness of the epigastrium is great and vomiting threatens, then he applies six to ten wet cups to the part, and if it relieves, go on as before; if not, blisters to the inside of the thigh. These often act like a charm, but they must be applied before black vomit has set in. If this has occurred, they seem to exasperate rather than benefit.

Typhoid cases require a different treatment, as tonics, to assist the system to rally from the onset of the attack. Here, he uses sulphate of quinia, and in *very large doses.* "If the stomach is still quiet, give immediately thirty grains in about half a dozen spoonfuls of coffee." If the stomach will not permit it, then he dissolves sixty grains in an ounce of ether, with two drachms of liquor ammoniæ, and rubs this under the armpits and over the abdomen several times daily.

To combat the black vomit, after trying without success every remedy so often recommended, he used *corrosive sublimate*, with the recovery of *seven patients.* He dissolves six grains of it in two ounces of water, with twelve drops of muriatic acid, and gives it in teaspoonful doses, first clearing out the stomach with a mild emetic. He repeats this every two or three hours, according to the symptoms, using poultices to the stomach for the slight burning it produces. If useful, it usually is so, after three or four doses; the vomitings are altered in color, and finally the matter passes off by the bowels.

When this does not yield to remedies, it remains from two to four days, and then ceases spontaneously, and the typhoid stage ensues, followed by convulsions and death.

Often the patient lies without pain or any symptom, but so weak as to be perfectly helpless. Now, a nourishing diet should be given, with stimulants, as brandy, tinc. of musk, etc. Sulphate of quinia may be used with good effect, and frictions to the limbs for the pains. Thus, the patient may remain for fifteen or twenty days, but each day brings additional hope.

In cases where it is difficult to diagnose between the inflammatory or typhoid state, we should be watchful, as these are the most dangerous; quinine should be given in large doses, and the most active treatment employed.

A singular fact he noticed, was, that five out of seven who recovered after the black vomit were Americans, and only fourteen out of fifty-two deaths were of American birth. Cabin boys are peculiarly unfavorable subjects, and the only one he brought through was an American cabin boy.—*Phila. Med. and Surg. Reporter.*

ACTION OF COD-LIVER OIL, AND THE SUBSTITUTION OF ORDINARY FATS IN FOOD.—Dr. Edward Smith, in an interesting paper (*Edin. Med. Journ.*, Jan., 1858), urges the profession to reconsider the question of the administration of cod-liver oil, with a view to determine if the good which it effects cannot be brought about by ordinary articles of food.

The following are his deductions in reference to the effect of the administration of the oil in phthisis :—

"1. It does not remove the disease.

"2. The cases of arrest of the disease are very few.

"3. Commonly the disease progresses.

"4. In about a half the progress is lessened.

"5. The patient may report himself both stouter and stronger, and yet the disease silently progress.

"6. When it disagrees, it is chiefly from its influence upon the digestive organs.

"7. When it agrees, it is chiefly by improving nutrition; but in an appreciable number of cases it is believed by the patient to have a local influence, more or less apart from the state of general nutrition.

"8. This local influence is most important in the pharynx and other parts of the mucous tract, and there results from the direct application of the oil to the mucous surface.

"9. It acts almost exclusively by its fatty elements supplying a nutritive material of which the system has been in defect.

"10. In this respect, it has no advantage over the fats used in food, and may be rejected by the appetite like any other fat.

"11. There is a large class of cases in which it is not beneficial, and it is our duty to administer it with discrimination. As a rule, it is more commonly beneficial in those cases in which there is an appetite for the ordinary kinds of fat, and is rejected when ordinary fats are rejected; and hence the substitution of ordinary fats for the oil in equal quantities is not difficult.

"12. Upon the well known and beneficial principle of separating food from physic, it is our duty to supply this desideratum as an ordinary article of diet, to be selected from those which are commonly used in food, and to instruct the patient that we administer it as food.

"13. Although we may thus supply an important element in nutrition, and thereby avert many evils, we do not touch the essence of the disease, or do anything which shall necessarily remove the deposition which maintains the disease and tends to its further progress; and, in fact, we leave the disease much as we found it. It is not, therefore, a specific remedy, but an adjunct to treatment; and the great temporary good which it has so frequently effected proves the great importance of fat in the animal economy.

"My own practice may be readily inferred from these statements. If I desire to produce a local effect upon the throat, I often employ the cod-liver oil, either alone or in an emulsion, giving it in small quantities, and directing that it be swallowed slowly. If I have reason to believe that the patient has diminished the quantity of fat, either with or without other kinds of food, I direct him to increase it; but I do it with moderation, and by substituting a form of fat which he likes for one which he does not like, to take a moderate quantity of some kind of fat with every meal. An increase in the quantity of butter, and also of suet, when added to rice or other puddings, or when boiled in milk, is that which may be the most easily effected; but if the quantity directed is unreasonable, as for example, the common plan of boiling half an ounce of suet in half a pint of milk, it often disgusts the patient, defeats the object, disturbs the digestion, and is waisted by the bowel. I recollect an instance in which having asked an Irishman the usual questions about fat meat, butter, suet milk, and bacon, and directed him to increase the quantity of all of them, he mixed them altogether and took them, to his great delight and benefit.

"In the foregoing paper, I have not discussed the mode by which fats exercise so beneficial an influence over the system, but have assumed that this benefit is admitted. I purpose to defer this inquiry, and consequently, the consideration of the relative value of various other oils and of cod-liver oil. I may, however, state that the patients' objections to many oils, viz., the flavor and the ill effect upon the bowels, do not, in any degree, apply to the ordinary fats of food, for the simple fact that they are such implies that the flavor and general effect are commonly good and agreeable. The best contrast is, therefore, not that of cod-liver with other medicinal oils, but with such ordinary fats in food as nature has provided for us and made agreeable to us; and it is to this contrast alone that I have desired to direct attention.

"The chest diseases, other than phthisis, in which I have found cod-liver oil beneficial, are some states of chronic bronchitis and various pharyngeal affections, of a more or less acute form. In these cases, it produces its immediate action upon the mucous membrane of the pharynx, and, if properly administered, certainly relieves the cough, with the expectoration and its train of evils. In the latter class of diseases, which often closely simulate phthisis, its influence is often exceedingly marked, and impresses the mind very deeply with the importance of the condition of the throat, in reference directly to the production of cough, expectoration, and slight hæmoptysis, and indirectly to the degree of irritability of the general system. Indeed, I have no doubt that the most serious disease often originates, or first manifests itself, in that locality."

PUERPERAL CONVULSIONS.—Dr. James M. Newman presented a report on this subject to the Buffalo Medical Association, at a late meeting, which is published in full in the Buffalo Medical Journal, comprising some account of 33 cases collected from various sources. Of these 17 were primiparæ, 9 multiparæ, 7 not stated. Recovered, 24; died, 9. Described as anasarcous, 7. The urine was albuminous in 12 cases; not albuminous in 2; dark colored in 2; no secretion of urine in 2; and condition not noted in 19. Ether or chloroform was employed after bloodletting in 19 cases—and of these, 16 recovered and 3 died; employed without bloodletting, 9—and of these, 6 recovered and 3 died. Convulsive movements modified and controlled by anæsthetics, in 23 cases; convulsions not diminished by them, in 6 cases. Two cases proved fatal in which chloroform was administered without previous treatment being indicated.—*Boston Med. and Surg. Journal.*

NEW FEBRIFUGE.—*Azederach Bark.*—Dr. W. R. Cornish, of the Bengal army, (*Indian Annals of Med. Science,* Oct., 1856,) states that the Margosa or Neem tree has long enjoyed a considerable reputation in India as a febrifuge, and that it belongs to the natural order Meliaceæ and genius "*Azadirachta.*" It is probably the *Melia Azederach* of our Pharmacopœia, and which under the name of Pride of India is so extensively employed as a shade and ornamental tree in the southern States. Here the bark is used as a vermifuge. Dr. C. describes the bark as being nearly white internally and purple externally. When chewed, in its recent state, its taste is at first sweetish, followed quickly by a powerful and lasting bitter. Dr. C. employed the bark during six months with nearly all the fever patients that came under his care, in the form of decoction, (the strength of which is not given,) from one to two wine-glassfuls being administered repeatedly before the accession of the paroxysm. Dr. Cornish considers that the active principal resides more especially in the light colored inner bark, the dark exterior being highly astringent, owing to a tannin analogous to that in catechu.—*American Jour. of Pharmacy.*

Two Superfluous Ears.—A little girl, recently under Mr. Birkett's care in Guy's Hospital, might have more than answered to Macbeth's requisition, "Had I three ears, I'd hear thee!" since she possessed no fewer than four aural appendages. The two superfluous ones were situated on the sides of the neck, somewhat lower than the angles of the jaw, and were well developed, as far as regards their external contour and the possession of fibro-cartilage. They had, of course, no deeper structures, and merely grew from the skin. Mr. Birkett had had the child under observation almost from the time of birth.

In connection with these examples of congenital deviations from natural formation, we may mention that the curious in these matters may have an opportunity of seeing a case in which one lower extremity is nearly two inches longer than its fellow, by looking into Mr. Birkett's wards. The muscles, in this case, appear proportionately developed to the extra length of the bones.—*Med. Times and Gaz.*, May 22, 1858.

Perils of Hydropathy.—There are few more dangerous men in society than he who prides himself upon his sound common sense. Every one has met this man, and knows his characteristics well. He has no book-learning, and is inclined to be thankful that he has not; he is none of your recondite book-worms, full of cranks and nonsense,—he is a thoroughly common sense man. And so, without any special knowledge upon any subject, he thinks himself qualified to decide upon all. He makes his own will in a plain and straight forward way, which involves his heirs in an endless chancery suit. He does everything in his own private and sensible fashion, and being always "open to conviction," ten to one but he falls a victim to the first plausible quack whom he meets. He eschews the mysteries of medicine, and laughs at the carefully wrought theories of the treatment of disease. He trusts to the light of common sense, and adventures by its aid to grope obscurely amongst the complicated ropes and pulleys by which man's frame is guided, and to tamper with the delicate machinery, with about as much success as an ignorant landlubber might have in adventuring to handle a ships's ropes in a storm, reefing when he should furl, and hoisting sail when he should scud with bare poles. Common-sense men delight in acting as their own physician; and this to be sure they have a right to do; and, if they choose, to poison themselves with lobelia, salivate themselves with "vegetable" mercury, or line their intestines with antiseptic charcoal. We should be very little inclined to dispute this, their undoubted privilege. But they are commonly unwilling to confine the benefits of their common sense to their own proper person, and, in their anxiety to extend them to other less sensible fellows, they bring themselves within the range of sharp criticism. It is thus that we find ourselves called upon to concur in a vigorous rebuke inflicted by an inquest jury at Woolwich, upon a Mr. Clark, a dissenting minister. This gentleman, considering hydropathy to be based upon reasonable principles, and to be a more "sensible system" of medication than that recommended by the great men who have devoted their lives to the study of medicine, proceeded to apply his principles to the friendly treatment of an unfortunate man, afflicted with pneumonia, pleurisy, and pericarditis. He immersed his victim in cold water, and swathed him in wet bandages, at the time that he was undergoing treatment by calomel. Gross deception was practised upon the qualified attendant. The patient sank rapidly, and Mr. Clark, finding things looked badly for him, beat a hasty but untimely retreat. It was too late to save the patient—if at any time he could have been saved. The jury included in the blame Mr. Clark and the relatives; they greatly censured the blind assurance which could presume to meddle in a matter of life and death, the dangerous ignorance which could so mischievously blunder, and the unworthy deception which could counsel concealment from the attending surgeon. It is hard to say how many lives are snuffed out by sheer ignorance and stupidity. We chronicle but a tithe, and yet our gathering is abundant. Our sheaves overflow with fulness, and the sprouting crop of quackery almost defies the edge of the scythe.—*London Lancet, July,* 1858.

DR. BROWN-SÉQUARD'S LECTURES.—Dr. Brown-Séquard has concluded his course of lectures, at the Royal College of Surgeons, amidst the enthusiastic acclamations of a crowded scientific audience. The interest created by these extraordinary discourses continued to increase up to the last moment of their delivery.

It is in the highest degree gratifying to introduce to our readers the productions of so distinguished a physiologist; indeed, the author's name is a sufficient guarantee for their excellence. But lest some should hesitate to attach full weight to his experience, through not being acquainted with the long and arduous course of patient investigation that led Dr. Brown-Séquard to his brilliant discoveries, we take this opportunity of briefly sketching his scientific career.

Born a British subject, in an English colony, of an American father, more than fourteen years have elapsed since this gentleman entered upon a series of experimental researches on various animals, with a view to ascertain the truth on many of the more obscure points of physiology and pathology. Commenced in Paris, these investigations were subsequently carried on in the Mauritius and in the United States, and they have been prosecuted with little intermission up to the present time. The results have been from time to time given to the world in communications to the French Academy of Sciences, in the periodicals of the day, or in lectures delivered in Paris, in the United States, at St. Bartholomew's Hospital, and in the course just now concluded at the College of Surgeons.

The French Academy of Sciences have fully acknowledged their sense of the value of Dr. Brown-Séquard's labors, by presenting him with prizes and with honorary and pecuniary rewards, most of them for his researches on the nervous system, as well as for his investigations on the blood. Nor is a just appreciation of Dr. Brown-Séquard's merits confined to the scientific circles of Paris. In America he has lectured to large and distinguished audiences of physicians and men of science, and has received most substantial proofs of appreciation from more than one institution in that country. In England, the Royal Society has, on two different occasions, voted to him a portion of the government grant of money, entrusted to them to distribute in aid of scientific research. In 1856, Dr. Brown-Séquard received a tenth of the whole sum at their disposal, and in the succeeding year a considerable amount was awarded to him in furtherance of his eminently practical investigations. No greater compliment could be paid to the discrimination of the Council of the Royal Society than is now offered in the interest exhibited by the medical and scientific public in Dr. Brown-Séquard's lectures. At these may be seen men in the highest rank of their profession and in active practice, who snatch an hour from their too short day to listen to a lecture, in which they cannot fail to hear something of direct practical interest. There, too, may be found the most accomplished physiologists of the day, content to be taught by one whose ingenuity and dexterity *all* cannot but admire, and whose opinions and trustworthy observations *they* have learnt by experience to respect.—*Ibid.*

[The interesting researches of Dr. Addison on the Pathology of the Supra-Renal Capsules, and the so-called Bronze Disease, which was supposed to be dependent upon the disease of these obscure little bodies, have given rise to much investigation on this subject. Among others, Dr. Harley, of London, has given in the April number of the Medico-Chirurgical Review an interesting paper on this subject which represents the present state of the science. We shall present to our readers a few extracts which resume the opinions of the author.—EDS.]

AN EXPERIMENTAL INQUIRY INTO THE FUNCTION OF THE SUPRA-RENAL CAPSULES, AND THEIR SUPPOSED CONNECTION WITH BRONZED SKIN—*By George Harley, M. D., F. C. S.*, of University College, London.—*Pathology of the Supra-Renal Capsules.*—Comparatively little attention has been paid to the pathology of the supra-renal capsules. All that we as yet know is, that they, like other organs, are liable to a variety of organic changes, such as cancerous, calcareous,, and tuberculous deposits, as well as fatty and fibrous degeneration. With regard to their functional derangements we know absolutely nothing. As it happens that at the present moment the question of the connection existing between bronzed skin and supra-renal capsular disease is agitating the whole medical world, both here and abroad, and as I consider it still *sub judice* whether or not bronzing of the skin ought to be regarded as a pathognomonic symptom of diseased supra renal capsules, it may perhaps be advisable for me to add a few remarks upon the subject.

Since the publication of Dr. Addison's interesting monograph 'On the Constitutional and Local Effects of Disease of the Supra-renal Capsules,' many cases similar to those he described have been reported, both in home and foreign journals. At the present moment the facts of the case seem to be the following:—A certain number of cases have been observed where bronzing of the skin and disease of the supra-renal capsules have been found associated together, and from that it has been concluded by some that the one was dependent on the other state. Now, let me ask if the mere fact of supra-renal capsular disease and bronzing of the skin being found associated together, is sufficient to justify us in regarding the latter as a pathognomonic symptom of the former? Add to these textural changes the functional symptoms of anæmia, debility, languor, feebleness of the heart's action, and irritability of the stomach, and have we then incontestable evidence that bronzing of the skin so accompanied is the result of supra-renal capsular disease? Do we require no further data to be laid before us ere we pronounce judgment? Surely every one will agree that this is but one side of the question, and that before we can decide it, the other must also be examined.

In order to consider the subject properly, it will be necessary for us to proceed in a categorical manner.

The principal points to be ascertained are—Firstly, Is supra-renal capsular disease invariably accompanied by bronzed skin? and, secondly, is bronzed skin always associated with disease of these organs?

At the time when Dr. Addison published his monograph, as well as in Ootober 1856, when a review of it appeared in this Journal, a number of cases had been recorded in which a bronzed condition of the skin was found concomitant with total destruction of the supra-renal capsules; one or two cases where partial disease of the these organs occurred, and in these bronzing of the skin was sometimes present, sometimes absent; but not a single case was at that time on record in which total destruction of the supra-renal capsules existed without manifest discoloration of the skin. And still farther, no case of bronzing of the skin had been published in which the capsules were found healthy. So that the conclusions come to by these gentlemen at the time they wrote might be considered as legitimate. At the present moment, however, the case is very different; several cases have been reported in various journals where extensive and even total destruction of the supra-renal capsules existed unaccompanied by any discoloration of the skin. In proof of this I may cite the following examples:

Professor Friedreich relates a case of extensive amyloid degeneration of the supra-renal capsules, without a trace of bronzing of the skin. He says that the organs were of the normal size, but unusually hard. On being cut into, the brown cortical pigment-zone was found entirely absent. On microscopical examination, this part of the organ was seen to be full of fat granules. The medullary substance was even in a still more advanced stage of degeneration; the greater part of the cells being transformed into homogeneous glittering masses—a fact, as the author remarks, of particular importance in connection with the absence of any discoloration of the skin.[*]

Professor Friedreich, in the same paper, mentions still another case, in which the supra-renal capsules were found of twice their normal size, and extensively affected with amyloid degeneration. The microscopical characters were similar to those mentioned as occuring in the other specimens, and yet there was no discoloration of the skin.

At one of the meetings of the Bath and Bristol Association, Mr. Davis reported two cases that he had met with of extensive disease of the supra-renal capsules, without any bronzing of the skin.[†] The one patient died of heart, the other of kidney, disease.

Prof. Virchow writes me, that he has met with several cases of tubercular deposit in one or both supra-renal capsules, unaccompanied by bronzed skin. He mentions one case in particular, in which both capsules were completely destroyed by cancerous deposit, without the slightest discoloration of the skin having been observed.

Is bronzed skin always associated with disease of the supra-renal bodies?

M. Puech communicated to the French Academy[‡] the case of a man, aged fifty-four, whose skin for a year and a half before death was observed to become gradually darker in color. This change of tint was accompanied by loss of strength, bad digestion, alternate diarrhœa and constipation. During a short interval he slightly improved, but afterwards he was seized with pain in the right iliac region, followed by black and fœtid stools. He ultimately died of peritonitis from perforation of the intestine. In this case, the bronzing of the skin was most decided upon the face, breast, abdomen, and anterior and interior of the thighs. M. Puech, and others who saw the patient, expected to find diseased supra-renal bodies. The post-mortem examination, however, undeceived them. The author's own words are, "Les capsules surrenales, minutieusement examinées, n'offrent pas la moindre alteration."

Dr. Fricke has published the case of an Irishman, aged twenty-five, who in the latter part of 1856 complained of debility, nausea, headache, and constipation. A month afterwards this man was attacked with jaundice, from which he gradually recovered. Last January (1857) it was noticed that he had bronzing of the skin. It gradually became more decided. On the 29th of April last the man died, and on post-mortem examination his supra-renal capsules were found of the normal size, hue and consistence, presenting no alteration whatever. There was very marked cirrhosis of the liver.*

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[*] Virchow's Archives, April, 1857, p. 389.
[†] British Medical Journal, Feb. 28th, 1857.
[‡] April 6th, 1857.

Dr. Luton mentions a case of bronzed skin in a patient who died of phthisis. The supra-renal capsules were carefully examined. The right weighed 3 grammes (=46½ grains); the left weighed 3½ grammes (=53 grains) They were quite normal in volume, in color and in structure. While in Edinburg last summer, Professor Simpson mentioned to me that he had had a patient who presented a peculiar discoloration of the skin. The post-mortem examination, however, revealed healthy supra-renal capsules. The following notes of the case were kindly furnished to me by Dr. Alexander Simpson. The patient was a young girl aged fourteen. She had a tumor in the upper part of the abdominal cavity, most pronounced in the left hypochondrium. The trunk of the body and the arms were discolored; the tint was more yellowish than brown, and was darkest in front of the axillæ. She ultimately died of phthisis. The liver and spleen—particularly the latter—were enormously hypertrophied. The left supra-renal capsule seemed perfectly normal, and so was the right, with the exception of a small tubercle, about the size of a mustard-seed, on its anterior surface.

The various questions already considered having all received negative answers, it may now be asked—WHAT IS THE CAUSE OF BRONZED SKIN? To this question I find it very difficult to reply. For as yet we know but very little regarding the chromatogenous function of the animal economy. In glancing at the animal kingdom, we cannot fail to be struck with the great difference of color existing in the various species of animals, and even in different members of the same species. Take, for example, the human family, and we find every shade of tint, from the fair complexion of the inhabitants of the temperate, to the jetty black of those of the torrid zone. How are we to explain the cause of this difference in the distribution of the cutaneous pigment in the human race? The action of light and heat has undoubtedly something to do with it, but that cannot be regarded as the sole cause. For although a white man becomes very dark after many years' exposure to the sun in the tropical zone, yet he never acquires the tint of the negro; nor does the latter, after a prolonged sojourn in a temperate climate, ever obtain a fair skin. It is scarcely necessary to remark, that the sun's rays are a powerful stimulant to the formation of pigment in the human skin. Every one is aware that the blond complexion during the summer months often becomes of a swarthy hue, while at the return of winter the pigment is reabsorbed, and the skin regains its former whiteness.

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* North American Medico-Chirurgical Review, July, 1857: also, the October No. of this Journal.

In the case of some of the lower animals, we have a still more beautiful illustration of the direct effects of light upon the deposition of pigment. The proteus, for example, that curious inhabitant of the dark caves in Styria, when first brought into the light is of a most beautiful transparent flesh color, and after a few days' exposure to the sun's rays it becomes of a dark hue. The cause of the change of color in this creature appears to be the chemical action of the sun's light. In the case of man, however, there are some other things to be taken into account. For we find that individuals of equally blond complexions subjected to a similar amount of exposure to the sun, are tanned in different degrees; one, perhaps, becoming of a dark-brown, while another is scarcely tinged. Again, we may even find that in the same individual the skin becomes unequally darkened, as in the case of freckled persons, in consequence of the pigment being irregularly deposited. These facts clearly prove that the deposit of coloring matter in the human skin is not wholly dependent upon external agencies. Recent researches have shown that the color of some of the lower animals may be modified by the direct application of different chemical agents to the cutaneous surface, as well as by galvanic stimulants applied to the nerves.

The special organization of some animals is remarkably exemplified in the functions of their skin. We find, for example, that some of those inhabiting northern latitudes, the ermine, the fox, and the common weasel, which during the summer months are of a brown or grey color, at the approach of winter become perfectly white. It is said, too, that this change of color is not the effect of a change of the hair, but merely of the pigmentary matter. The plumage of certain birds is also said to change in color, without a corresponding change of feathers taking place. These phenomena most probably depend more upon the peculiarity of the animals' constitution than upon external agencies, although no doubt the latter are also involved.

The differences of tint observed among the various races of the human family, are equally to be attributed to constitutional peculiarities. The gipsies are a striking illustration of this fact; for notwithstanding that they have lived during many centuries in countries whose inhabitants are perfectly fair, they still retain the black hair, the dark eye, and swarthy complexion of the Moultanee race. We have yet further evidence of constitutional peculiarities, in the fact that great difference in color exists among the inhabitants of the same locality. Among whites we occasionally meet with individuals who might be mistaken for half-castes, and among negroes those that might pass for white men.

Natural physiological changes occurring at different periods of life, also react upon the cutaneous function. It has long been observed, that at puberty the skin of the sexual apparatus becomes of a darker hue, and that in pregnant females a similar change takes place in the areola around the nipple; the depth of the color being various in different individuals. In some the change is scarcely, if at all, perceptible, while in others it is very marked. Pregnancy is also occasionally accompanied by a deposit of pigment in the cutaneous surface of different parts of the body, more particularly of the abdomen in the neighborhood of the umbilicus. Eight years ago, while house-surgeon to the Edinburgh Royal Maternity Hospital, I met with three cases of this sort, and in 1852–3, I saw other three cases of the same affection in the lying-in wards at Paris. I remember one case in particular, where a fair-complexioned young woman had a large yellow patch on the forehead extending down the nose, and another of much greater size and darker color on the abdomen. During one of the visits, Professor Dubois remarked that he had frequently met with similar cases, and that the discoloration sometimes disappeared altogether, or got paler after delivery.

Mr. Erasmus Wilson, in his admirable work on skin diseases, relates the following interesting case. A young girl, aged eighteen, stated to Mr. Jackson, her medical attendant:

"That shortly after she became pregnant the areola round each nipple looked very dark, but no further perceptible change took place until she quickened, when an evident darkness of the whole breast was visible, extending upwards to the throat and downwards to the thighs, gradually assuming a deep black color. Over the hips it extended laterally; but no part of the posterior surface of the body was affected. Her complexion was naturally rather dark, with black hair and eyes. Her health had been always good. * * * She left the house (High Wycomb Union) a month after her confinement, at which time there was no alteration in the blackness of the skin; but on my last meeting her (Mr. Jackson remarks) about a year afterwards, she assured me it had entirely disappeared."*

When discoloration of the skin accompanies pregnancy, we must look upon it as a purely physiological phenomenon, and as such leave its treatment to nature. There are, however, many cases of discoloration of the skin which cannot be regarded in this light, some being ushered in by different signs of morbid action, while others seem to arise spontaneously, without any apparent abnormal state of the system accompanying them. As an example of the former, I may mention a case that I saw last year. The lady had several yellowish-brown patches on different parts of the body, and was then under the care of Dr. Madge, to whom I am indebted for the following notes of the case:

"The patient, a stout, healthy-looking person, about thirty years of age, first noticed a patch of a brownish-yellow color on the right breast about six years ago. Shortly afterwards she became pregnant, and the patch rapidly increased in size. It extended to the arms, and covered the greater part of the epigastrium. After her confinement, having to use a stimulating application to the breast, in the form of an embrocation, the discoloration gradually disappeared from the part to which it was applied. At the same time it became less marked in the other parts affected. A large patch in the umbilical region, and several smaller ones higher up, however, remained unchanged. The patient has observed that the discoloration becomes darker and increased in extent during any disturbance of the general health. This was the case when Dr. Madge first saw her. The increase of the discoloration preceded an attack of a mild form of typhoid fever. The skin at that time was rough and tender (as in cases of chloasma). During the course of treatment it was necessary to apply mustard poultices to the epigastrium, and these appeared to have a decided effect in lessening the discoloration."

On examining with the microscope a piece of bronzed skin taken from a patient whose supra-renal capsules were diseased, I found that it presented, as shown by Mr. Hutchinson and Mr. T. West, the very same appearances as the skin of the negro. The pigmentary matter was deposited in the epithelium cells of the under layer of the epidermis—the rete mucosum, as it is sometimes called. The superficial layer of the epidermis, on the other hand, was almost entirely free of pigment. After carefully examining the individual cells containing the coloring matter, with the view of ascertaining whether the pigment was diffused throughout the cell, or merely deposited in the nucleus, I was able to satisfy myself that in this piece of bronzed skin, as a general rule, the coloring matter was not limited in its distribution to the nucleus, but spread throughout the cell. The nucleus was, however, most deeply tinged.

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* Diseases of the Skin, p. 557. Fourth Edition.

In terminating the present communication, I have only to add that the conclusions which I draw from the facts stated in the preceding pages are: Firstly, That bronzed skin may exist without the supra-renal capsules being diseased. Secondly, That complete degeneration or total absence of the supra-renal capsules may occur without any bronzing of the skin. Thirdly, That bronzed skin may be associated with a variety of different morbid conditions of the system, among which a prominent one is disease of the supra-renal capsules. Fourthly, That bronzed skin may be present without any derangement of the other functions of the body being observed. The treatment of the affection must consequently be varied accordingly.

A HOMŒOPATHIC LAWSUIT IN FRANCE.—It appears that twelve homœopaths, practising in Paris, have brought an action against the journal called *L' Union Médicale,* for an article written by Dr. Gallard, and which puts the delusion in its proper light. The damages are laid at £2000.—*Loudon Lancet, July,* 1858.

[We shall endeavor to translate this article of *L' Union Médicale* for our readers.—EDITORS.]

CONGENITAL DEFICIENCY OF ONE BREAST IN A GIRL.—A girl, aged 10 years, now attending as an out-patient at the Metropolitan Free Hospital, under the care of Dr. Staveley King, affords an example of a rare congenital abnormity, viz., the entire absence of one mammary gland and its nipple. It is the right gland which is wanted. The opposite one appears naturally developed. As is, we believe, usual in these cases, the lower serations of the pectoralis major muscle are also deficient. There is no history of any of the child's relatives having had a similar abnormity, but her mother states that at an early period of her pregnancy she saw a woman who had had an operation performed on her breast, and that the circumstance made a vivid and painful impression on her mind.

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ON THE CLINICAL STUDY OF THE HEART-SOUNDS.

By AUSTIN FLINT, M. D., Prof. of Clinical Medicine and Auscultation, in the N. O. School of Medicine.

LETTER NO. I.

PROF. FENNER—*Dear Sir:* In compliance with your request, I will endeavor to give in this and a succeeding letter, prepared for publication in the *Medical News and Hospital Gazette,* a *résumé* of the results of the study of the heart-sounds in health and disease--a subject on which I have of late bestowed some attention. The modern history of auscultation is to be dated from the epoch of the application to the chest of a quire of paper rolled into a cylinder, by Laennec, in the case of a patient affected with disease of the heart. But the illustrious founder of physical exploration at once directed his researches to the phenomena pertaining to the respiratory system, and, although he was the first to observe adventitious sounds, or *murmurs,* in certain cardiac affections, he accomplished, in behalf of the diagnosis of the latter, little in comparison with the results of his investigation of pulmonary diseases. This was in a great measure owing to his holding false notions respecting the production of the normal sounds of the heart. The correction of these errors by means of the investigations of Hope and others, prepared the way for a better knowledge of the physical signs referable to this organ. The importance of an accurate acquaintance with the heart-sounds in health, as regards their sources, their relations to the cardiac movements, and the mechanism of their production, is obvious to any one who has given attention to the subject. But the importance of the *clinical* study of these sounds, with reference to these points, has not been sufficiently appreciated. A dependance too exclusive has been placed on experiments by means of vivisections and on the few instances of ectopia which have fallen under observation. From these experiments valuable results have been obtained, more particularly as regards the movements of the heart, but also, to some extent, with respect to the normal sounds. There is still scope for experimental researches with the heart exposed to view. But it must be apparent, on a little reflection, that, so far as auscultation is concerned in these experiments, it involves difficulties and incidental circumstances which render it, in a measure at least, unsatisfactory and unreliable. Suppose that in an animal of large size we have succeeded in opening the chest and pericardium, so that the heart may be seen and handled for a considerable period, its movements being maintained by artificial respiration, one important normal condition is necessarily wanting, viz: contact with the thoracic parieties; nor have we a right to assume that, after such an operation, the conditions as respects the quantity of blood within the cavities, the contractions of the walls and the flap of the valves, are such as to render the sounds faithful types of those incident to the normal state. But, irrespective of these considerations, auscultation must be practiced by applying the stethoscope immediately upon the heart, a muscular organ in almost unceasing activity. How can the sounds as thus heard, represent fairly those obtained by auscultation with the stethoscope placed upon the chest? These remarks will, of course, equally apply to cases of ectopia. A single trial suffices to show that the study of the heart-sounds thus pursued, is unsatisfactory, and, to a certain extent, unreliable. On the other hand, the *clinical* study of the normal sounds (and by this expression I mean the study by means of examination of the healthy chest,) is capable of yielding results, reliable and satisfactory, which lead to important conclusions concerning the origin of these sounds and the agencies involved in their production. If to the clinical study in health, be conjoined the clinical study in disease, together with the information respecting the movements of the heart obtained by vivisections, our knowledge of the normal heart-sound may be rendered sufficiently complete for all practical purposes—that is, so far as this knowledge is necessary to serve as a point of departure for obtaining, by means of clinical observation, an acquaintance with the abnormal modifications of the sounds of the heart, and the significance which belongs to the latter as physical signs of cardiac affections.

The importance of the abnormal modifications of the heart-sounds, as constituting physical signs of disease, have hitherto been inadequately estimated. Of the small value attached to them in this point of view, the trifling space generally allotted to their consideration in works treating of diseases of the heart, is sufficient evidence. A few sentences, or at most, a few pages only, are devoted to this subject. The attention of auscultators appears to have been absorbed by the study of adventitious sounds, or murmurs.* I am far from wishing to depreciate the value which the latter possess in diagnosis. In determining the existence, seat, and some of the effects of valvular lesions, they are invaluable. They by no means, however, meet all the wants of the diagnostician. Their presence, as is well known, is not, in itself, evidence of organic disease of the heart. The practitioner is to distinguish between organic and inorganic murmurs. This, it will be granted, may be done without much difficulty in the great majority of cases. Assuming that this is done, and excluding the rare instances in which a bellows sound accompanies uncomplicated enlargement of the heart, what information do murmurs afford beyond the fact of valvular lesions and their situation? Do they indicate the extent of damage which the lesions have occasioned—in other words, the amount of obstruction or regurgitation resulting therefrom? No one will venture to answer affirmatively the latter question. The intensity and quality of murmurs have no special pathologicical significance. A rough or intense murmur may be due to lesions which are insignificant, involving no immediate danger; or, on the other hand, the most serious lesions may be accompanied by a soft and feeble murmur. To be able to determine the injury which existing lesions have occasioned, is of vastly more importance than to ascertain the single fact that lesions exist, and to localize them; for, valvular lesions give rise to danger and inconvenience only in proportion as they render the valves insufficient or induce contraction of the orifices. It were easy to adduce cases in illustration of the correctness of these statements, which will be admitted by all who have given attention, practically, to diseases of the heart. Now, the study of the abnormal modifications of the heart-sounds, taken in connection with the murmurs, is capable of supplying, in a great measure, that which the study of the latter alone is incapable of affording, viz: information respecting the extent of valvular lesions, and the damage which they have occasioned. Attention to the heart-sounds in disease, moreover, affords aid, in certain cases, in the localization of these lesions.

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* It is hardly necessary to remind the reader that, conventionally, the term *heart-sounds* is now restricted to the normal sounds and their abnormal modifications; while all adventitious sounds are included under the name of *murmurs*

In the clinical study of the heart-sounds in health and disease, the two sounds are, of course, to be made separate subjects of investigation. It is quite needless to premise that these sounds are distinguished, respectively, as the *first* or *systolic*, and the *second* or *diastolic* sound; that the *first* sound takes place synchronously with the apex-beat and the pulsation of the large arteries near the heart, while the *second* sound occurs after the systolic movement of the ventricles; that the two sounds succeed each other in a certain rythmical order, and that, combined and forming what is called the beat or revolution of the heart, they follow after successive intervals nearly or quite uniform; and, finally, that each sound and interval has a certain relative duration, the sounds differing from each other in pitch, quality and intensity, as well as in length. Proceeding to present, as concisely as possible, those points pertaining to each of the sounds, which are of practical importance with reference to diagnosis, I shall commence with the second sound. It might seem more natural to give precedence to the first sound. The reason for reversing the natural order is, that the second is the simpler sound; as regards its seat and mechanism it is better understood, and its consideration will prepare for the better study of the first sound. The remainder of this letter will be devoted to the second or diastolic sound in health and disease, and in a subsequent letter the first or systolic sound will be considered.

SECOND OR DIASTOLIC SOUND OF THE HEART IN HEALTH AND DISEASE.

That this sound is produced mainly at the orifices of the aorta and pulmonic artery, and that the semilunar valves are essential to its production, may be assumed as sufficiently established. The commonly received explanation is, that the sound is due to the sudden, forcible expansion of these valves, caused by the recoil of the arterial coats succeeding the dilatation by the column of blood propelled from the ventricles. The force with which these valves are unfolded and rendered tense, and the consequent intensity of the sonorous vibrations, will, obviously, other things being equal, be proportionate to the quantity of blood propelled through the aorta and pulmonic artery by the ventricular systole and the muscular power of the ventricles. The clinical study of this sound in health confirms the correctness of this localization. The sound has its maximum of intensity above the base of the heart, at the points where the aorta and pulmonary artery approach nearest to the thoracic walls, viz: in the second intercostal spaces on either side of the chest, near the sternum.

This sound emanates from the aortic and pulmonic orifices. Now, is it true that the aortic and the pulmonic sound may be disconnected and distinguished from each other in certain situations, as has been of late maintained by some observers? Plainly, this is a question to be settled by comparing the sound heard at different points on the chest in a series of healthy persons. With reference to this and other points relating to the normal heart-sounds, I examined with care twenty-five healthy persons, selected for this purpose, noting the results at the moment of the examination, and afterwards subjecting them to analysis. In nearly all of these persons, the sound in the second intercostal space, near the sternum, presented, on the two sides of the chest, well marked differences in pitch, duration, intensity, and apparent proximity to the ear. On the left side it is, relatively, more feeble, dull, distant and longer. On the right side it is more intense, more acute, nearer and shorter. These differences can hardly be due to modifications received by the sound in its transmission to the ear; and, hence, they point to a distinct source of the sound which, in health, predominates in each of these situations. Taking into view the anatomical disposition of the aorta and pulmonic artery, we arrive at the conclusion, that the sound in the second intercostal space, on the right side, emanates from the aorta, and that on the left side, from the pulmonic artery. Observation in cases of disease confirms the correctness of this conclusion. When the aortic valves are the seat of lesions which prevent their play, the second sound on the right side is impaired or wanting; that on the left side remaining, and preserving its normal characters.[*]

This point settled, another inquiry arises: The second sound, as heard in the other situations within and near the præcordia—at what points is it aortic, i. e , emanating from the aortic orifice, and at what points is it pulmonic, i. e., referable to the orifice of the pulmonary artery? This question, plainly, is to be settled by auscultation of the sound in different situations, and comparing its characters successively with the aortic and the pulmonic sound as heard in the second intercostal space on the two sides, near the sternum. An analysis of the results obtained by examining twenty-five healthy persons, leads to the following conclusions: The sound presents the characters which distinguish that emanating from the pulmonic orifice, in a few instances, in the third intercostal space on the left side, and over the body of the heart within the 'superficial cardiac region.' In a larger proportion of instances it presents these characters over the inferior boundary of the heart, just above the xiphoid cartilage. In other situations within and near the præcordia, in which the second sound is heard, it uniformly presents the characters distinctive of that emanating from the aortic orifice.

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[*] "Ce que nous ne pouvovs produire traumatiquement et brusquement, la nature le fait sans nous, d'une maniere graduelle et sans porter de profondes perturbations dans les fonctions de l'organe, comme ferait une vivisection; en sorte que nous considerons ces lesions pathologiques comme de tres-bonnes *experiences* cliniques."—*Bouillaud-Lecons cliniques sur les maladies du cœur*, 1853.

Other important conclusions are deduced from the results of the clinical study of the second sound in health. The sound has a strongly marked quality which conveys the idea of valvular action, and may be distinguished as a valvular quality. It preserves this valvular quality wherever it is heard. It does not present any marked variations in different situations, save, as regards intensity. It is characterized by uniformity, and is evidently an unmixed sound, that is, due exclusively to a single element, in these respects differing, notably, from the first sound of the heart. As regards intensity, it varies far less than the first sound, being affected in a less degree by the causes which increase or diminish the force of the heart's action. In the rythmical succession of the two sounds at the base of the heart, the stress appears to fall on the second; the latter is said to be accentuated. Over the body and apex, it is otherwise; the accentuation is on the first sound. But without the præcordia, at points more or less removed from the heart, when the two sounds are heard, the second sound is accentuated; showing that, although within the limits of the præcordial region the intensity of the first sound preponderates, the second sound is more widely diffused. This is also shown by the fact that in carrying the stethoscope away from the heart, the first sound, as a rule in health, becomes inappreciable sooner than the second. The explanation of these results of observation will appear when the different elements composing the first sound are considered.

Directing attention now to the study of the second sound in disease, we are at once brought to the practical importance of distinguishing between the aortic and the pulmonic sound. In certain cases of cardiac disease, the sound referable to the pulmonic artery becomes more intense than that referable to the aorta. Prof. Skoda, of Vienna, was, I believe, the first to call attention to the diagnostic significance of this intensification or reinforcement of the pulmonic second sound. It is at first view mysterious that there should exist a relation of sequence between this event and lesions of the mitral orifice producing either obstruction or regurgitation, or both; but well known pathological laws render it intelligible. The first effects of mitral contraction or insufficiency, are distension and dilatation of the left auricle; incidental to these effects is pulmonary congestion, and the third link in the chain of sequences is hypertrophy of the right ventricle. The increased power of the right ventricle due to its hypertrophy, and the resistance to the current of blood in its circuit through the lungs in consequence of the congestion of the pulmonary vessels, increase the distension of the pulmonic artery by the column of blood propelled by the ventricular systole; hence, a corresponding increase of the recoil of the coats of the artery after the systole, and an abnormal intensity of the second sound emanating from this artery. Intensification of the pulmonic second sound is thus a sign of hypertrophy of the right ventricle, a pathological effect, in the vast majority of cases, of mitral obstruction or regurgitation, either separately or combined. I have given the explanation of the fact, but the first question is, does clinical observation afford sufficient evidence of the fact? I can testify from considerable experience to the intensification of the pulmonic sound in the great majority of the cases of mitral lesions which have eventuated in hypertrophy of the right ventricle. The greater intensity of this sound, however, as compared with the aortic, is to be explained, in a measure, by the diminished intensity of the aortic sound as an effect of the reduced quantity of blood propelled through the aorta in cases of mitral contraction and of insufficiency. This will account, in part, for the greater relative intensity of the pulmonic sound. But, aside from this explanation, a positive augmentation of the intensity of the latter undoubtedly takes place in a large proportion of cases. The sign is valuable, taken in connection with the evidence afforded by other signs of the existence of mitral lesions, as denoting that the heart has begun to suffer from the effects of these lesions, which, until they lead to enlargement, are not attended by much danger or inconvenience. Prof. Skoda does not appear to attach sufficient importance to the greater relative intensity of the pulmonic sound being measurably due to the weakening of the aortic sound. The positive augmentation only, of course, is evidence of hypertrophy of the right ventricle. It is important, therefore, to decide, in individual cases, whether the aortic sound preserves its normal intensity or not. This can generally be done by observing, not only its absolute intensity in the right second intercostal space, but its intensity in situations removed from the præcordia, where it is usually found in health to predominate over the first sound. It is also to be borne in mind that morbid conditions pertaining to the lungs occasion greater intensity of the second sound in the second intercostal space on the left side. A tuberculous deposit near the summit of the left lung has this effect; so, chronic pleurisy affecting the left side after absorption of the effused liquid, etc.

Attention to the aortic second sound in connection with the diagnosis of lesions affecting the valves and orifice of the aorta, is a practical point of much importance. The existence of lesions in this situation is shown by the presence of aortic murmurs. But the characters of these murmurs furnish no evidence of the amount of damage to the aortic valves which the lesions have occasioned. How are we to judge of the condition of these valves prior to that stage of disease when enlargement of the left ventricle is proof that aortic obstruction or regurgitation, or both, have existed for a considerable period? So long as the aortic second sound retains its normal characters and intensity, notwithstanding the presence of aortic murmur, the valves are not greatly damaged. A loud murmur may be due to aortic changes which have left the valves intact, and are therefore of comparatively little consequence. This statement is applicable to a diastolic, as well as to a systolic aortic murmur. The aortic valvular sound thus becomes a highly important criterion of the condition of the valves, in connection with aortic murmurs. Immobility of the aortic valves involves suppression of the aortic second sound, the pulmonic sound remaining. Repeated illustrations of this fact have fallen under my observation. Abnormal weakness of the aortic sound will correspond to the degree of contraction, rigidity or destruction of the valves, which the lesions have occasioned. In making the weakness of the sound the criterion of the amount of damage which the valves have sustained, it is important to bear in mind that this result also occurs as a consequence of mitral obstruction and regurgitation As evidence of the impaired condition of the aortic valves, its value is therefore lessened in the cases in which, as not unfrequently happens, mitral and aortic lesions co-exist. The muscular power of the heart is also to be taken into account. Feebleness of the contractions of the left ventricle, of course involves a feeble aortic sound. But as the causes inducing diminished muscular power in general act on the whole heart, the pulmonic and the aortic sound are alike expelled. Both, however, are affected under these circumstances, far less than the first sound of the heart.

In another point of view attention to the aortic second sound is of practical importance in diagnosis, viz., in the localization of a diastolic murmur. A diastolic murmur proceeds either from a retrograde aortic current, or from the direct current through the mitral orifice. The occurrence of a mitral diastolic murmur, except in very rare instances, is ignored by some writers; but I am satisfied that I have repeatedly met with instances in which it was present. The discrimination of this from an aortic regurgitant murmur, is attended with difficulty. The aortic murmur is said to be post-systolic, and the mitral murmur pre-systolic; but when it is considered that these distinctions of time are to be made within less than one third of a second, it is plain that their practical application is not always easy. Moreover, in localizing a diastolic murmur, we have not the advantage of those rules as regards difference of situation in which the maximum of intensity is observed, and the different directions in which the murmur is propagated, which are so useful in discriminating between mitral and aortic *systolic* murmurs.

The normal intensity of the aortic second sound, or otherwise, divests this problem in diagnosis of much of its difficulty. If the aortic sound preserve its normal intensity, this is evidence against the aortic origin of the murmur, and, of course, in favor of its being referable to the mitral orifice. Abnormal weakness of the aortic sound, on the other hand, is evidence that the murmur is not mitral, but referable to the aorta. I need not add that in resolving this problem, other circumstances are to be considered. My object has been simply to point out the importance of the aortic second sound in this connection.

I have endeavored in this letter to present, with great brevity, the conclusions drawn from the clinical study of the *second* or *diastolic* sound of the heart in health, together with the abnormal modifications of this sound, which are of most importance in their diagnostic bearings. The latter, not less than the former, are based on clinical studies, and, were it consistent with the limits of this communication, I could have adduced cases illustrative of the practical points which have been noticed.* I am well aware that to those readers who have given little or no attention to physical exploration as applied to the diagnosis of cardiac affections, these points will seem obscure. This is unavoidable, and not to be regretted should any be led thereby to cultivate a field of practical medicine not less attractive than important. In a subsequent letter I propose to consider the clinical study of the *first* or *systolic* sound of the heart in health and disease. In the mean time, I remain, with much respect, yours,

A. F

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* The reader will find illustrative cases in the essay by the writer in the Transactions of the American Medical Association for the present year.

WÜTZER'S OPERATION FOR THE RADICAL CURE OF REDUCIBLE INGUINAL HERNIA—WITH CASES.

By S. CHOPPIN, M. D., Professor of Surgery, New Orleans School of Medicine; Visiting Surgeon, Charity Hospital.

After having confided for a long time in the efficacy of the thousand remedies successively enjoying a reputation for the radical cure of hernia, it had been finally concluded that this result was next to impossible. Charlatanism availed itself of this conclusion, and hence have arisen that miserable class of quacks who, styling themselves hernia curers and physicians, have infested the medical profession from the time of Hippocrates, Galen and Celsus, to the present day, proclaiming the infalibility of this or that system of springs or trusses, all equally useless and absurd. I do not wish to be understood as being opposed to compression in the treatment of hernia, and the application of good trusses. Far from it; for I believe that the safest and most permanently successful cures can be, and are effected by the compression of a well adapted truss. In this way, for instance, hernia of infants are often radically cured; but the same result rarely occurs at a more advanced period of life. Trusses, in adult age, are, at best, but palliatives in the vast majority of cases, although pretended hernia curers tell us that a good truss is far better than a radical cure.

While on this subject, I had intended examining the doctrines and cases contained in an article entitled "Radical Cure of Inguinal Hernia, by the use of the Truss: by W. T. Leonard, M. D., formerly Professor of Anatomy in the Washington University, of Baltimore," published in the May number of the New Orleans Medical and Surgical Journal; but upon a careful perusal of the article, I find it so inaccurate, so unscientific, so filled with inconsistencies, and such an evident puff of Sherman's trusses, that any notice of it in a scientific journal which has for its end the dissemination of truth, would be out of place. Enough, then, of hernia curers, and their partizans.

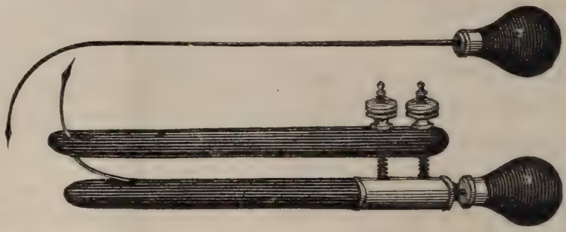
The radical cure of hernia is a triumph so important to surgery, and a subject so deeply interesting to humanity, that the surgeon must seize with avidity, in the experimental spirit of our age, upon every new plan of treatment proposed for the cure of one of the most common and most fatal diseases to which the human race is subject. I take pleasure in advocating Wützer's operation for the radical cure of reducible inguinal hernia, because I consider it rational and efficacious, and proved, by very ample experience, to be both safe and successful, and therefore worthy of general adoption.

The operation is rational, because it proposes to effect all the conditions required for a radical cure, viz: the plugging up of the ring and canal, causing there the production of an inodular cicatrix, which entirely blocks up the passage.

That the operation is not dangerous, we adduce from the fact that there has not yet been recorded one single instance where fatal results followed its application; and Professors Wützer, of Bonn, and Rothmund, of Munich, who certainly have operated over a thousand times, both tell us that they have never seen any bad consequences result from it. Besides these, a host of English surgeons, Spencer Wells, Holmes Coote, Hutchinson, Erichsen, Cummings, Redfern Davies, etc., etc., all testify to the perfect safety of the operation.

Wützer's operation consists in plugging up the ring and canal with the skin of the invaginated scrotum and producing agglutination of the hernial sac. Thus far it is nothing more than a modification of Gerdy's operation, who was, certainly, the first to propose the plugging up of the ring and canal in this way. His operation is performed in the following manner: The patient is placed on his back, with the shoulders elevated and the knees drawn up; andthe hernia reduced. The surgeon then places the index finger of the left hand on the scrotum, two inches below the hernial opening, pushes the fold of the scrotum into the ring as deeply as possible, and into the inguinal canal. In this blind sac the finger remains. A curved needle, provided with a handle, and its eye armed with a double thread, is next introduced on the palmar surface of the forefinger, to the bottom of the invagination, thrust through the invaginated skin and parieties of the abdomen, so that it emerges above the inguinal ring. One end of the thread is then drawn out and confided to an assistant, the finger still remaining in the canal. The needle, armed with the other end of the thread, is partially withdrawn and again thrust through the parts at some lines from the former stitch, when the thread is withdrawn from it. After the removal of the needle, the threads are to be divided and a cylinder of plaster placed between them, upon which they are to be tied; the ensheathed part of the scrotum is thus fully retained in its place.

This operation, performed in a great many instances, has given very satisfactory results, but it has also been followed by frequent relapses and occasional fatal consequences. Although not abandoned, it has been almost superceded by Wützer's operation, which effects the same object, viz: the agglutination of the neck of the hernial sac and the closure of the inguinal canal by the invagination of the scrotum, in a safer and more successful manner. This plan of treatment is carried into effect by means of an instrument which consists of a needle, a cylinder, and a concave cover or compressor. The cylinder, which is intended to take the place of the index finger after it has pushed a plug of scrotum through the ring into the canal, is made of very hard wood or rubber, of different lengths and diameters, according to the conditions of the rings and canal in each case. In its centre is a canal, through which an elastic needle passes and comes out on the upper surface, near the point. Over this a concave cover, also of hard wood, is made rather wider than the cylinder, with an opening to admit the point of the needle, and another opening for two metallic staffs, which rise from the cylinder, near the handle, on which a couple of screws work, by which the cylinder and cover can be brought together. The handle and point of the needle can be removed by unscrewing.



As to the details of the operation, we will borrow the description from T. Spencer Wells, F. R. C. S.,* who was certainly the first sugeon to perform the operation in Great Britain and bring it to the notice of the profession there. Besides, Mr. Wells has had the opportunity of witnessing the performance of the operation by Wützer himself.

"Now for the manner of using the instrument. I need hardly say
" that the patient's bowels should be open a day or two before the opera-
" tion, and the rectum cleared the same day by an enema of warm water;
" the bladder is emptied and of course the hernia returned if it be down.
" This being done you place the patient on his back, with the shoulders
" raised, the thighs semi-flexed, and the knees separated, just as if you
" were about to reduce a hernia by the taxis. You place yourself on the
" side you are about to operate on."

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* Medical Times and Gazette, January 23 1858.

" If you can use right and left hand equally well, you may use the left " forefinger for invaginating on the left side, and the right to hold the " instrument—the reverse for the right side. But the left finger will do " for invaginating on either side very well. You see, I operate on the " left side, so I stand on the left side of the patient. I place my left fore- " finger here on the scrotum, about an inch below the external ring, and " then push a fold of the scrotum before my finger with a little rotatory " movement, slowly and steadily into the canal, keeping the palmar sur- " face of the finger turned forwards and a little outwards, until it is well " under the tendon of the external oblique, and the plug of scrotum is " well pressed up to or through the internal ring. If you place one fore- " finger on the abdomen, just over the internal ring, while the other is in " the canal beneath the tendon of the external oblique, you will feel the " tendon very distinctly. As you move the finger backwards and forwards " it rolls over the tendon. You should accustom yourselves to feel this, " because it is the best test by which you know and can be certain that you " are well in the canal, and when you have introduced the cylinder, that it is " in the canal and has not slipped anterior to it When you are quite sure " that your finger is in the canal, the next thing is to get the cylinder into " the place of the finger, and then to fix it there by the needle. To do " this, you take the cylinder, with the needle passed within it as far as you " can without permitting the point to project, and hold it, as I now do, in " the right hand, with the thumb before the screw, the forefinger and second " finger on either side of the prong which supports the cover, and the " ring and the little finger on the under side of the cylinder. Then bend " the left forefinger a little, draw it forwards, and slip the cylinder along " its dorsal aspect, at the same time as the finger itself is being withdrawn. " This is the most important step of the whole operation, and the only step " which is at all difficult. Without care, the plug may follow the finger. " Without care, the cylinder, instead of slipping beneath the tendon of " the external oblique, may slip between it and the integument. You " must be very careful that this does not occur, by feeling, as you felt " when your finger was in the canal, that the tendon rolls over the cylin- " der. If you do not feel this, and if the cylinder moves freely beneath " the integument, you may be quite sure it is not in the canal, and you " must begin again. I wish, particularly, to guard you against this mis- " mistake, because I know it has been made at least once, and I suspect ' oftener. If you feel the tendon rolling over the cylinder, you may be

" quite sure it is in the canal, and the end well up to the internal ring. " To fix it there, I have only to push on the needle till its point appears " through the abdominal parietes, then to put on the cover and use the " screws until the cover and cylinder are evenly pressed together. Then " the point of the needle is unscrewed, the knob put on, the handle of the " needle removed, and all is done. You leave the patient with the instru- " ment secured thus.

" The whole affair does not take a minute, and as the only part of it at " all painful is the simple puncture of the needle, it is quite unnecessary " to give chloroform.

" I should have told you that you should oil the cylinder before using " it, or else smear it, as Rothmund does, with cantharides ointment. " Wützer does not think it at all necessary to remove the epidermis of the " invaginated scrotum. Rothmund, however, thinks additional security is " obtained by removing the epidermis, and thus producing adhesion of the " opposed surfaces of the plug.

" Then, as to the degree of pressure you apply with the screw upon the " cover. It should be very slight the first day, to allow for a little swell- " ing. The next day the screw may be tightened or loosened, according to " the state of the skin. The patient should be kept in bed, lying upon his " back, with the knees raised by a pillow, and the scrotum supported by a " cushion. Every day the cover should be raised, to see whether the pres- " sure is equal and not too great. If it does not press evenly, a little cot- " ton or lint may be inserted, or if it press too much at any point, this may " be raised by a little cotton put near it.

" About the fourth or fifth day, you will see a little inflammatory red- " ness and swelling around the needle puncture, and commencing suppura- " tion. On the sixth or seventh day, some serous fluid begins to escape " from the end of the cylinder. This shows that the epidermis is separating. " There is more purulent discharge from the puncture, and the instrument " may then be taken away, by withdrawing the needle and carefully remov- " ing the cylinder. You will find that the skin of the scrotum may be " pulled tolerably hard without yielding at all, showing that pretty firm " adhesion has taken place. If it appear to yield on pulling slightly, it " would be well to replace the instrument for two or three days longer.

" The after treatment will vary a little, according to your desire to pro- " duce adhesion of the opposed surfaces of the plug to each other, or not. " Wützer filled the cavity left by the withdrawal of the cylinder, with

" charpie, dressed the puncture simply, and then put on a common bandage. Rothmund endeavors to obliterate the cavity. He keeps the " patient in bed, raises the scrotum by a suspensory bandage, applies simple " dressings to the puncture, and over this a graduated compress which is " made to exert some pressure and keep the two sides of the cavity together, " by a common figure of light bandage.

" Allowing six or seven days for the period the instrument is applied, " and from seven to fourteen days more till cicatrization is completed, you " should generally prepare a patient to expect a confinement of three weeks " to his room. After this, he must wear, for two or three months, a very " elastic truss, with a weak spring and well stuffed pad. If the pressure " is too great, or the pad too small, absorption of the plug may take place, " and relapse of the hernia follow. If no truss be worn, the adhesions, which " are still soft and yielding, might give way. It is also well to have a suspensory bandage worn as long as the truss, that the weight of the testicles " may not drag down the skin of the scrotum to its former position.

" You should also forbid violent exercise, until the truss can be left " off. After three months the truss may be left off, and I believe the " patient is fully as secure against the occurrence of hernia on the side " operated on as on the other side, perhaps more so; in other words, " that he has not only been cured radically of his hernia, but he has been " protected against its occurrence—he is less likely to become subject to " hernia, on that side, than a healthy man.

" We are still in want of knowledge of the precise anatomical conditions " resulting from the operation, which post-mortem examination alone can " afford. Rothmund has never, in all his cases, had one fatal result, " although he had several when he operated by the methods of Gerdy, " Bonnet, Belmas; nor has he happened to be able to open one of his patients who had died of some other disease. But Dr. Thormann, of Chur, " opened a patient of Rothmund's, who died a short time after the operation, of some other affection, and he has described the appearances. The " patient, 20 years old, of middle size, suffered from a reducible inguinal " hernia, of the size of a hen's egg, on the left side. The canal and rings " were small, and the smallest cylinder was used. The radical cure was " effected, and when the patient died of some other disease, Dr. Thormann " found, after death, the invaginated scrotal plugs so firmly united by adhesive inflammation to all parts of the canal that it could not be separated " without dissection. The external ring and the whole canal, to within six "lines of the external ring, were completely filled and closed up by adhesion of the plug."

Case I.—Henry Bailey, native of Utica, New York, aged 23 years, a laborer, and in the most perfect state of bodily health, has been the subject of oblique inguinal hernia on the left side since five months; the hernial tumor, gradually increasing, is now about the size of a goose egg. It is intestinal only, and is easily and completely reducible. The patient lying on his back, with his knees drawn up, the bowels having been well cleared out, the parts shaved, and the hernia reduced, Wützer's instrument, (as made by Tiemann, of New York,) the cylinder being well smeared with cantharides ointment, was applied on the 18th day of June. The testicles were supported upon a soft cushion, and the handle of the instrument confined to the thigh.

During the first twenty-four hours the patient complained of some pain, probably caused by the blistering surfaces of the invaginated portion of the integument.

On the fourth day after the operation there was some swelling and tenderness around the invaginated plug, and a little purulent discharge.

On the sixth day the instrument was removed, and upon examination the following was the condition of the parts:

On the surface of the abdominal walls, through which the needle had passed, there was a superficial ulceration of the size of a twenty-five cent piece; along the track of the inguinal canal, the invaginated portion of scrotum could be felt distinctly and separately from the surrounding parts. Around the invagination, extending for a distance of two inches, the parts were red, swollen, thickened, and somewhat tender on pressure. A pad and a figure of 8 bandage (spica) was applied, and the patient ordered to keep his bed for a few days. Ten days after the removal of the instrument, the parts were perfectly cicatrized and the patient allowed to walk about the ward.

After the removal of the apparatus and cicatrization of the parts, the patient was made to cough and jump, without producing a return of the hernia, and the plug remained firm. The patient was advised to wear a soft elastic truss for a few weeks, and was discharged from the hospital perfectly cured of an affection which had given him a great deal of annoyance and suffering for five months.

I by chance saw the patient one month after his discharge from the hospital, and, although he had been hard at work on the levee, the hernia had not returned.

Case. II.—Michael Clansey, native of Ireland, aged 23 years, was admitted into my surgical service at the hospital on the 18th day of May, affected with a reducible oblique inguinal hernia of the right side, of four years standing, and produced by violent exertions in skating. This patient could not wear a truss, on account of the excessive pain it produced. The hernia was a large one, about the size of a fully developed fœtal head; the inguinal canal and external abdominal ring were very large. I applied Wützer's apparatus, on the 22d May, and allowed it to remain until the 29th, when, as the parts began to suppurate and give signs of adhesive inflammation, it was removed.

I observed, at the time of the operation, that the cylinder was not large enough to fill up the external ring, even with the invaginated scrotum. This was a bad omen, as Wützer insists that the condition *sine qua non* to a successful result is the exact fitting of the cylinder to the diameter of the passage.

No untoward symptoms appeared during the application of the instrument. When removed, there was an ulceration around the exit of the needle. A spica bandage was applied, and the patient ordered to keep his bed for a few days, after which he was allowed to walk about the ward. As soon as the parts were cicatrized, I examined the patient, and found that when he made efforts at coughing, a small tumor, about the size of a hen's egg, could be felt at the upper portion of the inguinal canal. I explored the external ring, and found that I could introduce the end of my little finger into that opening. The operation had evidently been but partially successful. I procured a smaller cylinder which exactly fitted the ring, and again applied the apparatus, on the 24th of June. The instrument remained six days, after which it was removed. A hard plug could be distinctly felt in the canal, and although the patient was made to cough and compress his abdomen, no descent of the hernial tumor occurred. This man was discharged from the hospital about the 15th day of July, perfectly cured. He visited the hospital once afterwards, and said that notwithstanding his having been at very hard laboring work, the hernia had not returned.

Case III.—J. O'Neil, aged 52 years, a native of Ireland, affected with a very large obliqe inguinal hernia of the left side, of over two years standing, was operated upon by Wützer's method on the 26th day of June. This case was peculiarly interesting to me, as I had operated on him some months previous for strangulated hernia, (vide New Orleans Medical News and Hospital Gazette, for April, 1857,) and reported him as having been radically cured by the operation required to relieve the strangulation. At the time the report was made, I examined the patient and found that there was no descent of the hernial tumor. According to his account, the hernia recurred three months after the last examination.

At the time of his admission into my wards, the hernial tumor was larger than it had ever been before. The canal and ring were both excessively dilated, and allowed a considerable amount of intestines to protrude.

The cylinder of the instrument in my possession was too small to fit the diameter of the ring and canal, so I obviated this difficulty by rolling strips of adhesive plaster round the cylinder until I made it as large as desired. Thus altered, and smeared with cantharides ointment, it was introduced into the ring and allowed to remain seven days. During that time there was a great deal of purulent discharge from around the invaginated scrotum, and, when the instrument was removed, a sloughing ulceration existed at the point of the needle. The plug, of very large dimensions, could be easily felt through the parietes of the abdomen, completely filling up the hernial passage. This patient was discharged, cured, on the 19th day of July. He promised to let me know from time to time as to his condition.

Case IV.—James O'Kane, a native of Troy, New York, stone-cutter by trade, aged 45 years, affected with reducible oblique inguinal hernia of the right side for upwards of twenty years, was operated upon by the same method, on the 10th day of July. The ring and canal, in this case, were also very much distended, and the hernial protrusion was as large as a child's head of three months. The cylinder was again wrapped round with a strip of adhesive plaster until it was made large enough. The instrument was applied and remained in seven days, during which time there was some erysipelatous inflammation about the groin and parietes of the abdomen. This, however, did not interfere with the operation of the instrument, which remained in the required time.

On the 17th July, the instrument was removed, and we found exactly that which we had noticed before in all the other cases—sloughing and ulceration at the point of exit of the needle.

In this case, the plug is a very large one and seems to fill up the canal perfectly. This patient is still in the hospital under my observation, and up to the present time the hernia has not returned.

Case V.—X. Wogensthale, a native of Germany, aged 62 years, presenting a reducible oblique inguinal hernia, of 12 years' standing, was ad mitted into the Charity Hospital in July. Wützer's instrument was applied on the 19th of July. This patient complained of much pain during the application of the instrument, which was removed on the seventh day, June 26th. After the removal of the apparatus, the invaginated scrotum was well agglutinated and formed a perfect plug. This patient is still an inmate of the hospital, although he walks about and assists in cleaning out the ward, etc. He still wears a truss, and the hernia has not returned.

Case VI.—John Nugent, a sailor, native of Ireland, aged 49 years, affected with a reducible oblique inguinal hernia of the right side for two years. Does not know how the hernia occurred. Abdominal muscles thin and weak.

Wützer's instrument was applied on the 29th of July, the canula having been smeared with cantharides ointment. The apparatus was removed on the eighth day, (5th August,) and the parts were found in a satisfactory condition, well agglutinated. This patient is walking about the hospital, feeling quite comfortable, and without a return of the hernia.

REPORT OF A CASE OF POISONING BY STRYCHNIA.

By Orren L. Ellis, M. D., of Trinity, La.

On the 17th of June, 1856, Mrs. L. G., an old woman, who was very dissipated, and was almost blind, called in her servants (there being no white persons on the place) and told them, "Farewell, for the last time;" that she had been taking medicine, and (holding up a bottle of strychnia) that she would never see them again. She also gave directions where they could find her burying clothes, and how she wished them arranged on her body after death. The servants were very much alarmed, and sent a runner after her son who lived about a mile off. On his arrival, he sent immediately for Dr. J. H. P., who lived about a quarter of a mile off. When the doctor arrived, he found her with an ounce bottle of acetate of strychnia grasped tightly in her hand, and about one third the contents missing. On inquiry, he was informed that it was a full bottle she had opened that evening. She presented at the time the following symptoms, viz: Almost the entire muscular system was rigidly contracted, with occasional spasmodic action; intense pain in the stomach and bowels, a sensation of burning in the stomach, and almost constant retching. Dr. P.

dispatched a messenger after me, and gave her zinc sulph. grs. x, ipecac ʒss, at one draught, and awaited its action for a few minutes, during which time the least excitement would produce spasms of the entire muscular system; and she complained of excruciating pains in the stomach, spine and extremities. Emesis coming on very unsatisfactorily, he gave her tart. emetic and sulphate of zinc, ää grs. iv. In a short time emesis was very free, and she vomited about half a gallon of green, bilious-looking matter, complaining all the time of an intensely bitter taste in her mouth and a burning sensation in her throat. I arrived just before vomiting ceased, and we gave her chloroform, ʒi, in a little water, and repeated it in about fifteen minutes. The muscular rigidity soon passed off, leaving her with occasional spasms, for which we gave her chloroform both by the mouth and by inhalation, until they became less severe and farther apart. We then gave her, after a free evacuation from her bowels, a full anodyne dose. She became quiet, and we retired for the night.

June 18th.—Called, in company with Dr. P., this morning, and found our patient doing very well, all symptoms of strychnism having passed off.

Dr. P. called on her again the next morning. Found her sitting on the side of the bed, feeling very well, but weak. He gave her citrate of iron and quinia, three times a day. "She was now reflecting on the past," says the doctor; censured her son for sending for the doctors, and expressed much dissatisfaction at having been thwarted in her designs; inquired for the bottle of strychnia; seemed very much displeased because it had been put out of her way, and said, with an independent air, that she had a plenty more. Dr. P. cautioned her son to search diligently for and remove every particle of strychnia from about the house.

On the evening of the next day, having been left alone with one of her servant girls, she directed her where she could find some strychnia pills, and ordered them thrown into the fireplace. This done, she ordered the girl to go out and prepare some tea for her, which she did, and brought it in. On attempting to sit up to the table and drink the tea, she was attacked with a spasm. She was put to bed, and Dr. P. again sent for. On his arrival he found her almost completely tetanic. There was great opisthotonos, and it was with considerable difficulty he got her to swallow chloroform and laudanum, ää ʒi, immediately after which she expired.

In this case we see the happy effects of chloroform in relieving the tetanic convulsions and muscular rigidity produced by strychnia. There being no positive antidote known, I would sooner rely on the free use of chloroform in case of poisoning by strychnia, than any other remedy with which I am acquainted.

[The quantity of strychnia taken in this instance was extraordinarily large, and the prompt recovery is really wonderful. That the woman died ultimately from a second dose of the poison, seems to us more than probable. While the servant-girl was making the tea, she must either have taken some of the pills thrown into the fireplace, or else she had more of the poison secreted in the room.—Eds.]

DURATION OF LIFE AMONG THE JEWS.

New Orleans, August, 1858.

Messrs. Editors :—In the August number of the New Orleans Medical News and Hospital Gazette, there is published a brief excerpt from Brown-Sequard's Jour. of Phys , under the heading of "Duration of Life among the Jews." The article records the observations of E. Gatters, an Austrian Physician, on the respective longevity among Jews and Christians, in different climates. Gatters adduces Israelites, Germans, Croats and Austrians, and declares the average length of life as being vastly in favor of the first. His calculations are based on statistical data. He attributes this result entirely to the influence of race, whilst the French Journal, entertaining some doubt with regard to the hypothesis advanced, suggests the probability that the phenomenon may result from the fact, that Jews are more wealthy than Christians, and that their hygiene is superior to that of the latter.

To this is added, in brackets, "The Jews in America cannot certainly be considered as superior to Christians in their hygienic condition; they are, in fact, far below the latter in this respect. We have no means of ascertaining the comparative longevity of the two races in this country. —Editors."

Without intending to enter into a lengthy argument, I would simply state, that the thesis so apodictically laid down in the first sentence of this addendum is flatly contradicted in the second. If there "are no means of ascertaining the result," on what grounds can "editors" make a positive assertion? Gatters builds his conclusions on statistical data; his statements bear the stamp of scientific acumen. He may be mistaken in his hypothesis, but his facts and figures cannot be controverted. Even the journal from which you quote, and which, to judge from the police regulations of France, has, doubtless, the means of ascertaining the result in its own country, appears tacitly to coincide with the Austrian savan as regards the calculations furnished, and only differs with him on his theory.

It appears to me, however, that the suggestion of Gatters, "to pay attention to the ethnographic element in etiology," is by no means to be despised, if correctly understood. Ethnical peculiarities, physical or intellectual, may be produced by peculiar modes and habits of living. The cause of the favorable hygienic and macrobiotic condition of the Jews can only be found in the strict observance of the sanitary laws instituted by Moses, and generally observed by Israelites. Those laws refer not only to diet, but also to the most delicate processes and functions of the human organism, all having in view the Mosaic warning, "Ne polluatis animas vestras," and naturally tending to produce a result similar to that reported by Gatters. On the principle, therefore, that like causes produce like effects, an equally favorable state of things may be claimed for the Jews in America.

There is another point in the appended remarks, to which I would respectfully call your attention. I mean the new classification of races. This decidedly novel ethnographic distinction, coupled with the illogical reasoning of the appended paragraph in brackets, leads me to suppose that the latter does not at all emanate from the talented and erudite editors of this journal, but must have been inserted by mistake and without their knowledge.

Very respectfully,

J. K. G.

[The item to which our highly esteemed correspondent refers above, (both the original and that signed "Editors,") appeared amongst the excerpta of our August number. The original we copied as an interesting item, and as one most probably based on reliable data; but it was a "mistake of the printer" to copy the remarks of "Editors," as they were bare assertions, and, therefore, worthless.—Eds.]

SPECIAL SELECTIONS.

On the Pathology of Asthma—*By Hyde Salter, M. D., F. R. S*—There are three motives that have induced me to put together the following thoughts on the pathology of asthma. One is the extreme interest of the subject in a pathological point of view, the beautiful illustration which the clinical history of the disease affords of the phenomena and laws of that particular form of perverted nervous action in which I believe it essentially consists. Another is the importance of correct pathological views in relation to the therapeutics of the disease. A third is a desire to refute the various erroneous theories that have been, and still are, advanced with regard to the pathology of asthma, and to supplant by something precise and definite those muddy and loose notions about it that I find, both from conversation and from their writtings, to be so common among medical men. Some, I find, even now-a-days, do not believe in stricture of the bronchial tubes at all; others do not believe that such a condition would explain the phenomena of the disease; and others again, while admitting that spastic contraction of the bronchial tubes does exist in asthma, believe it to be only a part of a complex condition, that it does not *constitute* the disease, but that some special derangement of the pulmonary circulation shares, equally with it, in the production of the symptoms. I may even add a fourth class—those who deny the existence of asthma as a substantive disease altogether; who affirm that you never have it without organic disease of the heart or lungs, and that it is merely a particular form of dyspnœa induced by that organic disease. No doubt there are many circumstances peculiar to asthma that go far to explain why such vague and erroneous notions should be entertained with regard to its absolute nature, such as the rarity of death in cases of uncomplicated asthma, the slightness and unconspicuous character, or the total absence of anything special and peculiar in the post-mortem appearances in such cases, and the remoteness of the actual morbid condition from the manifest phenomena of the disease. But, at any rate, such is the fact; and if any doubt of it existed, a reference to the literature of the subject, or ten minutes' conversation with any half-dozen members of our profession, would soon dissipate it.

What I shall endeavor to show will be this:

1. That asthma is essentially, and, with perhaps the exception of a single class of cases, exclusively, a nervous disease: that the nervous system is the seat of the essential pathological condition.

2. That the phenomena of asthma—the distressing sensation and the demand for extraordinary respiratory efforts—immediately depend upon a spastic contraction of the fibre-cells of organic or unstripped muscle, which minute anatomy has demonstrated to exist in the bronchial tubes.

3. That these phenomena are those of excito-motory or reflex action.

4. That the extent to which the nervous system is involved differs very much in different cases, being in some cases restricted to the nervous system of the air-passages themselves.

5. That in a large number of cases the pneumogastric nerve, both in its gastric and pulmonary portions, is the seat of the disease.

6. That there is a large class of cases in which the nervous circuit between the source of irritation and the seat of the resulting muscular phenomena involves other portions of the nervous system besides pneumogastric.

7. That there are other cases in which the source of irritation, giving rise to the asthmatic paroxysm, appears to be central—in the brain; consequently, in which the action, though excito-motory, is not reflex.

8. That there is yet a class of cases in which the exciting cause of the paroxysms appears to be essentially humoral.

Let us now examine the propositions in the order in which I have stated them, and see what proofs can be brought in their support.

1. The reasons that force upon one's mind the conviction that asthma is essentially a *nervous* disease are very numerous, and not less forcible and convincing; but I shall here be able only briefly to indicate them, as their fuller consideration would involve too great space. They are principally derived from the following considerations:—*a*, The *causes* of asthma; *b*, its *remedies*; *c*, its *associated and precursory symptoms*; *d*, its *periodicity*; *e*, the *absence of organic change*; *f*, the circumstance that the phenomena of the disease are *muscular*.

a. We see, in the first place, that the *causes* of asthma are such as affect the nervous system, and such as give rise to other diseases acknowledged on all hands to be nervous. Thus, fatigue and physical exhaustion, and sudden or violent mental emotion, will bring on an attack. I was informed, some short time since, by my friend, Dr. Theophilus

Thompson, of a case in which, on two occasions, severe asthma was brought on in a gentleman by sudden fear, from his having, as he imagined, administered accidentally something deleterious to his wife. I knew the case of an asthmatic boy, some years ago, who used constantly to be warned by his parents not to over-excite himself, as if he did he would be sure to have the asthma the next day; and lately I met with another case, in which I was told that when the asthmatic was a little boy, he found in his disease a convenient immunity from correction; "don't scold me, papa," he would say, if he had incurred his father's displeasure, "or I shall have the asthma;" and so he would; his fears were as correct as they were convenient. Venereal excitement will bring on asthma; a gentleman once told me that one of the severest attacks he ever had in his life was brought on in this way. Moreover, many well-known and recognized causes of asthma can only act on the lungs through the intervention of certain parts of the nervous system; thus, gastric irritation can produce spasm of the bronchial tubes only through the intervention of the pneumogastric nerve--it is the only connecting link between the two organs, either physiologically or anatomically.

b. Again, the *remedies* of asthma are such as appeal to the nervous system—as antispasmodics, sedatives, direct nervous depressants, etc.; tobacco, for example, stramonium, antimony, chloroform. Perhaps the effect of chloroform is, of all remedies, the most striking, and at the same time the most illustrative of the purely nervous nature of the affection—a few whiffs, and the asthma is gone; a dyspnœa that a few seconds before seemed to threaten life is replaced by a breathing calm and tranquil—it acts like a charm. Now, remembering the action of the drug, that it is the nervous system to which it appeals, it is impossible to help seeing in this the most conclusive proof that the symptoms are due to a nervous cause. And besides these ordinary remedies, there are other circumstances that will put a stop to the paroxysm, that eminently prove its nervous nature; one of these is mental emotion—any strong or sudden passion, such as fear, fright or surprise. It is a curious thing that mental emotion should have the effect both of inducing and relieving asthma, but so it is; and we are not without analogies—e. g., shock will bring on chorea, and shock will cure it. I think the *immediate* effect of emotion is always to cut short asthmatic spasm, if it exists, by a sort of nervous revulsion; whereas its tendency to induce it is *remote*, and only shown after some time, as the next day; and it acts, I think, by producing an exaltation of nervous impressibility—and thus facilitating the induction of excito-motory action. Nothing, indeed, in the whole range of pathological phenomena is to my mind more remarkable than the effect of emotion upon asthma. Dr. Todd has told me that he has had patients come to him who have lost their asthma the moment they have entered his house; suddenly, and without any apparent cause, except the mental perturbation at being within the precincts of the physician, the difficulty of breathing has vanished. We see just the same thing in tooth-ache—the sight of the dentist's house is enough to cure it. I witnessed once myself so striking an example of this sudden disappearance of asthma under the influence of alarm, that I cannot forbear relating it.

A gentleman, a confirmed asthmatic, was suffering an unusually bad attack of his complaint, so bad that he was unable to move from his chair, or speak even, except in catchy monosyllables. He had been suffering all day, and in the evening his sister was going to give him an emetic of ipecacuanha, when she suddenly fell down in an hysterical fit, to the occurrence of which she was subject. The suddenness of her attack, and the severity of her sufferings, so alarmed him that he sprang from his chair and ran to her relief, and as soon as he had placed her in a position of safety, ran down two flights of stairs to procure the restoratives that were usually administered; having run up stairs again with the same speed, and applied the remedies, he found to his surprise that his asthma was gone, and indeed it was its sudden departure under the influence of alarm that had alone enabled him to perform such a feat; a man who, two minutes before, was unable to speak or move, had, under the influence of an absorbing alarm, ran down two flights of stairs and up again, and found himself after his exertion breathing with perfect freedom. The asthma gradually returned, and within an hour he was as bad as ever. I do not think it possible to adduce a stronger proof than this of the purely nervous nature of asthma. I might cite many such cases.*

c. Again, the *periodicity* of asthma implies its nervous nature—that is, such a periodicity as characterizes asthma. There are three kinds of periodicity in disease. One, in which it is produced by the periodical return of its cause, as in the recurrence of hay-fever every summer, the morning expectoration after a night's rest, indigestion every day at a certain time after dinner. Another, in which the periodicity seems to depend upon that rhythmical impress which is stamped on the functions, that sort of diurnal oscillation in which the body is swung by the constant recurrence, at one unvarying daily interval, of the habitual actions and passions of the body; I think that hectic and ague acquire their periodicity from this diurnal beat into which the body falls. But there are other diseases whose rhythmical recurrence cannot be explained on either of these suppositions, whose periodicity has no relation either to the diurnal interval or to the renewal of the cause, but which must be intrinsically periodic; such are epilepsy and asthma. In these the interval is long and of no certain standard—that is, though tolerably constant in the same individual, it differs very widely in different cases—the period is peculiar to each case, is an integral part of the pathological condition. This last kind of periodicity, and this alone, it is that points at all to the nervous nature of a disease.

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* A remarkable exception to this curative power of excitement once came under my observation. in the case of a woman who suffered from a volent attack of asthma the whole of the time she was in labor.

d. Furthermore, the associated and precursory symptoms of an asthmatic attack are such as point to its nervous character. The quantity of limpid water passed in the early part of the paroxysm, white as pump-water, like the nervous water passed in the students' "funking room," or like the urine of hysteria, or that of nervous headache; the neuralgia, which I have often noticed; the frontal headache; the drowsiness and languor of the previous day by which the approach of the attack is foreknown; or, on the other hand, a peculiar and unwonted hilarity and animation and sense of health,—all these are just such symptoms as we meet with in various diseases of the nervous system, such, for example, as hysteria and epilepsy.

e. Another circumstance in favor of this view, in fact consistent with no other, is the possible absence of appreciable organic change, as shown by post-mortem examination, in cases where the disease has not been of long standing. A man may have been known during his life to have had attacks of asthma, he may have seemed over and over again almost *in articulo mortis* from want of breath; and yet, if death from some other cause† gives an opportunity of examining his lungs, they may be found apparently in every way healthy—no trace of inflammation or its products, the vesicular structure perfectly normal, the passages leading to it lined by a healthy and unchanged membrane, the cavities of the pleura free from all abnoral contents, their surfaces smooth and apposed, the heart sound. The disease shows no cause, and has left no trace, either in the respiratory or circulatory systems—in fact, no trace anywhere. Where, then, shall we locate it! What is its starting-point? We may, I think, lay it down as a rule, that all those diseases that leave no organic trace of their existence produce their symptoms through the nervous system.

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† I say *from some other cause,* because if asthma kills, it always does so by producing organic change in the heart or lungs, or both.

f. Lastly, the phenomena of the disease are muscular, the proximate diseased condition is situated in the muscular system, and whenever the proximate derangement is muscular, we may always, with one or two exceptions, safely affirm that the primary disease is nervous. In epilepsy, tetanus, chorea, paralysis agitans, hemiplegia, child-crowing,—in all these, the obvious departure from health is in the muscular system; but the essence of the disease is nervous. The only exceptions that I know of are, the cases in which the muscles are either poisoned by some material present in them as, for example, in the paralysis of lead-palsy, the cramps of cholera; or disorganized by fatty degeneratien, as we see in the heart, the muscles of diffused limbs, etc. In these cases, the disease is radically and primarily muscular. In all other cases, muscular disturbance is but the index of nervous disease. Hence the very fact that the phenomena of asthma are muscular, is all but proof positive that the nervous system is the seat of the primary derangement.

2. *That the phenomena of asthma—the distressing sensation and the demand for extraordinary respiratory efforts—immediately depend upon a spastic contraction of the fibre-cells or organic muscle, which minute anatomy has demonstrated to exist in the bronchial tubes.*

Although this is a proposition that many perhaps might think it hardly worth while to set about proving yet I think it will be well not to assume it, partly for the reasons I have already mentioned (the general absence, namely, of precise pathological views on the subject), partly because it is a necessary stepping-stone to the succeeding propositions, and partly because I think its proof will be the best way of expressing my notions of the ultimate pathology of asthma, and my reasons for them. It will certainly be an advantage if it can be shown beyond cavil that spasmodic stricture of the bronchial tubes is the only possible cause of asthma, that it is adequate to the production of all the phenomena, that it is a form of perverted physiology that may exist pure and uncomplicated with any organic disease, and that the view that would assign it as the sole essential condition in asthma is—what all pathological views should be—physiological and rational. I think perhaps the eliminative or exhaustive method of proof will be as good as any.

I will suppose a case of severe uncomplicated asthma, such as we sometimes see. Now, what have we here? We have, as the sole constituent symptom, *dyspnœa*—dyspnœa of a peculiar kind—sudden in its access intense, and agonizing, following a state of perfect apparent health and ease, and relapsing as suddenly, possibly, and speedily, and perhaps without any expectoration, into ease and tranquility again. What, then, can give rise to such phenomena as these? We know that the only way in which such an arrear in the respiratory changes as produces a sense of dyspnœa can be brought about, is by a derangement of the supply of one or both of the two fluids, the air or the blood, or by a disorganization of the functioning portion of the lung. On what recognized diseases, then, can we fall back, as supplying in such an instance the necessary conditions? On heart-disease, possibly, bronchitis and emphysema. But if we examine the heart, we find it all right,—it cannot then be that. The mucous membrane of the air-passages could not assume and relinquish a condition of inflammation so suddenly; and moreover, to produce such dyspnœa, the inflammation must be intense, and could not fail to give rise to the results of inflammation, yet none such are thrown out. There is not necessarily any mucous exudation; crepitation or expectoration may both be absent. Besides, it would be impossible for bronchitis to exist to such an extent as to give rise to the amount of dyspnœa, without producing the constitutional signs of inflammation; but none such are present—the patient is not ill, he is wheezing and laboring—he passes from a state of health to a state, not of illness, but of dyspnœa, and back again from dyspnœa to health: there are no sequelæ, there is no convalescence. It cannot therefore be bronchitis. Emphysema we know it is not, for the dyspnœa of emphysema is constant and unvarying, and moreover, we listen to the breathing before and after the attack, and find evidence that the spongy structure of the lung is perfectly healthy. We see, then, that in none of the three ways in which dyspnœa is ordinarily produced —on the side of blood-supply, on that of air-supply, or on that of injured functioning structure,—by heart-disease, emphysema, or bronchitis respectively—can the symptoms of asthma be explained. Moreover, the character of the dyspnœa is altogether peculiar; it is utterly unlike either of the three dyspnœas that have been mentioned. Heart-dyspnœa is intolerant of the slightest exertion, or of the recumbent position, and sitting up, or stillness, may cure in two minutes the most violent paroxysm. The breathing, too, has rather a panting and gasping than the wheezing, laboring character of asthma. Bronchitic dyspnœa is short, crepitous, and accompanied with cough; asthma, often long-drawn, dry, and without cough. In pure emphysema, the dyspnœa is abiding, varies but little, and has no wheeze.

But the dyspnœa of asthma tells a plainer tale than this; it tells us not only what it is not, but what it is. It gives the most positive evidence of narrowing of the air passages. The asthmatic's breathing is what our forefathers called "strait," what we call "tight;" he feels as if a weight were on his sternum, as if his chest were compressed, as if a cord bound him, as if it would be the greatest relief to him if some one would cut his breast open and allow it to expand; he rushes to the window to get air, he cannot tolerate people or curtains about him, his clothes are loosened, and all the muscles of respiration tug and strain their utmost to fill his chest. But he can neither get air in or out, he can neither inspire or expire—his respiration is almost at a dead lock; he cannot blow his nose, can hardly cough or sneeze, cannot smoke a pipe, and if his fire is failing, cannot blow it up; he has hardly air enough to produce the laryngeal vibrations of speech. The chest is distended, indeed, to its greatest possible limit, the cavity of the thorax is enlarged both in the costal and diaphragmatic directions; the costal distension is shown by the fact that a waistcoat that would ordinarily fit will not meet over his chest by two inches, while the descent of the diaphragm is shown by the increased girth of the abdomen and by the heart being drawn down to the scrobiculus, where it is seen beating plainly; such are the violent instinctive efforts of the respiratory muscles to overcome the obstruction to the access of air. But they are unavailing; the air that is without cannot get in, and that which is within is locked up. In spite of the violent muscular effort there is hardly any respiratory movement, the parietes of the chest cannot follow the action of the muscles; on listening to the chest, the respiratory murmur is inaudible, even when not drowned by the wheezing; respiration is almost *nil.* Where, then, can this obstruction to the introduction and exit of air be? It must be in some part of the air passages—the larynx, trachea, or bronchial tubes. In the larynx and trachea we know, from the symptoms, it is not. The fact of bronchial stricture, then, is certain.

The very intensity of the dyspnœa, too, its agonizing and laborious character, implies that the seat of the mischief is in the air-passages. Dyspnœa is essentially remedial, and tends directly, both by its sensory and muscular phenomena to diminish and relieve its cause. As soon as respiration is not going on satisfactorily, the sense of dyspnœa or want of breath, at once prompts to more violent respiratory efforts, which tend to relieve it. The distressful sensation is an essential link in the chain—it gives warning of the condition to be remedied, and is the irresistible stimulus to the remedial efforts. But this sense of dyspnœa, being in its nature remedial, would be likely to be felt only in those cases in which the condition giving rise to it could be remedied by those extraordinary respiratory efforts to which it irresistibly prompts. Now, consistently with this view, I think I have noticed a very curious law with regard to dyspnœa—it is this, that it is proportionate, not to the amount of injury done to the organ, but to the amount of relief that the condition admits of by extraordinary respiratory efforts. If the parenchyma of the lung, its functioning structure, is injured, no amount of respiratory effort will better the condition, and accordingly violent dyspnœa is not induced. Thus, half the lung may be destroyed by phthisis or solidified by pneumonia, and the tranquility of the respiration be hardly interfered with : a little hurried, perhaps, but no distress or violent effort. But if, while the lung-substance is healthy, the free access of air is prevented, violent and distressing dyspnœa is immediately induced—as in croup, laryngitis, the sudden infarction of a large bronchus. For here, if the air could only be got in sufficient quantity to the healthy functioning structure, the balance of the function would be completely restored; hence it is that such cases are always characterized by those violent respiratory efforts which have for their object the freer introducton of air, and that urgent sense of want of breath which is the constituted stimulus to these efforts. We recognize, therefore, in the very urgency of asthmatic dyspnœa, evidence that the mischief is in the air-passages, and that it is of such a nature as to shut off the air-supply.

But the *sounds* of asthma give us, perhaps, still more certain and circumstantial evidence as to the condition of the bronchial tubes. We know in health that respiration is noiseless, but that when the breathing becomes asthmatic it is accompanied with a shrill sibilant whistle. We know, too, that hollow tubes give no musical sound, when air rushes through them, if they are of even calibre, but if they are narrowed at certain points, if their calibre is varied, the air in them is thrown into vibrations, and they become musical instruments. The wheezing of asthma, then, is as positive evidence of bronchial contraction as if we could see the points of structure—it is physical demonstration.

Now, in what ways may the bronchial tubes be narrowed? In four, I think: by a plug of tenacious mucus, partly closing the passage; by congestive or inflammatory thickening of the mucus membrane; by plastic exudation thrown out in the sub-mucous areolar tissue in severe bronchitis, and undergoing subsequent slow contraction (as we see in œsophageal and urethral stricture); and by contraction of the circularly-disposed organic muscle which exists in the bronchial wall. This last is spasmodic stricture; the other three are not; the first is no stricture of the tube at all; and the second and third are inflammatory stricture; the second recent, vascular, and mucous; the third old, fibrous, and sub-mucous. In all these ways the column of air in a bronchial tube may be constricted, and the tube converted into a musical instrument—the seat of a sound that will be rhonchus or sibilus, of high or low pitch, according as the tube is large or small. Now, which is the cause of the sound in the cause before us—the sibilus of asthma? The sibilus depending on a plug of tenacious mucus sticking to the side of the tube is generally (always *ultimately*) relieved by coughing; the sibilus of asthma is never affected by coughing. Vascular tumidity of the mucous membrane can never be dissociated from the symptoms of existing bronchitis, and the sibilus arising from it is not of transient appearance and disappearance; the sibilus of asthma, however, may come one minute, and the next be gone, and is ever changing; moreover, the signs of bronchitis are absent. The sibilus arising from the contraction of plastic exudation thrown around the tube is unvarying and irremediable—a permanent condition, and must have been preceded by some recognized attack of severe bronchitis; the wheeze of asthma, on the other hand, ceases with the paroxysm, and there need not have been bronchitis in any part of the previous history of the case. We have thus got rid of three of the possible causes of sibilus—we have seen that in the case before us (asthmatic wheezing) it cannot be produced by mucous plugging, by vascular tumidity of the mucous membrane, or by the slow contraction of old plastic exudation thrown around the tube. Muscular spasm alone remains. And should we have in this a condition consistent with all the phenomena, and sufficient for their production? Perfectly. The supposition of spasmodic stricture of the air-tubes would explain the sudden access and departure of the dyspnœa, for it is a state that may be instantaneously induced, and may instantaneously vanish; it is consistent with perfect health in all other respects, with the absence of all organic disease or vascular disturbance in the lungs (except that which results from it), with the kind and characters of the sounds generated, with the particular type of the dyspnœa, with the effects of remedies, and with all those circumstances that point to the nervous nature of the disease, such as its causes, the effect of emotion, its periodicity, etc.; for only by the production of muscular contraction of their walls can nervous stimuli affect the condition of the bronchial tubes; everything, therefore, that points to the nervous nature of the disease, points to spasmodic bronchial stricture as its proximate pathological condition.

Thus we see by evidence as certain as sight, that in asthma bronchial spasm must and does exist, and that no other conceivable supposition will explain the phenomena. And we see this independently of that anatomical and physiological support that dissection and experiment supply, and that has hitherto been the chief evidence adduced.

But we find in the muscular furniture of the bronchial walls and the nervous furniture of the whole bronchial system, a valuable confirmation of the correctness of these views, both negatively and positively; for while their absence would be a sad stumbling-block in the way of our inferences from other evidence, their presence supplies exactly the required machinery. Nay, more, it is the most positive proof that could possibly be, that muscular contraction of the bronchial tubes does take place. For, what is the purpose of circularly-disposed muscle, if not to vary the calibre of the tube it invests? The muscular coat of the bronchiæ consists of circularly-disposed bands of fibre-cells, forming a continuous layer immediately beneath the mucous surface; these fibre-cells may be seen in tubes of great minuteness—as small as one-tenth or one-twelfth of a line in diameter. The nervous system of the lungs is derived from the vagus and the sympathetic and the anterior and posterior pulmonary plexus, and is from these origins furnished with wide-spread and varied connections. It is the wide-spread nervous connections that can alone explain some of the phenomena of asthma to which I shall have presently to refer. The nervous system of the lungs, thus derived, consists of ramifying plexuses, supported by the bronchial tubes as upon a scaffolding, and conducted by them to every part of the lungs. These plexuses form a sort of network, investing the bronchial tubes even to their finest ramifications, and are furnished with microscopical ganglia.

But besides this anatomical evidence, we have the positive proof of direct experiment; for Volkman, C. J. B. Williams, and others, to whose accounts I must refer the reader, have clearly shown that the bronchial tubes undergo contraction, in some cases even to complete occlusion, from the application of various stimuli, both to the tubes themselves and to the trunks of the pneumogastric nerves. This completes the chain of evidence.

3. *That the phenomena of asthma are those of excito-motory or reflex action.*

Whenever the peripheral application of a stimulus results in muscular motion, we say that the phenomena are reflex. And so they are, universally. As far as our present knowledge goes, we believe that a stimulant applied to a sentient surface or organ must first be transmitted to a nervous centre by incident, and thence reflected by motor filaments, before it can affect the muscular tissue and stimulate it to contraction. The nervous centre may be a ganglion of microscopical minuteness, and the filaments emanating from it to their peripheral distribution may be of extreme shortness; but still, however near the seat of movement may be to the seat of stimulation (and this may be completely concident), such a centripetal and centrifugal course, and such an intervention of a centre, are essential. We see a very good example of this kind of reflex nervous action in the peristaltic movement of the intestines. In this case the stimulus travels along a perceptive filament to one of the ganglia of the abdominal portion of the sympathetic; there it comes into relation with a motor filament, and is by it transmitted to the muscular wall of the intestine. Of just such a nature is the contraction of bronchial tubes in obedience to sources of irritation applied to their internal surface: the filaments distributed to the mucous surface receive the impression, along them it travels to some of the scattered ganglia of the pulmonary plexuses, and thence returns by motor filaments to the bronchial muscles to which they are distributed. This is the normal function of the bronchial nervous system; it is for the production of bronchial contraction, in obedience to stimulus thus applied, that it is especially organized. It is by this reflected path that the surface-stimulation arrives at and contracts the muscular wall. It is in this way that the bronchi know when and where to contract; that a plug of mucus produces a circumscribed strait through which cough drives it with greater force; that exudation occupying the capillary bronchial tubes is expelled by their peristaltic contraction; that offending material that has found ingress through the glottis is shut off by bronchial stricture from reaching the ultimate lung-structure—supposing, that is, such actions really take place.* In asthma from the effluvium of hay, and of certain animals, as cats and rabbits; asthma from inhaling the emanations from ipecacuan powder; asthma from breathing ammoniacal or carburetted fumes; asthma produced by certain airs; asthma complicating bronchitis,—in all these the bronchial spasm is of this natural, physiological character; the seat of the application of the stimulus, and its reflected path, being the same as that by which ordinary stimuli arrive at, and produce contraction of, the bronchial muscle.

But one of the peculiarities of asthma is that it may be induced by stimuli applied to remote parts; in these cases the nervous circuit is much longer, and the phenomena of reflexion clearer and more conspicuous. Take, for example, that most common of all the varieties of asthma, what we may call *peptic asthma,* in which the induction or prevention of attacks is entirely controlled by the state of the digestive organs, in which an error in diet—the eating some particular thing, eating too largely or late in the day—is sure to bring on an attack; while a certain dietetic abstention is as certain to be attended with immunity from the disease. Here the reflex character of the phenomena is clear, and the nervous circuit by which the reflexion is completed conspicuous and evident. I think there are three degrees of remoteness of the application of stimulus producing asthma, and thus three groups into which we may divide these clearly reflex cases.

1st. Those that I have just mentioned, in which the source of irritation is alimentary, and generally gastric. Here the nerve irritated is the gastric portion of the pneumogastric, by which the stimulus is conducted to the medulla oblongata; this is probably the seat of the central reflexion, and transmits the stimulus immediately to the lungs by the pulmonary filaments of the same nerve, the bronchial muscles contracting in obedience to this reflected stimulation, just as they would have done if it had been primarily pulmonary. Here we have only one nerve concerned—the pneumogastric, but two portions of it, one of which plays an afferent and the other an efferent part, while the portion of the centre involved is confined to the origin of the nerve—to the seat of implantation of these respective gastric and pulmonary filaments.

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* For a further discussion of this point I must refer the reader to the concluding paragraphs of this paper.

2nd. Those cases in which the irritation is more remote, but is still confined to the organic system of nerves—e. g., asthma produced by a loaded rectum.

3d. Where the cerebro-spinal system is the recipient of the irritation which is the provocative of the attack—for example, that remarkable case related to me by Dr. Chowne, in which the application of cold to the instep immediately produced the asthmatic condition.

Of these two last groups of cases, I know of very few examples, and I may dismiss them with a few words. A case came under my observation some years ago in which a patient could regulate his asthma entirely by the condition of his bowels. They were, as a rule, relieved every evening; if the customary relief took place, and he retired to bed with an empty rectum, he awoke the next morning well; but if he neglected to relieve his bowels, or his efforts to do so were abortive, he was quite sure to be awoke towards morning by his asthma. Strange as this may appear, it is strictly true. Dr. Copeland remarks that the attack is often preceded by costiveness and inefficient calls to stool—an observation quite in accordance with the case I have just related. I should be disposed to think myself that these were not only precursory and premonitory symptoms, but that they had something to do with the causation of the asthma that followed them—that the attack had a *propter* as well as a *post* relation to them. Of a strictly analogous nature are those cases of hysterical asthma in which the attacks are preceded by recognized symptoms of uterine irritation. The remarkable case communicated to me by Dr. Chowne, to which I have already referred, was as follows: J. G., a man of about fifty years of age, made application to an insurance office for the assurance of his life. In reply to the questions of the physician of the office, he stated that he was liable to spasmodic asthma. He stated that he was subject to these attacks if by any accident cold water fell upon his instep, or his instep became cold. The impression on the mind of my informant, who was the medical man who examined him, was that this was the commonest, but he is not sure it is the only, cause of the attacks. The asthma came on suddenly and *immediately*, and the attacks were very severe. The circumstances were considered so curious, that great pains were taken thoroughly to sift the case, and the result was that the facts were clearly established, and the man's life refused in consequence.

But while cases illustrating in this remarkable way the excito-motory nature of asthma, and the distance from which stimuli may reach and influence the innervation of the lungs, are rare, cases of peptic asthma, in which the attacks are caused by pneumogastric irritation, are so common that I think few cases could be found of true spasmodic asthma in which the disease is uninfluenced by the state of the digestive organs, while in a very large number it is under their control. This fact is so patent and so generally recognized that it has by many writers been made the basis of their classification of asthma; thus Dr. Bree and Dr. Young erect into a distinct species those cases that are dependent on gastric irritation. Therapeutically, the full appreciation of this fact is most important; more is to be done for our patients on the side of the stomach than in any other direction. An observant and thoughtful physician once said to me that he considered dietetic treatment *the only* treatment of asthma.

But there is yet again another class of cases that have suggested to my mind the belief that asthma is sometimes *central*, not reflex, in its origin; that it may originate in irritation of the brain itself, or the spinal cord. The two following cases appear to me to be examples of this kind of "*central*" asthma. The first was communicated to me by my brother, Mr. James Salter, and occurred under his own observation. The patient was a boy of about ten years of age, and the disease acute hydrocephalus, which ran a fatal course in about a fortnight. Five days before his death he was suddenly seized with an attack of dyspnœa of the asthmate kind; it was very severe, lasted about half an hour, and then entirely vanished. The following day he was seized with a precisely similar attack; but this was the last; the symptoms never reappearad, and the patient sank in the ordinary way, from the brain disease, about four days afterwards. He had never before suffered from asthma; there were no chest symptoms either before or after the attacks—no cough, no expectoration. My brother is very precise as to the nature of the dyspnœa; he says there was nothing cardiac about it—no panting, no orthopnœa—but that it had the laboring "difficult" character of asthma. I conceive that in this case the bronchial spasm was a phenomenon of deranged innervation from central irritation analogous to the jactitations, rigidity, and convulsive characteristic of hydrocephalus.

The other case was that of a man about fifty years of age, subject to epilepsy. His fits had certain well-known premonitory symptoms, and occurred with tolerable regularity, I think about once a fortnight. On one occasion his medical attendant was sent for in haste, and found him suffering from violent asthma; the account given by his friends was, that at the usual time at which he had expected the fit, he had experienced the accustomed premonitory symptoms, but instead of their being followed, as usual, by the convulsions, this violent dyspnœa had come on. Within a few hours the dyspnœa went off and left him as well as usual. At the expiration of the accustomed interval after this attack, the usual premonitory symptoms and the usual epileptic fit occurred. On several occasions (I do not know how many) this was repeated, the epileptic seizure being, as it were, supplanted by the asthmatic. Of these four points my informant, who was the medical attendant, seemed certain: that there was nothing amiss with the lungs either before or after the attacks, that the character of the dyspnœa was asthmatic, that each attack of asthma occurred at the usual epileptic period, and that they were preceded by the premonitory symptoms that ordinarily ushered in the epilepsy. I think such a case admits of only one interpretation—that the particular state of the nervous centres, that ordinarily threw the patient at certain periods into the epileptic condition, on certain other occasions, from some unknown cause, gave rise to bronchial spasm; that the essential diseased condition was one and the same, but that its manifestations were altered, temporary exaltation and perversion of the innervation of the lungs supplanting unconsciousness and clonic convulsion. Bearing in mind the many points in their clinical history that asthma and epilepsy have in common, this case is one of peculiar interest.

To this same category of *central* asthma we must, in strictness, assign those cases in which the paroxysm is brought on by violent emotion, as in that remarkable instance I have related of the gentleman who had, on two distinct occasions, violent spasmodic asthma suddenly induced by alarm, from the fear that he had poisoned his wife. In such a case, the seat to which the stimulus is primarily applied is the brain itself.

Lastly, there is a class of cases in which the exciting cause of the paroxysm appears to be essentially *humoral.* I have stated that the most frequent of the exciting causes of asthmatic attacks are alimentary, and that an error in diet, or the mere introduction of food into the stomach, produces bronchial spasm by reflex stimulation, through the intervention of the pneumogastric nerve. But is this the only way in which the lungs can be affected by what is put into the stomach? No. Although the pneumogastric nerve is the only single structure that has a distribution common to both organs, yet the venous system affords a very close and intimate bond of connection between the stomach and the lungs; for, any rapidly absorbable material introduced into the stomach, is at once taken up by the venous radicles of the gastric mucous membrane, and within a few seconds, having passed through the liver and the right side of the heart, finds itself in the pulmonary circulation. In this way the blood in the lungs is liable to constant change in its composition, from admixture with it of the different materials thus taken up by the gastric veins; and, from the absence of secernment or elective power on the part of these veins, is ever at the mercy of the food. The chief parts of the normal results of healthy digestion, or the morbid results of depraved digestion, and that numerous class of bodies which are at once taken up without any change, are thus thrown directly upon the lungs. In the intestines we have an additional channel for the introduction of the contents of the alimentary canal into the pulmonary circulation, namely, the lacteal absorbents. It is, I believe, in this way, by the actual presence in the vessels of the lungs of the materials taken up from the stomach and intestines, that the introduction of food into the alimentary canal frequently gives rise to bronchial spasm. A contaminate blood is the irritant, and excites the bronchial tubes to contract through the intervention of the pulmonary nervous system, just as the effluvium of hay or an irritating gas would. In one case the irritant affects the surface to which the nerves are distributed, in the other, the capillaries among which they lie. When I say a "contaminate" blood, I do not mean that the material present in it is necessarily peccant. I believe it may be perfectly normal, and yet produce asthma. I believe that digestion may be everything that it should be, and its results in no way different from that of a perfectly healthy person, and yet they shall, in an asthmatic, produce asthma the moment they arrive at the lungs. I believe this because some of the materials that give rise to asthma are such as undergo no change, but are at once absorbed, and must therefore necessarily be identical in the lungs of the sound man and in the lungs of the asthmatic, and also because many persons who are rendered asthmatic by taking food, exhibit no symptoms of deranged stomach-action whatever. One such case in particular, with which I am very familiar, is that of a lady who every day within a few minutes after commencing her dinner experiences that dry constricted straitness of breathing characteristic of asthma; even if she has her asthma at no other time, she will have it then; after lasting a quarter of an hour, or half an hour, it passes completely off. Now this lady's digestion is remarkably good, unusually powerful and rapid; she is free from the ordinary restrictions of diet that most people are obliged to acquiesce in—radishes, cucumbers, and other unwholesomes, agree with her perfectly well; and the production of the asthma does not depend upon the quality of the food; such things as I have mentioned do not seem to induce it, while she will often become asthmatic during the plainest meal. I believe in this, as in hay, ipecacuan, and other asthma, that the irritant differs not in the asthmatic and the healthy person, but that the essential difference is in the irritability of the pulmonary nervous system—that it resents that which it should not resent—that its morbid sensitiveness exalts that into a stimulant which should not be a stimulant, and that thus the pulmonary nervous system registers (as it were) on the bronchial tubes those changes in the constitution of the pulmonary blood of which it should be unconscious.

There are, however, certain articles of diet which, either from their being peculiarly offensive when materially present in the lungs, or apt to give rise to dyspepsia and its vitiated results, or specially irritating to the gastric portion of the vagus, are very apt to give rise to asthma. Such asthmatic articles of food are—cheese, nuts, almonds and raisins, and sweet things in general, salted meats, condiments, potted and preserved and highly-seasoned things, fermented drinks, especially malt liquors and sweet wines. I think malt liquor, especially the stronger sort, with a good deal of carbonic acid gas in it, is perhaps the most asthmatic thing of any; next to that I should place raisins and nuts. I know the case of an asthmatic gentleman who cannot eat a dozen raisins without feeling asthmatic. But there is great caprice about asthma in this respect, strongly marked idiosyncrasies in individual cases. Thus, in one case, a single glass of hock would invaribly bring on an attack, though any other wine might be drunk with impunity; in another, Rhine and Bordeaux wines—hock and claret—were the only ones that could be drunk; a dinner at which they alone were partaken of was sure never to be followed by atshma, but if port or sherry were drunk, the asthma would infallibly come on within an hour or two. In another case the whole mischief of a dinner, its sting, lay in its tail, in the usual post-prandial coffee; if that vicious drink was declined, no harm came of the dinner, but if it was partaken of, on came the asthma.

But why, it may be asked, do I choose to adopt the opinion that these different alimentary materials produce asthma by their material presence in the blood of the lungs, consequent on their gastric absorption? Why is not the supposition, that they act as irritants to the gastric portion of the vagus sufficient for the explanation of all cases? I do not say positively that it is *not* sufficient; I do not say it is not the sole way in which all these articles of diet excite asthma. But there is in particular one circumstance that makes me think that some of these materials at least, and in some cases, act by their presence in the lungs themselves, and it is this—that they induce the asthma in just such time as they would reach the lungs in subsequent to their absorbtion; the interval between the taking the material and the supervention of the asthma will be long or short, according as the absorption of that particular material is immediate or deferred. Thus in a case which once came to my notice, in which the taking of wine or any alcoholic drink was always followed by asthma, the asthma was immediate, within a minute or two; while in another case, in which the food producing the asthma was such as would furnish material for lacteal absorption, the asthma did not come on till about two hours after taking the food, that is, when the chyle would be beginning to reach the blood being poured into the lungs.

But we must not forget that asthmatics are very commonly dyspeptics, and often exhibit symptoms of perverted and capricious stomach action, that suggest to one's mind the belief that the innervation of the whole of the vagus is vitiated, its gastric as well as pulmonary portion, and that the dyspeptic and asthmatic symptoms are but parts of a whole. I could mention some very interesting cases illustrative of this, if space permitted.

What I would wish, then, to express on this subject is—that I believe it possible that asthma is sometimes produced by particular materials admixed with the blood in the lungs, and that therefore it is so far *humoral;* but these particular materials—whether absorbed unchanged, as alcohol, ethers, and saline solutions, or the results of healthy digestion, or of perverted digestion—have nothing particular in them, but are the same as they would be in any non-asthmatic person, and that the essence of the disease in these cases, as well as in all others, consists in a morbid sensitiveness and irritability of the pulmonary nervous system.

Great and valuable light is often thrown on pathological questions by considering the laws of the physiology of the part concerned, for pathology is often but deranged physiology, and pathological aberrations in strict subservance to physiological laws. Let us, then, see if we can detect in the probable purpose of the muscular endowment of the air tubes, an explanation of any of the phenomena of asthma. What *is* the purpose of the muscular contractibility of the air passages?

This is a question that has been variously answered, and whose certain solution is beset with considerable difficulties. In one light we may look upon the bronchial system as the ramifying efferent ducts of the great conglomerate gland the lungs, of whose excretion, carbonic acid, it affords the means of out-draught. Now the ducts of all large glands are furnished with organic muscle—liver, pancreas, salivary glands, kidneys, ovaries, so that the existence of muscular elements in the walls of the bronchial tubes is in strict conformity with anatomical analogy. But when we pass on from anatomy to function all analogy ceases. The purpose of the muscular endowment of the ducts of glands in general is the expulsion of the secretion; little waves of vermicular contraction pass along them, always in a direction *from* the gland, and thus their contents are driven along towards the orifice. Such cannot be the purpose of the muscularity of bronchial tubes, for they are permanently and necessarily patulous; indeed, throughout their greater length a special arrangement is adopted by which their closure shall be effectually prevented, by means of the little flakes of cartilage scattered throughout their walls. In the larger tubes these are such and so placed that only a slight amount of contraction is possible; they are continued down, in increasing tenuity and scantiness, to tubes of a smallness of half a line in diameter, and wherever they exist perfect closure is impossible; so that in none except tubes of extreme minuteness can perfect closure take place. But beyond the point of the cessation of cartilage flakes muscle still exists; it has been demonstratad in tubes $\frac{1}{120}$th of an inch in diameter, and probably even in the ultimate lobular bronchiæ; here, of course, perfect closure can be effected.

What are the acts, then, of which the bronchial tubes, thus constituted, are the seat? Respiration and cough. For we must consider cough a normal act. It is the constituted mechanism of expulsion of any particles of foreign matter which may at any time be introduced with the respired air, and against whose ingress the stricture function of the glottis so imperfectly provides. Indeed the respiratory organs have no power of selection, no means of filtering the material on which they are every moment dependent for the exercise of their function; they are ever at the mercy of the air, and of any materials that may contaminate it. The power, therefore, of expelling any foreign or offending particle that may have found entrance, becomes a necessary appendage to respiration. Cough is, no doubt, often a phenomenon of disease, but it becomes pathological from the material on which it is exercised and not from the essential nature of the act. If exercised on blood, pus, or excessive mucus, it is a symptom of disease; if on some foreign particle that has found accidental ingress, it is strictly normal. It is no more pathological than sneezing is pathological, which a particle of dust, or a sun-beam, may cause at any time. What purpose, then, if any, has the muscular endowment of the air passages in relation to these two acts—respiration and cough?

It has been maintained by many that the bronchial tubes contract at each expiration, and so assist in the expulsion of the air. But the character of fluid expelled—gaseous, and the method of its expulsion—quick, transient, and iterated, are neither such as the ordinary slow vermicular contraction of organic muscle would be appropriate for; it is impossible that the bronchial tubes can expel the expired air in the same way as the ducts of glands do their secretion, it is impossible that a wave of contraction can pass from extremities to trunk of the bronchial tree at each expiration; it would be a rapidity of transit entirely at variance with all that we know of the law of organic muscle contraction. Moreover, we know that uninterrupted patulence from glottis to air cell, both in inspiration and expiration, is an essential condition of normal breathing. If there is any bronchial contraction coincident with expiration, it must be a slight narrowing of all the tubes, which would act not as a special expiratory force, but would merely diminish the amount of residual air locked up in the air-passages at the end of expiration.

This slight contraction of the whole bronchial system at each expiration I am, for the following reasons, inclined to believe. I have long observed that rhonchus and sibilant râles are often only audible during expiration; that in inspiration they cease; that they are frequently confined to the end of an expiration, not becoming audible till the expiration is half performed; that the longer and deeper the expiration the louder they are; and that the inaudible respiration of persons in apparent good health, particularly the old, may be rendered wheezing by making them effect a prolonged expiration. I have remarked this, not only with the dry sounds of asthma, but the moist râles of bronchitis; the crepitous wheeze of a senile bronchitis is often confined to the termination of the expirations. If you tell a person with unsound lungs to wheeze, he immediately effects a prolonged expiration, as if he knew that that was the way to produce a wheeze. Now the source of sound—the plug of mucus, or the inflamed tumid membrane—exists as much during inspiration as expiration: why, then, should the sound be present in the one and absent in the other? On what does it depend? Manifestly on the alternate contraction and dilatation of the air passages. The plug of mucus, or the tumid membrane, which does not narrow the tube sufficiently to give rise to a musical sound in inspiration, in expiration does. I do not know how we can explain these phenomena except by supposing that all the bronchial tubes undergo contraction during expiration, and thus magnify the effects of any sources of inequality in their calibre; for a plug of mucus, for example, that would form hardly any impediment to the passage of air in a wide and patulous tube, would form a considerable barrier, and throw the air into strong vibrations, if that tube were in a state of contraction. Moreover, I have noticed (and this is a very curious fact) that when the breathing is inclined to be asthmatical the dyspnœa may be aggravated, and the asthmatic feeling very much increased, by a prolonged expiration; while, on the other hand, the spasm may be broken through, and the respiration for the time rendered perfectly free and easy, by taking a long, deep, full inspiration. In severe asthmatic breathing this cannot be done; but in the slight bronchial spasm that characterizes hay-asthma I have frequently witnessed it. It seems as if the deep inspiration overcame and broke through the contracted state of the air-tubes, which was not immediately re-established.

Now all this certainly looks as if expiration favored, and inspiration opposed, contraction of the air-passages—in fact, I think it amounts to positive proof of it. But I am not prepared to say that the bronchial tubes undergo *distinct muscular contractions* at each expiration. I think it possible to explain the phenomena otherwise. I think it possible that the diminution of their calibre at expiration may be due to the constant and unvarying tendency of their muscular and elastic walls to contract. This tendency is antagonized and overcome during inspiration, by which the tubes, like the other contents of the chest, are forcibly distended; in expiration this tendency is no longer opposed, and, like the other contents of the chest, they collapse. Still the assumed active contraction of the bronchi is quite consistent with the phenomena, especially those which I mentioned with regard to asthma—that a long expiration, namely, deepens the spasm, while a full inspiration may temporarily annihilate it. This looks as if bronchial contraction and inspiration were incompatible, and could not co-exist. Still, it would be possible to explain both these circumstances by supposing that prolonged expiration merely *suffered* the tube more completely to yield to the pre-existing asthmatic spasm, while the distension of expiration was too strong for it and overcame it.

In relation to cough, the opinion has been advanced that the muscularity of the bronchial tubes may, by diminishing their calibre, increase the rapidity of the rush of air driven through them by the act of coughing, and thus increase its expulsive power. If this contraction were general and extended to the smaller tubes, the reverse would be the case, for a smaller stream of air would be brought to bear upon the obstructing material. If, however, it is a circumscribed contraction, confined to the situation of the matter to be expelled, then it would be a veritable adjuvant, and the air would rush through the point of narrowing with increased rapidity, and therefore increased expulsive power, just as narrowing an outlet of water clears and deepens the channel. But the contraction being at the seat of the matter to be expelled, and there alone, is an essential condition to this increase of expulsive power. A little glottis is, as it were, formed there, and the material inevitably driven through it.

But there is a third purpose, I think, arising from this very danger of the access of deleterious matter to the lungs, more important than either of the other two to which I have referred, and which I believe to be *the* purpose, *par excellence*, of the muscularity of the air-tubes. It is the guarding the delicate ultimate lung-structure, the shutting off from the air-cells, and preventing reaching them, any deleterious material that may have gained entrance through the glottis. After having passed the glottis, there is no other means by which the further progress of any foreign matter may be arrested; and its arrest ere it reaches the pulmonary structure seems as essential to the well-being of the lung as its subsequent expulsion by cough. We know, in fact, that very little of the foreign particles contaminating the air reaches the air-cells; that it is almost entirely arrested and expectorated; and we know, too, that the respiration of air charged with any irritating or noxious material, such as the smoke of burning pitch, etc., will immediately produce in many people the symptoms of bronchial stricture.

Another office that has been claimed for the muscularity of the bronchial tubes, is the regulation of the supply of air to those portions of the lung in which they terminate, in the same way as the muscularity of the arteries regulates the blood-supply to the capillaries. We can easily understand how, when one lung or one portion of a lung is injured, a relaxation of the bronchial tubes of the other portion would enable them to deliver to it a freer supply of air, and thus capacitate it better for doing double duty. On the other hand, we can imagine how, in certain violent inspiratory efforts, a narrowing of the bronchial tubes would limit the supply of air, and exercise a conservative and protective influence by preventing too great and sudden a distension.

But if we would form a correct idea of the purposes which are stored up in an organ, our attention must not be restricted to its working in health, but must embrace also those exceptional processes that arise in the course of disease—those latent, and, during health, inoperative powers kept in store, as it were, against the emergencies they are to meet—by the possession of which in its different organs, the body becomes not only a self-maintaining and self-regulating, but a self-correcting machine. Have the bronchial tubes any such exceptional office, adapting them to a pathological exigency? Such a one has been assigned them. It has been maintained that when the smaller bronchial tubes become filled with secretions, as they so commonly do in disease—with mucus for instance, as in capillary bronchitis; or with muco-plastic matter, as in pneumonia—they empty themselves of it, and pass it on into the larger tubes by a peristaltic contraction. This would be the acquisition in disease of the same kind of action as the ducts of most glands have in health. There are two strong negative reasons in favor of this view. One is, that there is in these minute bronchial tubes an absence of that anatomical peculiarity that would prevent anything like a peristaltic contraction of the larger ones—namely the flakes of cartilage. The other is, that these ultimate tubes are wanting in the ordinary mechanism of discharge by which the bronchial system keeps itself empty—the *vis à tergo*, namely of cough; for having behind them only so small a volume of air—that contained in the lobule or group of lobules in which they terminate—sufficient explosive force cannot be brought to bear upon the mucus obstructing them—there is no air behind it to drive it forth. The larger the tubes the more effectual the cough becomes, because the greater the portion of lung with which they correspond, and the larger the volume of air on which the parietes of the chest exercise their pressure in the explosive expiration of cough. The ultimate bronchiæ, then, being deficient in this power of clearing themselves, we should naturally expect that some succedaneum would be provided, and this we recognize in the peristaltic contraction of which they are probably the seat. Were it not for this, one can hardly conceive by what mechanism they would empty themselves of the materials by which, as shown by the sounds of respiration during life and post-mortem examination, they are so apt to become infarcted.

Now, can we see in these real or probable purposes of the muscular contractility of the bronchial tubes any explanation of the phenomena of asthma, and any clue to the ultimate pathology of the disease? Yes, I think most clearly. We see that the purpose of this muscular furniture of the bronchial tubes is that they should contract under certain circumstances, and on the application of certain stimuli; and seen by this light we recognize in asthma merely a morbid activity—an excess—of this natural endowment; the tubes fall into a state of contraction, with a proneness, a readiness that is morbid; the slightest thing will throw them into a state of spasm, the irritability of the muscle is exalted, the contraction violent and protracted, that becomes a stimulus, to contraction which should not be, and the nervous and muscular system of the lungs is brought within the range of sources of irritation applied to such distant parts as ordinarily in no way affect them. Any healthy man may have his bronchial tubes temporarily thrown into a state of asthmatic spasm by the inhalation of ammoniacal or carburetted or other irritating gases; but only by such materials, whose exclusion is necessary for the safety of the lung, will this natural asthma be brought about. A greater degree of bronchial sensibility is shown in those cases, by no means uncommon, of what is called "hay-asthma," in which the stimulus to bronchial spasm is the effluvium of hay; a still greater, in those cases much rarer, in which the emanations from ipecacuan powder will at once give rise to asthma; a still greater in that numerous class of cases of asthma in which the disease is called into activity by certain atmospheric peculiarities which are altogether inappreciable, as where an attack of asthma is inevitably brought on by going to a certain place, living in a certain house, sleeping in a certain room. All these cases fall strictly under what we may call the formula of health; they are physiological, they are instances of the contraction of a muscular tube in obedience to stimulus applied to the mucous membrane that lines that tube; the nervous system engaged is the *intrinsic* nervous system of the tubes, its own ganglia and perceptive and motor filaments, as in æsophageal deglutition or intestinal peristalsis; the error is merely a morbid exaltation of a normal irritability. But there are other cases in which the error is more than this, in which the nervous apparatus involved in the phenomena is abnormally extended; in which certain outlying and distant parts of the nervous system are the recipients of the stimuli that give rise to the bronchial spasm, as in those cases to which I have referred, where an attack is induced by an error in diet, a loaded rectum, the application of cold to the instep, mental emotion; in which the gastric filaments of the pneumogastric nerve, the sympathetic, the cutaneous nerves of the foot, and the brain, are respectively the recipients of the stimulus that gives rise to the bronchial contraction. In the former class of cases the bronchial spasm takes place in obedience to the wrong stimulus applied to the right place; in the latter, place and stimulus are alike wrong; the relation of the asthma to its cause is in the one case immediate or primary, in the other, remote or secondary,—mediate, through the intervention of some part of the nervous system extrinsic to the lungs.

In what, then, does the peculiarity of the asthmatic essentially consist? Manifestly, in a morbid proclivity of the musculo-nervous system of his bronchial tubes to be thrown into a state of activity; the stimulus may be either immediately or remotely applied, but in either case would not normally be attended by any such result. There is no peculiarity in the stimulus, the air breathed is the same to the asthmatic and the non-asthmatic, the ipecacuan powder, the hay effluvium, is the same in both; nor, probably, is there any peculiarity in the irritability of the bronchial muscle; the peculiarity is confined to the link that connects these two—the nervous system, and consists in its perverted sensibility, in its receiving and transmitting on to the muscle, as a stimulus to contraction, that of which it should take no cognizance. In those cases where the spasm is produced by some irritant applied to the air-passages themselves, this perverted irritability is confined to the bronchial nervous system. The exact seat of the perverted nervous action in those other cases where the stimulus is remote, is more doubtful. Take, for instance, that case in which cold water thrown on the instep immediately produced asthmatic dyspnœ. Was the cerebro-spinal nervous system in fault here? Did it transmit to the pulmonary a morbid stimulation, or was the fault, as in the other cases, confined to the pulmonary nervous system—to its being morbidly affected by a nervous impression perfectly normal? These are questions that in the present state of our knowledge it would be difficult or impossible to pronounce upon. At any rate, it is clear that the vice in asthma consists, not in the production of any special irritant, but in the irritability of the part irritated.

These considerations, I think, tend to rationalize our notions of asthma, and to impart at once an interest and an order to its phenomena.—*Brit. and For. Medico-Chir. Review.*

EDITORIAL AND MISCELLANEOUS.

Health of our City.—Our readers will perceive that we have little to say editorially. In the first place, our list of exchanges furnishes no items for comment; and then the weather is so depressing, that it is exertion sufficient to attend to the sick. At night, our usual time for driving the quill, we feel much more like resting ourself, both mentally and physically, than working.

The great topic of each day is "the health of the city," and no doubt our readers will be anxious to know what is going on. When we were pushing our last number through the press we thought we had reasonable grounds for hoping that there would be no real epidemic of yellow fever this summer; but, until more is known of the coming and going of this terrible scourge, it is useless to be expressing opinions either one way or the other. If a man makes a guess and hits the mark, he is, of course, a wise man—a very prophet; and yet, so much, or so little does he know about the matter, when he predicts again he misses the mark, and then, of course, he is a fool.

Yellow fever is certainly epidemic in New Orleans, and has been so for some time, as the figures will prove. So far, there have been 872 deaths from this disease alone. Since the 20th of June the deaths have been as follows: 1st week, 2; 2nd, 8; 3d, 9; 4th, 20; 5th, 25; 6th, 70; 7th, 140; 8th, 286; 9th, 312. The highest mortality for any single day has been 78, and we have reason to believe that the disease is on the decline. We write on the 27th of August, and our latest daily report is for 24 hours ending 12, M., of the 25th, being 69. Owing to some unexplained circumstances, there has been some confusion in the daily reports. The Charity Hospital report is made up to six o'clock every evening, while the general City returns at the Board of Health are made up to 12, M., of each day. It seems to us that both reports could, and should, be made up to 6, P. M., of each day; and then the papers could readily furnish the public with *late* data. As it is, we are always a day or more behind the times.

So far as we are able to judge from actual observation, we think the fever of this season quite manageable. We are satisfied that the proper plan is to nurse the patient through, giving as little purturbating medicine as possible, and only *attacking* when symptoms of localization of disease arise. We have little doubt that in our October number, we shall be able to report yellow fever rapidly disappearing from our city.

EYE INFIRMARY IN NEW ORLEANS.—We beg leave to direct especial attention to the advertisement of Dr. Beard's Eye Infirmary. An institution of this kind has long been a desideratum in the South-west, and we are sure its establishment will be hailed with satisfaction by our professional brethren throughout the country. Those may preach against specialties who will, but the longer we live, the more satisfied are we that there are both medical and surgical specialties to which the educated medical man may devote himself with the certainty of being more useful to a community than he could be if he undertook to cover the entire field in his practice. And it is herein that the city physician necessarily possesses advantages over the individual who pursues the practice of medicine in the country. If a man devotes himself to a specialty in the city, there is the material to insure him a support; whereas, in the country, there is not the population to warrant the effort. Hence it is, that there are many affections (and pre-eminent among them stands almost the entire class of diseases of the eye) which even the most intelligent country practitioner will gladly turn over to him who, from daily contact with, and exclusive study of the same, is more capable of both diagnosticating and treating properly.

For several years past Dr. Beard has been devoting his exclusive attention to diseases of the eye, and we can conscientiously affirm that he is the best diagnostician and operator we have ever met with. After having been most thoroughly educated in medicine both in this country and Europe, he resolved to devote himself to this specialty, and he stands unrivalled in our city. His Infirmary is the most complete we have ever seen, no expense having been spared to make it all that can possibly be desired. There is a place for the white man who can afford to pay for luxury; and there are the best accommodations for slaves we have ever seen. At the same time, the charges are as moderate as circumstances will possibly admit.

Such an enterprize as this should not be permitted to fail. It is the first of the kind ever attempted in this part of our country, and, being of inestimable value to the community at large, we confidently predict its entire success.

OBITUARY —Died at San Antonio, Texas, on the 13th of July ult., Dr. Gilbert S. Vance, a native of New Orleans.

In the death of this estimable gentleman, science has lost an ardent votary, and the profession a member who did it honor—one, who, though brief his career, had already placed himself among the foremost of his cotemporaries. Ambitious, energetic, and industrious, with a vigor and quickness of intellect rarely met with, his success in his much loved profession had been already established beyond a peradventure. Friends innumerable, an increasing practice, and the high respect of his confrères were the rewards which he merited and had obtained. Alas! that death should intervene to blight the hopes of the many who loved him.

We knew him well and intimately. As the friend of our early youth, as class mate, as business associate, and as rival, struggling with an honorable emulation for those distinctions which make life desirable, we found him ever warm-hearted, generous and noble.

In the rocky path of life, wherein men, selfish and self-aggrandizing, jostle each other rudely and harshly in their onward way; in this profession of ours, where unkind spirit—aye—bitter feeling, but too often interferes to destroy the harmony which should exist, how seldom do we meet with one like him to whose memory we offer this humble tribute! C. B.

DR. AUSTIN FLINT'S LETTERS ON THE HEART-SOUNDS. — We beg leave to direct especial attention to the letters of Dr. Austin Flint, the first of which we publish in this number. For consciseness and perspicuity we know of nothing like them in our language, and we are confident they will be read with the greatest pleasure, as well as profit.

ROBIN'S MONOGRAPH.—Owing to severe domestic affliction, our friend Dr. Peniston, the able translator of Robin's invaluable Monograph on the Mucous Membrane of the Uterus, has been unable to furnish the remainder of the same for this number of our journal. We have every hope that it will appear in our October number.

REPORT OF DEATHS IN THE CITY OF NEW ORLEANS, FROM 25TH JULY TO 22D AUGUST, 1858. — Men, 712; women, 258; boys, 250; girls, 210; adults, 970; children, 460; males, 961; females, 469; total, 1430. Under 1 year, 116; 1 to 2 years, 49; 2 to 5 years, 116; 5 to 20 years, 94; 20 to 40 years, 765; 40 to 60 years, 167; 60 to 80 years, 35; over 80years, 4.

Principal Diseases.—Yellow fever, 808; consumption, 52; infantile convulsions, 59; Teething, 26; still-born, 27.

Nativities.—United States, 562; foreigners, 649; not stated, 219.

Brought to the city for interment, 8.

Blacks, 45; mulattoes, 21. Of these—free, 30; slaves, 36.

BOOKS AND PAMPHLETS.—"Lallemand and Wilson on Spermatorrhœa."

"A Manuel of Psychological Medicine: containing the History, Nosology, Description, Statistics, Diagnosis, Pathology, and Treatment of Insanity; with an Appendix of Cases. By John Charles Bucknill, M. D., etc., and Daniel H. Tuke, M. D., etc. Philadelphia: Lea & Blanchard." Through our enterprising and accommodating friend, J. C. Morgan, Post-office building.

"Announcement and Circular of the Memphis Medical College, for the Session of 1857-58."

"Transactions of the Twenty-ninth Annual Meeting of the Tennessee State Medical Society, held at Nashville, April 6th, 1858."

"University of Nashville, Medical Department. Catalogue for the Session of 1857-58, and Announcement for the Session of 1858-59."

"Report and Announcement of the Medical Department of the University of Pennsylvania, for the Session of 1858-59."

"Thirty-ninth Annual Announcement of Lectures of the Medical College of Ohio, for the Session of 1858-59."

"Circular of the Faculty of Oglethorpe Medical College, Savannah, Georgia; with a Catalogue of the Students and Graduates: Announcement of Lectures—Session of 1858-59."

"Pathology and Treatment of the Paralysis of Motion. By. J. P. Batchelder, New York." From the author, and a most interesting and valuable paper.

LIST OF PAYMENTS TO AUGUST 27TH, 1858.—Drs. D. McKnight, vol. 5, $5; J. B. Morgan, vol. 6, $5; W. Wilcox, vols. 3, 4, 5, $15; T. J. Everett, vols. 3, 4, 5, $15; A. Thibaut, vol. 5, $5; J. E. Keator, vol. 5, $5; J. C. Harris, vol. 5, $5.

When our delinquent subscribers look at the above aggregate, we are sure they will be ashamed to feel that *we* are paying the printer. Is it possible that they do not intend to pay their paltry just debts? If not, we are sorry for those who credit them to a greater extent than we do.

EXCERPTA.

Observations on the Beneficial Effects of Pepsine in the obstinate Vomiting of Pregnancy.—*By Dr. L. Gros.*—In a great majority of cases the vomiting of pregnancy may safely be left to the influence of time ; but there are some cases in which females are scarcely able to retain in their digestive system a sufficient amount of nourishment to support their existence, and are therefore reduced to the last degree of emaciation. In some, also, the shocks occasioned by this obstinate and repeated vomiting become the source of abortions, which might have been prevented by moderating the activity of the morbid phenomenon. A very remarkable case was related in 1856, by M. Teissier, Professor of Clinical Medicine at Lyons, showing the immediately beneficial effects of a dose of pepsine in a case of vomiting during pregnancy. In this case the symptoms resisted all the ordinary methods which were employed, and the patient was unable to retain in her stomach any substance whatever. Under these circumstances, the patient was brought to M. Tessier, who found her in the following condition : The vomiting had continued for two months, and she was at the end of the fourth month of her pregnancy; she presented the appearance of a skeleton, having the aspect and cough of a phthisical subject ; the pulse was 140, and M. Teissier thought at first that the case was one of pulmonary tubercle. Finding that all treatment had been hitherto inefficacious, and that the lady was fast actually dying of inanition, he was seriously meditating upon the propriety of inducing abortion as a means of saving her life ; but as a last resource, before operating, he determined to employ pepsine. He accordingly prescribed one gramme, to be divided into two doses, and taken every day in a spoonful of broth. At the very first dose the broth was retained, and from that moment the vomiting never returned. On the third day the lady ate some chicken, and then some beef-steak. The treatment was continued in the same manner for three weeks, and at the end of that time the cure was complete ; the emaciation was replaced by *embonpoint*, the fever and the cough ceased with the vomiting, and at the end of the nine months the lady was safely delivered.

Dr. Gros then relates six other cases in which the pepsine was employed with the same success, and he thinks himself warranted in concluding that pepsine undoubtedly produces good effects in the vomiting which attends pregnancy. He explains the results by supposing that, although in the first instance the vomiting is due only to the sympathy existing between the uterus and the stomach, yet subsequently the stomach itself becomes affected, as is proved by the fact that in the beginning of pregnancy the vomiting occurs only in the morning or the evening ; but in aggravated cases it supervenes every meal, and all alimentary matters are rejected. In such cases, therefore, when the stomach has taken on a morbid habit, and exhibits an alteration of secretion, the pepsine seems to be really indicated ; although in a merely sympathetic action between the uterus and stomach it would be difficult to explain the efficacy of its action. (*Bulletin Général de Thérapeutique, Feb.* 15, 1858.)—*Brit. and For. Med. Chir. Review.*

SIEBOLD, C. Th. v. : *True Parthenogenesis in Butterflies and Bees.*—(Leipzig, 1856 ; and Canstatt, l. c., p. 146).—Siebold's work contains a review of the older publications on *parthenogenesis* and describes this curious phenomenon as it offers itself in the bee and in the silkworm. The queen-bee i. e., a female bee with perfectly developed sexual organs—is, according to Dzierzon, fecundated by a drone. The semen is retained in the receptaculum seminis of the queen. The eggs of the queen which are to be developed into working bees—i. e., female bees with imperfectly developed sexual organs, or into a queen-bee, require impregnation, while those which are to give rise to drones are not to be impregnated. The queen while placing the eggs into the cells, feels with the lower part of the abdomen whether she has to fill a wider or narrower cell, and accordingly induces impregnation by causing the coming forth of a particle of semen from the receptaculum. Siebold corroborates this statement, and has in addition, ascertained the entrance of spermatozoa, through the mikropyle, into the interior of the ovum. In the silkworm (*bombyx mori*), as well male as female individuals may be developed out of unimpregnated eggs ; while in the solenobia the unimpregnated eggs can produce only female individuals.

EMULSIONS—*By Robert Batty, M. D., of Rome, Georgia.*—There is, perhaps, no class of preparations in the dispensing practice of country physicians, so badly done up as emulsions. A few practical suggestions, from one who has spent some years in the drudgery of a dispensing shop, will, I trust, not be devoid of interest to those who desire to furnish their patients with nauseous remedies in this most eligible form.

An emulsion may be defined to be a combination of oil, fat, or resin, with water, through the intervention of an alkali, gum, albumen, or casein. It is, in other words, an artificial milk, of which nature has given us a perfect type in the milk of animals, the juices of the garden lettuce, the poppy, the asclepias syriaca, the ferula assafœtida, and a large number of both indigenous and foreign plants, are samples or copies furnished us for our study and imitation.

In attempting to arrive at the points of a good emulsion, we cannot do better, perhaps, than take cow's milk as our standard of comparison ; and first, we notice that it is, when fresh an opaque, homogeneous liquid—such should be the character of the well-formed emulsion The milk, upon standing for a time, throws up its cream to the surface—the emulsion, by age, will do the same ; but as we do not find masses of butter upon the milk, neither should we have globules of free oil on the top of the emulsion—the latter, like the former, should admit of free dilution with water, without any separation or decomposition. Like milk, while it should not be too thin and watery, it is by no means thick and viscid ; in color, pure white, if possible—if tinted, let it be cleanly and agreeable to the eye ; in odor and taste, as little repugnant as the nature of the remedy will permit.

Let us now inquire how these indications are to be carried out in practice. We will take, for illustration, the recipe upon page 163 of this Journal—a recipe medicinally admirable, but pharmaceutically bad. We have here, of oils 2½ ounces, solid materials 3 ounces, tincture 1 ounce, and water 3¼ ounces—a preparation entirely too concentrated, thick and viscid—a teaspoonful of which forms a more nauseating dose than four times the quantity of a well made milky emulsion. When diluted with an equal bulk of water, it separates into three distinct layers—a dirty looking fluid, with a curdled precipitate at the bottom and an oily layer upon the top : in both respects, then most unlike its prototype—milk. In this case, the quantity of water has been insufficient to prevent the curdling action of the lavender upon the mucilage ; it should therefore be increased ; and this may best be done by using a portion of aqua menth. pip. as a corrigent, and also to conceal the flavor of the turpentine. The quantity of soda, upon the other hand, is excessive, its saponifying properties being unnecessary—so much only should be used as is medicinally requisite.

Salts should enter into emulsions only when clearly indicated in the case, and then in small quantity; for, with the exception of the alkaline salts, they impair the permanence of the preparation—while the alkaline salts, themselves, in large quantity, offend the palate and stomach. The use of ordinary spts. turpentine should be avoided if possible—fresh camphine or pine oil is preferable; and when this cannot be obtained, the spts. turpentine may be put in a retort or flask, with a little water, and one half of it distilled off for medicinal purposes: as prepared in this manner, it is so nearly free of taste and odor, as to be scarcely recognizable in the emulsion. The castor oil should be clear and limpid, and wholly soluble in alcohol; and the powdered gum arabic, when rubbed upon a pill tile, with a little water, should form a bright and clear mucilage, free of color.

Upon remodeling the formula, in accordance with the views above indicated, we have a handsome preparation, as little repugnant to the taste as the remedies will admit, and the receipe will read somewhat as follows:

℞.	Ol. Ricini Opt. . . .	f ℥ ij.
	Ol. Terebinth, Recentis. . .	f ℥ ss.
	Sodæ Bi Carb. . . .	ʒ ij.
	Pul. Acacia Opt. . . .	
	Pul. Sacch. Alb. . . .	*aa* ℥ i.
	Sps. Lavendul Comp. . .	f ℥ i.
	Aquæ Camph. . . .	
	Aquæ Menthar Pip. . .	*aa* f ℥ vj.

M. Ft. Emulsio. Sig. a tablespoonful to be taken every two hours.

It is not sufficient, however, that we have a well devised formula: skillful manipulation in combining the ingredients is also of prime importance, if we would have a handsome product. The simple mixture of the materials in a bottle, gives us a heterogeneous mass, utterly devoid of form and comeliness; and even when well begun, a single false step in the process will often wholly defeat the end proposed. Yet the process, when once understood, is by no means a complicated or difficult one.

In compounding the above formula, the first step is to weigh the dry materials—soda, gum and sugar—and combine them in a suitable mortar, capable of containing a pint—then add of the camphor water sufficient to form a thick mucilage, which gives a crackling sound under the pestle. Mix well the turpentine and oil in a graduate, and add it in successive portions of 5 or 10 drops, triturating thoroughly after each addition, until about one-fourth has been thus combined with the mucilage, which will be found to have thickened in the process. It will now be necessary to to add a few drams of the camphor water to attenuate the mixture to its former consistence, when it will be proper to add the remainder of the oils, slowly, as before, and with constant trituration. The camphor water may now go in, dram by dram, followed by the mint water; and lastly, the sps. lavender, which should be added cautiously, drop by drop, continuing the trituration constantly throughout the process.

I have been accustomed in past years to compound an emulsion of copaiba, extensively and favorably known to the Profession here as "No. 310." The recipe, obtained from Prof. Miller, of the Augusta College, is as follows :

℞.	Copaibæ,	
	Tinct. Cubebæ,	
	*Syr. Uva Ursi, . .	*aa* f ℥ ij.
	Pul. Acaciæ Opt. . .	℥ ij.
	Aquæ Cinnam. . . .	f ℥ xvj.

M. Ft. Emulsio. Sig. Dose, a tablespoonful three times daily.

I use, in my own practice, a formula, as I think it somewhat more elegant than the above—

℞.	Copaibæ, ...	f ʒ ss.
	†Ext. Dios. Fl. Comp. .	f ʒ iv.
	Ol. Menth. Vir. . .	gtt. viij.
	Pul. Acaciæ Opt. . .	℥ ss.
	Aquæ Font. . . .	f ℥ v.

M. Ft. Emulsio. Sig. Tablespoonful three times daily.

In comparing the two recipes, three points are worthy of notice : First, the use of mentha virida to couch the taste and odor of the copaiba, as being much better fitted for this purpose than the cinnamon; secondly, the use of ol. menth virida, in place of aq. menth. vir., saving the unnecessary trouble of preparing the latter for the purpose; thirdly, the use of fluid ext. of buchu, in place of the tr. cubebs and syrup uva ursi. The buchu contains a notable quantity of sp. nitre, oil cubebs and oil juniper, and is altogether a more eligible preparation for this use.

In typhoid fever, and indeed all febrile cases where I have a dry, parched tongue to contend with, I use the following formula :

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[*] Syrup Uva Ursi contains the active matter of four ounces of the leaves in each pint of syrup.

[†] Each pint represents, of Buchu 2½ ounces; Sp. Nit. Ether 2½ ounces; Oil Cubebs and Oil Juniper, of each, 21 drops.

℞.	Ol. Terebinth Recini,	f ʒ ij.
	Sodæ Bi Carb.	ʒ j.
	Pul. Acaciæ Opt.	
	Pul. Sacch. Alb.	*aa* ℥ ss.
	Sp. Lavendul. Comp.	f ℥ ss.
	Aquæ Camph.	f ℥ iv.
	Aquæ Menth. Pip.	f ℥ iij.

M. Ft. Emulsio. Sig. Tablespoonful every two hours.

To this, tinct. opii. or tinct verat. viride may be added, if indicated.

In digesting a formula the following suggestions will be found useful:

1st. The quantity of the active ingredient must, of course, be governed by the case. For one pint of emulsion, two to four ounces of castor oil—one half to one ounce of oil turpentine, and one to two ounces of copaiba, will usually be found desirable limits.

2nd. The quantity of gum used may be one ounce for four of castor oil, two of copaiba, and one of oil turpentine—unless it should be desirable, as in the case of copaiba, to render the preparation more mucilaginous. When sugar is used, the gum may be a little curtailed, if desired.

3rd. Syrups may be added freely, if indicated; but with tinctures and spirits it is advisable that the quantity do not exceed an ounce in the pint.

4th. No insoluble powders should be added to an emulsion; nor should soluble salts be used in large quantity.

The rules for the preparation of emulsions may be briefly summed up in the order in which the materials are to be used.

1st. All dry materials.
2nd. Watery menstruum to a crackling mucilage.
3rd. The oils, until the mucilage thickens.
4th. Watery menstruum to attenuate.
5th. Remainder of the oils.
6th. Remainder of the watery menstruum.
7th. Simple or fluid extracts, if any.
8th. Tinctures, or spirits.

ANNUAL ADDRESS OF THE RETIRING PRESIDENT OF THE BUFFALO MEDICAL ASSOCIATION. *Delivered in June,* 1858, *by Austin Flint, M. D.* Gentlemen of the Association:—In compliance with a by-law of our association which makes it incumbent on the retiring president to deliver an address on some medical or surgical subject, I invite your attention to a few desultory thoughts on *Conservative Medicine.* I am indebted for this term to my friend and colleague, Prof. Hamilton. On a convivial occasion some years since, I had the honor to propose as a toast, *conservative surgery*, exemplified by the principles and practice of the distinguished gentleman just named, and in responding to this toast he claimed that conservatism is a principle not less important to the physician than the surgeon; that conservative medicine is as much a reality as conservative surgery, and that the former, as well as the latter, is to govern, within certain limits, the conduct of the practitioner. The term may guide us to some reflection which will not be without interest and value.

What meaning is to be attached to the expression "conservative medicine?" We understand by "conservative surgery" a reluctance to mutilate the body by surgical operations; a judicious expectancy which looks constantly toward cure without resort to the knife, not repudiating the latter, but resorting to it only as an alternative rendered necessary by insufficiency of the means of restoring or maintaining the integrity of the organism. The conservative surgeon directs his efforts first and chiefly to the preservation of the body with all its parts, and when these efforts become ineffectual and longer delay dangerous, he does not hesitate to sacrifice portions of the corporeal fabric, more than the prudent mariner hesitates to throw overboard a rich cargo to prevent his ship from sinking, or the fireman to pull down valuable buildings to stay the progress of a conflagration. Conservative medicine, in like manner, is reluctant to impair the conditions on which depend life and health. It accepts the maxim of Chomel, that the first object in the mind of the medical practitioner should be not to do harm; the second object being to endeavor to do good. The conservative physician seeks, above all things, not to compromise, except when absolutely necessary, and only to a necessary extent, the vital forces; in directing his attention to disease, he never shuts out of view the system at large; his aim is not merely to destroy maladies, but to effect recovery with the powers of the constitution as complete as possible. A brief definition of conservative medicine is not practicable. This comparison will serve to give a general idea of the significance of the expression.

The knife of the surgeon is manifestly a calamitous necessity; and it is accepted with gratitude because it is a substitute for other and greater calamities. The same is true of medication. A medicine is always an evil; it becomes a remedy in relieving of other and greater evils. The fact as regards surgery is clear enough; in its application to medicine it is less clearly seen, and, hence, is not only overlooked by the vulgar, but, to a vast extent, by medical practitioners. A remedy, not less than the scalpel, is an alternative. It is not to be inferred from the existence of disease, that remedies are to be employed, more than that every swelling is to be cut out, as a matter of course. The use of remedies should always imply that recovery from disease will not take place so certainly, so speedly or so perfectly without, as with them. If this be not assumed, it would be as absurd to subject the system to medication, as it would to make an incision into the flesh without any definite object. I speak now, of course, not of simple palliative remedies, but of those which exert some decided effect on the economy beyond the mere alleviation of present symptoms. In another point of view, a potent medicine may be compared with a surgical operation; not only is it either indicated or not indicated, and therefore useful or not useful, but it must either do good or harm. A remedy, in proportion to its activity, is potent for evil if not for good. A useless amputation is something more than an act of supererogation. This statement needs no argumentation. A salivation or a venesection when not needed, is not less entitled to be considered as involving an irrecoverable loss of a certain amount of vital force, although the fact is not always so distinctly appreciable. The system has lost somewhat of its power of endurance and resistance after being subjected to any unnatural disturbing agency. An active remedy when not required is always a perturbatory agent. If it be indicated, it may save life and secure the recovery of health; but if not needed, as an alternative by which a greater evil is avoided, it afflicts an injury on life and health not the less real and irreparable because slight and inappreciable.

Conservative medicine adopts as a principle of action, renunciation of all medication not sanctioned clearly by reason or experience, on the ground of its being useless and pernicious. This rule is only deviated from when, with proper care and judgment, and under appropriate restrictions, remedial measures are employed experimentally in order to advance our knowledge of therapeutics. Self evident as is the correctness of this principle when viewed as an abstract proposition, a glance at the history of medicine suffices to show that it has not had its legitimate effect on medical practice. The natural philanthropic impulses of our nature interfere with it. It is difficult to forbear endeavors to remove disease even at the risk not only of failure, but of doing harm to the patient if we do not succeed. The patient is often anxious to incur this double risk, and desires to submit to measures the potency of which is proportionate to the gravity of the malady. Another obstacle is the want of definite knowledge respecting the natural tendencies of diseases. The knowledge is the true point of departure for the study of therapeutics. Before we are prepared to judge of the influence of remedies in arresting or abridging any disease, we should know what will be its probable issue and duration without active medication. Strange as it is, the importance of the natural history of diseases in this aspect has not been fully appreciated, and at this moment we are acquainted with the intrinsic tendencies of few diseases sufficiently to be able to judge correctly concerning the extent to which they are controllable by art. The knowledge, however, which has within late years been acquired of the history of certain affections when left to run their course without active medication, has influenced greatly therapeutical notions. Nearly twenty-five years ago, Prof. Jacob Bigelow, of Boston, published a brief paper on the self-limited character of certain diseases. The enumeration of the simple truth that some affections have a defined career, ending after a certain duration in accordance with a limitation inherent in themselves, marked the beginning of a change in views and practice in New England almost revolutionary, and this change extended thence, more or less, over our country, so that when some years afterward Dr. Forbes, of London, published his papers entitled Young Physic, they contained little that was new or striking to the intelligent American medical reader.

Conservatism as a predominant principle in the management of disease, is, in fact a characteristic of the present epoch in medical history. The preservation of the vital forces is an object in the mind of the judicious practitioner in all cases prominent and often paramount. In the treatment of the continued and eruptive fevers, nearly all the important measures are directed to this end. We do not strive to arrest or to abridge these forms of fever, but to sustain the powers of life till the morbid processes which constitute the disease, end by their own limitations. It is however but a few years since these fevers were treated by active medication, which, as it was useless, must have been mischeivous. The periodical fevers may be controlled by certain remedies, and hence, the first object in these forms of febrile disease, is to resort to the medication which experience has shown to exert a special influence upon them. In the treatment of acute inflammations, conservatism is usually the leading principle. As a type of this class of affections take pneumonia in the adult. A few years ago it would have been culpably hazardous in any practitioner to watch the course of this disease without active interference. Some observers have had the hardihood to do this in a great number of cases, and the result has been that the disease is found to end favorably in a very large ratio of instances, when no potent medication is resorted to. Most judicious practitioners at the present time are agreed that in the management of this disease it is safer to forego active interference than to incur much risk of impairing by active measures those powers of the system upon which dependance must be placed for restoration. This remark will equally apply to acute inflammations in general. In chronic maladies, structural lesions, malignant affections, etc., our expectations as regards prolongation of life, and the preservation of those faculties of mind and body which are not directly compromised, are based far more on measures which are conservative rather than remedial. In such cases little may be hoped for from curative efforts, when much may be anticipated from judicious efforts to preserve the powers of life. As an instance illustrating the vast improvement which has been effected by the simple substitution of conservative measures for those which were erroneously supposed to be curative, pulmonary tuberculosis may be cited. They who have been medical observers for the last twenty years, have witnessed a complete revolution in the management of this disease. General and local depletion, mercurialization, cathartics, emetics, counter-irritation, low diet and confinement in an uniform temperature within doors, severally or collectively, were supposed to exert to some extent a curative influence. Nothing can be clearer than that these falsely considered means of cure were positively injurious. Since they have all been abandoned and reliance is had, not on any special curative agent, but on those conservative influences derived from hygiene rather than medication, recovery from the disease is not infrequent, and its progress in fatal cases is far less rapid.

The subject which I have selected for a few remarks offers in various directions abundant scope for reflection and inquiry. I have thrown out, as I proposed at the onset, only a few desultory thoughts. In conclusion, lest the tenor of these remarks should lead to misapprehension, let me say that there is a false conservatism in medicine as well as in surgery. Therapeutical measures which in their effects are depressing, enfeebling and spoliative, are, nevertheless, under certain circumstances, to be resorted to not less than mutilating surgical operations in certain cases of disease or injury. To determine correctly when these measures are necessary; to regulate their application, carrying them far enough, but not too far; in other words, to understand the limits of true conservatism, knowing when to employ heroic remedies and when to exercise an equal heroism in resisting the natural tendency of the mind to active interference, this is the part of the safe and efficient physician, as it is the part of the safe and efficient surgeon to know when to spare the knife, and when its use is required by science and humanity.—*Buffalo Medical Journal.*

ON THE USE OF QUININE IN SCARLET FEVER—*By Edwin A. Morrison, M. D., of Lawrenceville, Brunswick county, Va.*—Much has been written on scarlet fever, and various have been the remedies proposed for its cure; still, it continues to be the scourge of our race, at the most interesting period of life. That any treatment will control those cases in which the morbific impression in the beginning is so overwhelming as to depress the vital powers below the point of reaction, is not to be expected. We may hope, however, that medical science will yet succeed in disarming to a greater degree the malignity of this disease.

I have used with gratifying results, sulphate of quinine as a remedy for this disease, and I offer to my professional brethren the benefit of my experience, sincerely trusting that those of them who may be disposed to try the prescription, will find it as efficacious as it has proved to be under my direction. In a practice of more than thirty-five years, I have repeatedly treated scarlet fever in its epidemic form. In 1837 it raged extensively in this section of Virginia. I treated the worst cases successfully with cold water affusions, as used by Dr. Currie; and I vainly believed that by this means I could control almost any case; but this opinion was soon shaken.

In 1839 I encountered this epidemic again, when some of my cases defied this, and every remedy which I used. In the family of my friend, Mr. J. Ravenscroft Jones, in 1857, I met it again. The first case to which I was called was that of a little negro three years of age, when it was in a condition apparently hopeless. I ordered quinine, and directed the throat to be mopped with a solution of nitrate of silver. Contrary to my own expectation, and to that of my friends, Drs. George Mason and E. B. Jones, who saw the case frequently with me, the child recovered.

The daughter of Mr. Jones, a child of delicate constitution, was next taken with the disease. The above-named physicians attended this case with me. No quinine was administered. The child died. I cannot state positively how many cases afterwards occurred in this family—my impression is, about twenty. All these were treated with quinine, and all recovered except one. This was that of a negro boy about fifteen years old, whose condition was not made known until he was hopelessly ill. The father of the boy had undertaken to treat the case with copperas water. When I was called to see this case, I ordered quinine, but the boy lived ten or twelve hours only.

About this time I saw ten or twelve cases in other families. All were treated with quinine, and all recovered.

Early in May of the present year this disease again appeared within the range of my practice. I have treated about fifteen cases. One was that of a negro girl between ten and twelve years of age, who was speechless when I first saw her. She died in a few hours. Another was that of a negro child two years of age, which died of laryngitis, after having survived the constitutional impression of the disease. The other cases, with two exceptions, have fully recovered. The two excepted ones have had dropsical effusions. To-day (June 5th), they are so much better as to leave no doubt upon my mind as to their speedy recovery.

The mode of treatment which I have practiced is as follows: When the first symptoms of the disease appear, I order quinine, regulating the dose by the age of the patient. This is administered every two or three hours until the system is brought decidedly under its influence. During this time I occasionally give a few grains of blue mass to move the bowels gently, and mop the throat with a strong solution of nitrate of silver. When the patient is old enough, I direct a gargle of red-pepper tea and common salt to be used. I order the throat of a child to be washed with the same.

I am aware that Peruvian bark has long ago been used in cynanche maligna, and that some writers have maintained that this is identical with scarlet fever. Hence it may be said that there is nothing new in the use of quinine for scarlet fever. My object in this communication is not to claim originality, but to place before my medical brethren the results of my experience.

I will add, that my friend, Dr. R. Sims, of this county, has used quinine in some few cases of scarlet fever, with the happiest effect.—*Virginia Medical Journal.*

ON THE INTRODUCTION OF THE CATHETER IN STRICTURE—*By Dr. Slade, of Boston.*—I prefer that the patient should be in bed, that he should be warmly covered, and that he should be particularly protected against any sudden chill. A bougie is then to be selected, of a size corresponding to the size of the stream passed, as nearly as may be, or to the presumed diameter of the constricted passage; this is to be carefully lubricated with lard, cold cream, cerate, or some other equally tenacious substance, which is greatly to be preferred to the olive oil so commonly in use. Thus prepared, the instrument is to be carried carefully down to the seat of the stricture, and, if possible, pushed on into it, the entrance of its extremity being at once known by the peculiar manner in which it is grasped. After a few moments' delay, the bougie, in the great majority of cases, may be pushed on into the bladder. This, however, it must be borne in mind, is not always necessary; the mere presence of the instrument at the seat of the obstruction is generally sufficient to overcome the spasmodic action upon which the retention depends. The only difficulty in carrying these delicate instruments down to the stricture, is from their becoming entangled in the various lacunæ, which, as is well known, are greatly enlarged in this disease.

Mr. Henry Thompson, of London, has recently suggested a method of protecting the mucous membrane from injury, and of rendering the introduction of small instruments more easy, particularly in these very cases of narrow stricture, which on trial will be found very useful. It consists in the simple method of applying the oil to the urethra itself, and very freely, rather than to the instrument. In order to effect this, he says, the nozzle of a common glass syringe, containing from four to six drachms of pure olive oil, should be introduced into the urethra as far as it will go, the external meatus being at the same time closed upon the nozzle by the forefinger and thumb of the left hand, so that none can escape. Gentle pressure being now made upon the piston-rod, the oil gradually finds its way down to the stricture; and if this be very narrow, the urethra in front of it slowly fills and becomes slightly distended; but as the piston continues to descend, the oil will gradually pass through the stricture and onward into the bladder, thoroughly lubricating every part of the canal. At the moment the oil passes through the stricture, the operator may sometimes distinctly perceive a slight, but very complete, sensation communicated to the hand, of resistance overcome, and partial collapse of the previously distended urethra in front. The syringe is then to be removed, the finger and thumb still commanding the meatus of the urethra so that no oil escapes. The smallest catheter may now be introduced, and made to traverse the urethra—at all events as far as the stricture—with very little or none of that difficulty arising from the catching of its point against the walls of the passage, so often experienced with very small instruments and which renders so much care necessary in their employment.

Temporary dilatation is, without doubt, the safest and surest method of treating organic stricture. Although slow, at the same time it can be easily managed and can be suspended at any moment, according as circumstances require, and, above all, does not prevent the patient from pursuing his usual avocation—and for the early treatment of narrow irritable stricture, the use of gum-elastic or wax bougies is far preferable to metallic instruments. I have seen patients who have suffered so much from the passage of small metallic instruments, that they have not been willing to allow their farther use, but have made rapid progress under the employment of flexible instruments. When, however, the dilatation has proceeded so far that a No. 5 or 6 bougie passes with ease, then these may be laid aside and metallic instruments substituted.—*Ibid.*

ATTEMPTED SUICIDE WITH STRYCHNIA—TREATMENT WITH NICOTINE—RECOVERY.—*By Thomas O'Reilly, M. D., M. R. C. S., Eng.*—On Thursday, September 10, 1857, at one o'clock, P. M., I was urgently requested by Dr. Byrne to accompany him to see a Mr. Johnson in this city (St. Louis, Missouri, U. S.), who, he was informed, had taken poison. On arrival at his residence we learned the following history to account for his condition :

After a three years' cruise, as a musician on board an United States frigate, he was paid off in New York, and for the amount purchased drafts on St. Louis, which, on his arrival here were found to be worthless. This, together with recent domestic sorrows, so overwhelmed him, that he determined on self-destruction. To accomplish this end, he called on a respectable druggist, and demanded a large dose of poison for a dog. The druggist gave him six grains of strychnine, which he carried into an adjoining beer-house, and, playfully remarking to the bar-tender that he was going on his last spree, mixed the strychnine with beer, and drank it off. Soon repenting this rash act, he mentioned to those present that he had taken poison, and wished they would give him an emetic. One was procured which vomited him freely, but, notwithstanding, violent symptoms set in, and we were called on to see him.

On entering his room we found him stretched on his back, his countenance expressive of the most painful suffering and distress, his frame shaken by frequent convulsive spasms, his limbs rigidly extended, and his head slightly bent backwards. His face was of a livid red, and covered with a profuse sweat; his eyes were sunken, and moved with a rapid motion; his mouth was covered with saliva, which he ejected by spasmodic jerks, strongly reminding me of a case of hydrophobia which I had seen some time before. His respiration was quick and difficult, and attended with great pain in the precordial region; skin warm, and covered with a profuse clammy sweat, and he had copious watery discharges from his bowels. His intellect clear and collected, and his feelings were so morbidly acute that the slightest touch appeared to aggravate his sufferings, and to bring on a spasm. On this account we could not correctly ascertain the state of his pulse.

Ignorant of any antidote likely to relieve him, and pressed by the urgency of the case, Dr. Byrne, acting on the suggestion of Dr. Haughton's paper, (read before a meeting of the Royal Irish Academy, Nov. 29, 1856,) took a cigar from a gentleman present, and infused it in half a pint of water.

One hour and fifteen minutes after he had taken the poison, we gave him the first dose of the tobacco infusion, which he swallowed with difficulty. We continued it in table-spoonful doses at intervals of five minutes, until he had taken half the quantity infused, before we had noticed a favorable change. Then the muscles became relaxed, the spasms less severe, and the intervals between them longer; and so conscious was the patient of relief, that he constantly called for the tobacco-juice when he felt the paroxysm approaching. This encouraged us to persevere with the infusion, prolonging the intervals between each dose, as the frequency of the spasms abated; until, finally, after twelve hours, they disappeared, leaving him in a state of fearful nervous prostration, from which he recovered in a few days, under a careful tonic treatment.

In the quantity of infusion administered we used one ounce and two drachms of dry tobacco leaves, including the cigar—a quantity which no healthy condition of system could stand; but the urgency of this case demanded it, and the result justified it, and at the same time afforded us another illustration of the counteracting influence of poisons, and tended in some way to establish the correctness of the conclusions at which the Rev. Mr. Haughton had arrived by his experiments.

From a single instance like this, I should be far from recommending nicotine as an antidote; but I trust that the publication of the case may attract the attention of others, who will test the subject with due care, and give the result of their experience to the world.—*The Medical Times and Gazette.*

DIPHTHERIA—*To the Editor of the Medical Times and Gazette*—Sir: Since the date of the last paper published in your Journal, I have had three cases of the disease in the Queen's Hospital. One, very severe, in a young man, secondary to scarlatina; another, of moderate severity, in one of the nurses; the third, very mild, and treated on the day of its appearance, in a young servant of the Institution and daughter of the second case. All these patients were treated successfully on the plan laid down. It is important to mention that erysipelas has been unusually rife in the town during the last few weeks, and has attacked the hospital patients with a violence hitherto unexperienced. Ten days ago I saw a rapidly fatal case, with Mr. Sproston, of malignant pustule on the face.

But one of the most severe illustrations of the malady which have yet occurred to me, fell under my notice last Tuesday, the 11th. It is a representative case, equally in reference to the majority of the symptoms and to the effects of the treatment. On that day I was sent for at 3 o'clock in the afternoon, by my colleague, Mr. Langston Parker, to see a girl, A. H., aged 10, living at Ashted. That gentleman informed me that he had been in attendance four or five days; that, the night previously, he had advised a consultation, which was not agreed to; but that on seeing her deplorable condition early in the morning, he had obtained the assent of the friends to that measure, but he thought all consultation unpromising, as her state appeared now almost to preclude hope.

The patient has a dark complexion, eyes and hair; parents cousins; several children have died of various infantile disorders. The family are eminently weakly. The mother informed me that for two or three weeks the girl had been out of health, and especially had been troubled with diarrhœa. About five days before my visit she complained of sore-throat, and there was marked swelling under the jaws, with feverishness, anorexia, and loss of sleep. Mr. Parker observed that there was swelling of the tonsils, and a general œdematous rubescence of the palate and posterior fauces, but did not note exudation anywhere. The symptoms daily be came aggravated, and on Sunday, the 9th, dyspnœa to an extreme degree was superadded to the symptoms, the stridor being so loud and whistling as to be distinctly heard in the garden. These symptoms, conjoined with delirium, great restlessness, and prostration, assumed the most formidable proportions the next day—that preceding my visit. The usual remedies had been sedulously employed, such as caustic solutions to the fauces, etc. Several blisters had been applied to the throat, just beneath the jaw, which seemed to give relief.

I found her very restless; face pale, and indicative of prostration; eyes languid, and deeply sunken; delirium constant; complete insomnia. A thin sero-purulent discharge constantly trickled from the nares, the upper lip excoriated. Breathing stridulous, and accompanied by nasal sounds. This symptom seemed to Mr. Parker less severe than in the early part of the morning. A most complete aphonia. Dysphagia inappreciable. Fluids had never returned through the nose. Skin dry and harsh, not markedly above the ordinary temperature. On the uvula, tonsils, and velum were scattered patches of slightly yellowish exudation, especially on the edges of the uvula. These parts were swelled, and highly congested, but not of a violaceous tint. I could not observe any ulceration. The lips and general buccal membrane free from deposit. No hæmorrhages or petechiæ existed. Bowels once moved this morning. The blistered surfaces were covered with a fibrinous film, and looked "angry."

We agreed that we had to contend with a most formidable case of epidemic angina membranacea, and resolved upon the use of the dilute hydrochloric acid to the affected parts (of the fauces), which was thoroughly effected several times by Mr. Parker. Weaker gargles of the same acid were ordered; wine and beef-tea at frequent intervals; eggs and milk as far as they could be pushed. A mustard emetic was directed to be administered immediately, and the following medicine every second hour, in tablespoonful doses:—℞. Acidi hydrochlorici dil. ʒss.; tinct. ferri. sesquichloridi, ʒjss.; syrupi aurantii, ℥ss.; aquæ, ℥v. misce.

In the evening Mr. Parker saw her, found that two mustard emetics had failed to operate, and that the medicine had not been administered; but the stimulants, etc., had been proceeded with. He thought the patient rather better, and reapplied the acid.

12th.—There is a striking improvement in her condition. Several hours' sleep had been obtained, though delirium still existed. The countenance is better; exudation less marked on the fauces, though in considerable quantity. The breathing much easier; stridor scarcely observable, but aphonia hardly less complete than before. Pulse weak, but firmer than yesterday, about 100°. Bowels not moved once. The medicine has been given pretty regularly since 8 o'clock last night. The vesiccated surfaces less coated, perhaps, than yesterday. The acid was again freely applied, and everything else ordered to be persisted in.

13th.—Breathes and swallows easily; stridor has disappeared; slept eight hours in the night; eyes bright; expression animated; a little color in the cheeks. Skin moist; pulse less frequent and stronger; tongue moist. Exudation still exists on the fauces, but is greatly diminished. Nares do not pour out so much ichorous fluid. She wants to get up. The blistered surfaces look healthy, and the fibrinous film has all but disappeared. Bowels not open for two days. Perstet. An enema immediately. Yeast and water to be syringed frequently into the nares and mouth. The recumbent posture strictly enjoined.

Mr. Parker informed me on the 15th that she was fully convalescent, and had eaten a mutton chop the night before, against orders; but there was an increase of the discharge from the nares. On the 16th he writes me: "Miss H. continues better. She is, to-day, up and dressed, and none the worse for it. Tongue clean; appetite good; bowels acted; pulse 74. No remains of throat mischief except a very slight white streak down the uvula. Discharge from the nose again much diminished."

I am, etc.,

T. P. HESLOP.

Observations on the Duration of Pregnancy.—*Dy Dr. Elsässer.* As a contribution to this subject, Dr. Elsässer communicates the particulars of 260 cases of normal pregnancy with mature children, entered in the journals of the Stuttgart Lying-in-Hospital, and to which certainty may be attached.

1. *Reckoning from the day of conception,* the duration was:

Exactly 280 days in	23
Less than 280 days in	166
More than 280 days in	71
	——160

In nearly one half (126) the duration was between 271 and 280 days, and in 62 other cases between 281 and 290 days.

Separating the cases into primiparæ and pluriparæ, we find that there were 149 *primiparæ*, and in these the duration was—

Exactly 280 days in	14	
Less than 280 days in	96	
More than 280 days in	39	
		149

In 111 *multiparæ* the duration was—

Exactly 280 days in	9	
Less than 280 days in	70	
More than 280 days in	32	
		111

So that among primiparæ there have been greater abnormalities in plus and minus, and the mean normal period has been seldomer met with than in multiparæ. The extremes of the duration of pregnancy (232 days, and from 301 to 306 days) occurred in primiparæ.

2. *Sex of the Children.*—The sex of the child seems to have exerted little or no influence upon the duration of the pregnancy. There were born 130 children of either sex; but as among the primiparæ a great preponderance of female (85) over male (64), happened to occur, the extremes of duration were met with in them.

3. *Reckoning from the commencement of the last menstruation.*—The indication of this is often wanting, or too indeterminate to be relied upon; and among these cases it could only be depended upon in 175. Of these 13 menstruated once after conception, and the rekoning in them was made from the penultimate menstruation. The duration was found to be—

Exactly 280 days in	12	
Less than 280 days in	43	
More than 280 days in	120	
		175

Of the last number the duration was from 281 to 290 days in 60, 291 to 300 days in 49, and 301 to 318 days in 11.

If 280 days be accepted as the normal duration, the reckoning from the date of conception more nearly approaches it (in 8.8 per cent.) than that of the last menstruation (6.8 per cent.) This so-called normal duration is, however, that which is seldomest met with; for reckoning from the period of conception 91.1 per cent., and from the commencement of the last menstruation, 93 per cent. of all the cases either fall short of or exceed this term. The same deduction may be drawn from the observations of others. Thus, Merriman found only 9 of 114 cases in which labor came on at the 280th day. Reid enumerates only 18 of 40 women in whom, conception following a single coitus, labor came on between the 274th and 284th day; and Duncan found, reckoning from the period of conception, 275 days, and from the last day of menstruation 278 days, was the mean period.

4. *The interval between the ccmmencement of the last menstruation and conception* was found to be—

5 days in	13 cases.
From 6 to 10 "	47 "
" 11 " 15 "	38 "
" 16 " 20 "	18 "
" 21 " 25 "	9 "
" 26 " 30 "	14 "
Above 30 "	8 "
	——124

These statements, are, however, to be taken with the greatest caution, for it is seldom that this class of women observe exactly the last day of menstruation.

5. *The weight of the child in relation to the duration of pregnancy.*—Dr. Elsässer furnishes a table in which the mean weights of the children are placed side by side with the duration of the pregnancies of their mothers; but we need not transcribe this, inasmuch as it only proves that there is no relation prevailing between the two facts.—*Henke's Zeitschrift*, Band lxxiii. pp. 394—400.

QUESTION OF THE CURE OF CATARACT WITHOUT OPERATION.—M. Testelin has compiled an elaborate paper investigating the reputed cases of cure of cataract without surgical operation; and he quite agrees in the conclusion come to by a very large majority of opthalmologists who met at Brussels congress, viz., that there is not on record any case sufficiently authenticated proving that medical treatment can arrest or cause the retrocession of a spontaneous opacity of the crystalline lens.—*Med. Times and Gaz.*, July 3, 1858, from *Annales d' Oculistique*, t. xxxix.

TEARS CONTAINING SUGAR FROM A CASE OF DIABETES.—Dr. Gibb showed to the Pathological Society (May 18) some tears shed by a young married lady, aged 21, the mother of one child, who has had diabetes for two years since the child was weaned. They contained a large amount of sugar, as contrasted with that in an equal bulk of her urine, which was of the specific gravity of 1043. An evaporated drop of the tears on a piece of glass gave a much thicker and more opaque crust than was yielded by a drop of the urine.—*Med. Times and Gaz.*, July 3, 1858.

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ON THE CLINICAL STUDY OF THE HEART-SOUNDS.

By AUSTIN FLINT, M. D., Prof. of Clinical Medicine and Auscultation, in the N. O. School of Medicine.

LETTER NO. 2.

PROF. FENNER—*Dear Sir:* In my former letter I considered, *first*, of the results of the clinical study of the *second* or *diastolic* sound of the heart in health, those which are of importance as preliminary to the study of the abnormal modifications of this sound; and, *second*, the diagnostic value of the latter as based on clinical observation in cases of cardiac disease. I propose in this letter to consider the *first* or *systolic* sound in the same points of view, i. e., physiologically, as studied by means of auscultation in health; and pathologically, as furnishing, by its abnormal modifications, physical signs of disease. This sound is less simple than its fellow, and the agencies concerned in its production are not generally regarded as so well established. It is more liable to morbid alterations, and, in short, as a subject of investigation, it is more complicated, but, at the same time, more interesting and important. Without farther introduction I will enter on its consideration.

FIRST OR SYSTOLIC SOUND OF THE HEART IN HEALTH AND DISEASE.

Auscultation in health furnishes abundant proof that this is not, like the second sound, unmixed, in other words that it consists of more than one element. Its distinctive characters, as generally described, relate to the sound heard at the point within the præcordia where its intensity is greatest, viz.: over the apex of the heart. At this point, especially if the person auscultated be in the sitting posture, the first sound, as compared with the second, is more intense or accentuated; it is nearly twice as long in many persons; it is lower in pitch; the valvular quality is less apparent and may be absent; it has a booming, impulsive quality, sometimes accompanied by a slight shock, and sometimes by a faint singing intonation, or tinnitus. These are distinctive traits determined by noting and analyzing the results of four careful examinations in a series of healthy persons. They are present and more or less marked when the stethoscope is placed over the point of apex-beat. They are also present, but not as strongly marked, on auscultating over the body of the heart, within the superficial cardiac region. But if the stethoscope be carried to a point somewhat removed from the left border of the heart, viz.: over or a little without the left nipple, (in the male) the sound is divested of some of the more striking of its distinctive characters. It is shortened, being scarcely, if at all, longer than the second sound; the interval between the first and second sound is proportionately lengthened; the intensity of the sound is notedly lessened, the second sound being relatively more intense or accentuated; it is valvular in quality, resembling in this respect the second sound, but is lower in pitch, having lost the booming, impulsive quality which is apparent over the apex.* The same points of difference are observed if auscultation be practiced, first, with the stethoscope applied upon the naked chest, over the apex, and, afterward, with several thicknesses of soft cloth placed between the instrument and the skin. What is the explanation of this difference? Over the apex an element enters into the first sound which is eliminated when the instrument is carried beyond the heart, or when folds of cloth are interposed. What is the element eliminated? What is the element which remains after the elimination? Auscultation in health supplies data for answers to these questions. Answering the second question first, the residual sound is purely valvular. It is identical in quality with the second sound. It consists, then, of an unmixed *valvular element.* The clinical study of the sound in health and disease, seems to me to furnish proof that the characters eliminated are due to the impulsion of the heart's apex against the walls of the chest Attributing these characters mainly to this impulsion, I am led to distinguish, as the second element entering into the first sound of the heart, an

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* It should be stated that the examinations on which these statements are based, were made with Cammann's stethoscope, an instrument which, in the opinion of the writer, possesses great advantages over the ordinary modern cylinder.

element of impulsion. The first sound is thus composed of two elements, viz.: a valvular element and an element of impulsion.

What is the proof that the characters distinctive of the first sound as heard over the apex, are due to the impulsion against the thoracic walls of this portion of the organ. These characters vary in their degree of prominence when auscultation is practiced on the same person in different positions, the variations corresponding to alterations of the force of the apex-beat. After studying the characters of the sound in a healthy subject in the sitting posture, let him take a recumbent position on the back; the characters referable to impulsion are notably less marked, and, at the same time, the force of the apex-beat is diminished. Let the person next incline the body to the right side; the characters due to impulsion, are still less marked, while the apex-beat is feeble and may not be felt. Turning to the left side, the characters of the sound are more strongly marked than in the sitting posture, and the apex-beat is also more forcible. In certain abnormal conditions, the heart is prevented from coming into contact with the thoracic walls. This occurs in pericarditis with effusion, in cases of liquid effusion into the left pleura, and of emphysema affecting the left lung. Under these circumstances, the element of impulsion is lost; the first sound is short and valvular like the second. The same result is observed when the muscular power of the heart is greatly reduced, as in some cases of typhus and typhoid fevers, dilatation of the heart, fatty degeneration, etc. In these affections the first sound approximates, more or less, to the second sound as regards shortness and quality. The element of impulsion is wanting in the first sound of the fœtal heart auscultated over the abdominal walls. It may be said that this proof does not exclude the hypothesis of a sound of the muscular contraction entering into the first of the heart-sounds. Without discussing this hypothesis, which I think is disproved by auscultating the naked heart, I will only remark that, so far as the practical bearings of the analysis of the first sound are concerned, it is not a matter of much importance whether the characters attributed to impulsion be due to this cause, or more directly to muscular contraction. Regarding the former explanation as the correct one, I shall continue to designate the element, an element of impulsion.

The element of impulsion predominates, and, as it were, drowns the valvular element, over the apex and body of the heart. There are occasional exceptions to this rule in healthy persons, and in these instances the second sound may be accentuated even over the apex. At the base of the heart, frequently the valvular element predominates. At the right and inferior borders of the heart the valvular element often predominates. At any point removed from the præcordial region, to which the first sound is propagated, the element of impulsion is eliminated, being the valvular element. The latter, therefore, is alone diffused beyond the limits of the heart, notwithstanding it is generally drowned in the preponderating element of impulsion over the apex and body of the organ. The valvular element has less intensity and is less diffused than the second sound; for, on carrying the stethoscope away from the heart, the first sound ceases, as a rule, to be appreciable sooner than the second sound; and, as just stated, at a distance from the præcordia, the second sound becomes accentuated.

The analogy between the valvular element of the first sound and the second sound, as regards quality, leads to the inference that this element is due to the action of the auriculo-ventricular valves. Clinical observation in cases of disease, confirms the correctness of this conclusion. Hence the propriety of designating it the valvular element. Assuming the correctness of this explanation, the element consists of the action of two sets of valves, viz: the mitral and tricuspid; now, can the sound emanating from these two sources be disconnected and distinguished from each other, as the aortic and the pulmonic seconds sound are discriminated in certain situations? Over the inferior border of the heart, near the xiphoid cartilage, the valvular element of the first sound frequently differs in pitch from the same element when the stethoscope is applied at or without the left nipple. When this disparity in pitch is observed, it may be inferred that the predominating sound in the latter situation originates at the mitral, and in the former situation at the tricuspid valves. When this disparity exists, the pitch is lower at the inferior border of the heart. For convenience of reference, the sound near the xiphoid cartilage may be called the tricuspid valvular element, and the sound at or without the left nipple, the mitral valvular element of the first sound of the heart.

The decomposition of the first sound, and the facts which have been stated with respect to the two elements, respectively, which compose it, enable the clinical observer to understand the abnormal modifications which this sound presents in different cardiac affections. Both elements may be modified by disease, but in unequal proportions; either element may be affected independently of the other; either may be suppressed while the other remains; and of the two components of the valvular element, viz: the mitral and the tricuspid, the one may be lessened or suppressed, and the intensity of the other augmented, by disease. What are the pathological relations of these different abnormal modifications, or, in other words, what diagnostic significance belongs severally to the latter as physical signs of disease?

In cases of enlargement of the heart in which hypertrophy predominates, the intensity of the first sound is augmented more than that of the second sound; and this exaggeration is due mainly to the augmented intensity of the element of impulsion. This element is morbidly exaggerated disproportionately to the increased intensity of the valvular element. Hence, as all observers have remarked, the first sound in hypertrophy is abnormally dull and prolonged. This is intelligible, since the dullness and prolongation of the first sound are due to the element of impulsion. In proportion to the prolongation, the interval between the first and second sound is shortened; it may be almost or even quite inappreciable. In this effect of hypertrophy we have a feature which distinguishes the augmented intensity due to it, from that which occurs when the muscular action of the heart is increased by merely functional excitation. In the latter case, the first sound is abnormally exaggerated, but the exaggeration does not affect disproportionately the element of impulsion to the same extent. Hence, the sound is less dull and prolonged than in cases of hypertrophy. This fact has arrested the attention of observers. Disproportionate exaggeration of the element of impulsion in a marked degree, therefore, is a physical sign of hypertrophy.

On the other hand, whenever the muscular power of the heart is weakened, the intensity of the element of impulsion falls below that of health, and the abnormal feebleness of this element is greater than that of the valvular element. When the muscular power of the ventricles is greatly reduced, the element of impulsion may be suppressed while the valvular element is still appreciable, and in some instances both elements are extinguished and the first sound is wanting, while the second sound may still be heard. Abnormal feebleness of the element of impulsion, and its extinction occur in cases of enlargement of the heart in which dilatation greatly predominates. In proportion as this effect is produced, the first sound not only becomes enfeebled, but valvular in quality and shortened, in these respects approximating to the second sound. In proportion as the sound is shortened, the interval between the first and second sound is lengthened. This change in the quality and duration of the first sound in cases of dilatation, was observed by Laennec, and was regarded by him, and also by Hope and others, as distinctive of dilatation. It is, however, only significant of weakened muscular power of the ventricles. It is not peculiar to cases of dilatation. It characterizes equally, as has been remarked by Stokes, the enfeebled first sound in certain cases of typhus and typhoid fever; also in cases of fatty degeneration, and, in short, whenever the muscular power of the organ is greatly reduced. When this condition exists, from whatever cause, the first sound becomes relatively more feeble than the second, and also short and valvular, resembling the latter sound in these particulars, but always lower in pitch. The second sound being much less affected by any of the various causes which enfeeble the element of impulsion of the first sound, is accentuated, even over the apex, when these causes are operative to much extent. The element of impulsion may be enfeebled and even suppressed in cases of hypertrophy, if, notwithstanding this lesion, the muscular power of the heart, from any cause, be greatly weakened. Abnormal feebleness and suppression of the element of impulsion, therefore, are physical signs of the muscular weakness of the heart due either to organic changes, dilatation, softening, fatty degeneration, etc., or to great functional debility of the organ. It is to be added that this element is eliminated, as has been seen already, when from effusion of liquid within the pericardial, or the left pleural sac, from emphysema, or other causes, the heart is prevented from impinging during the systole against the walls of the chest.

The abnormal modifications affecting the element of impulsion, have been considered; it remains to consider those which affect the valvular element of the first sound. The intensity of this element is increased whenever the muscular action of the heart is unduly excited. In violent palpitation, both elements of the first sound are exaggerated in a ratio not notably unequal, while in hypertrophy, as has been seen, the intensity of the valvular element is not increased in proportion to that of the element of impulsion. Excited action of the ventricles, thus, tends to increase the loudness of the valvular element, more than the augmented, sluggishly excited muscular power in hypertrophy. As a physical sign, exaggeration of the valvular element has very little positive value. Diminished intensity or suppression of this element is of more importance in diagnosis.

The valvular element may be diminished or suppressed in conjunction with the element of impulsion. When both elements are thus similarly affected, great weakness of the muscular power of the heart is indicated, which may be due either to functional debility or organic change The first and most marked effects of weakness of the heart, are apparent in the element of impulsion. If the weakness be considerable, this element is suppressed, leaving the valvular element still appreciable. If the weakness be very great, the latter element at length becomes inappreciable, and the first sound is extinct, while the second sound may still be heard. Extinction of the first sound occurs in some cases of typhus and typhoid fevers, and from this symptom, as the reader is doubtless aware, Dr. Stokes, and others, desire an indication for the free use of stimulants in these fevers and other diseases attended with extreme adynamia. It is also observed in some cases of fatty degeneration of the heart. If the two elements are enfeebled conjointly, and not unequally, it is not to be inferred therefrom that there exists any valvular lesion.

But the valvular element may be enfeebled or suppressed while the element of impulsion is as intense and more so than in health. Valvular lesions involving more or less damage or destruction, are indicated under these circumstances. Inasmuch as, the auriculo-ventricular valves, the mitral are the seat of lesions in the vast majority of cases, the tricuspid valves, as is well known, being very rarely affected, it is the mitral valvular element which is enfeebled or suppressed in connection with the valvular lesions which produce modifications of the first sound. This element, it may be remarked, is to be studied in cases of disease, where it is best studied in health, viz : at or without the left border of the heart, in other words upon, or to the left of the nipple. The stethoscope is to be carried to a point sufficiently removed from this border of the heart, to eliminate the element of impulsion, the valvular sound which remains being that emanating from the mitral valves. Clinical observation goes to show that the valvular element in this situation is abnormally feeble, or suppressed, according to the damage which the mitral valves have received from lesions there seated. A murmur accompanies these lesions, not invariably, but in the vast majority of cases. This murmur points to the existence of mitral lesions, but it does not furnish information respecting the amount of injury which the lesions have occasioned. Cases are frequently met with in which a murmur referable to the mitral orifice is observed, without being accompanied by symptoms denoting much, if any disturbance of the circulation. The murmur may continue, denoting innocuous lesions, for many years.* When they are thus innocuous, the mitral valvular element of the first sound will be found to retain nearly or quite its normal intensity. On the other hand, when the lesions have occasioned more or less damage to the valves, the valvular element is feeble in comparison with the element of impulsion, and may be inappreciable, while the latter is not only present but perhaps exaggerated. The valvular element of the first sound, referable to the mitral valves, therefore, is to be observed in connection with mitral murmurs, as a means of determining whether, or not, and to what extent, these valves are damaged. Practically, these are the important points involved in the diagnosis of mitral lesions; for these lesions are serious, or otherwise, in proportion as the valves are injured. The presence of a murmur, it must be confessed, interferes, to a greater or less extent in some cases, with the study of the valvular element. If the murmur be intense, and especially if it be rough, the valvular element may be obscured or drowned by it. In drawing conclusions from its indistinctness or absence, when a co-existing murmur is loud or harsh, therefore, we are not to be so positive as we may be in concluding that the integrity of the valves is not greatly compromised when the element is found to retain nearly or quite its normal intensity. It is also to be borne in mind that feebleness and suppression of the valvular element, are not physical signs of valvular injury except when the element of impulsion is not proportionately feeble or wanting. It may be added, that, when they are physical signs of valvular injury, a mitral murmur is present, as a rule to which there are a very few exceptions.

Feebleness and suppression of the tricuspid valvular element, are of no value as physical signs, for, in the first place, it is only in a certain proportion of persons in health that this portion of the valvular element can be distinguished from the portion referable to the mitral valves; and in the second place, lesions of the tricuspid valves are exceedingly infrequent. Abnormal intensity of the valvular sound emanating from the tricuspid orifice, however, is a sign of some value. This sound in disease is, of course, to be sought for in the same situation as in health, viz.: at, or just below the inferior border of the heart, near the xiphoid cartilage. In some cases the valvular element in this situation is strongly marked, exceeding the intensity of the mitral sound. This occurs as an effect of hypertrophy of the right ventricle, generally resulting from mitral lesions which, of the same tissue, lessen the intensity of the mitral valvular element. Its significance, as a physical sign, is precisely that of intensification of the pulmonic second sound of the heart. Exaggeration of the tricuspid valvular element of the first sound and of the pulmonic second sound of the heart, will be likely to be associated; but I have met with instances in which the former co-existed with hypertrophy of the left ventricle, while the latter was not well marked. It may be remarked here, that comparison of the valvular element referable to the mitral valves, with that referable to the tricuspid valves, constitutes a means of judging, on the one hand, whether the power be abnormally weakened, or, on the other hand, whether the latter be exaggerated.

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* For cases illustrative of this statement, see Essay in Transactions of American Med. Association, by the writer, vol. xi. 1858.

In conclusion, the practical points pertaining to the diagnostic significance of the abnormal modifications of the heart-sounds, are embodied in the following series of propositions: 1. Increased intensity of the first sound, the two elements composing this sound being affected equally, is a sign of excited, muscular action of the heart, and is observed in cases of functional disorder without organic disease.

2. Increased intensity of the element of impulsion in the first sound, the intensity of the valular element not being proportionately augmented, if at all, is a sign of hypertrophy affecting the left ventricle.

3. Diminished intensity of the element of impulsion is a sign of weakened muscular power of the left ventricle, either from organic affections, such as dilatation, or fatty degeneration, or from functional debility of the organ. Cases are to be excluded in which, from the presence of liquid effusion in the pericardium or pleura, or from emphysema, the heart is prevented from coming into contact with the thoracic walls.

4. Abnormal intensity of the mitral valvular element of the first sound, is a sign of excited muscular action of the heart, and is accompanied by a corresponding increase of the intensity of the element of impulsion, as stated in prop. 1st. Abnormal weakness and suppression of this element, the element of impulsion retaining or exceeding its normal intensity, are signs of more or less injury of the mitral valves. A murmur referable to the mitral orifice co-exists in the vast majority of cases. Notwithstanding

the murmur, if the valvular element of the first sound referable to the mitral valves, retain nearly or quite its normal intensity, the valves are not seriously damaged. In judging of the normal intensity of the mitral valvular element, it may be compared with the sound emanating from the tricuspid valves.

5. Abnormal intensity of the valvular element referable to the tricuspid valves, is a sign of hypertrophy of the right ventricle, and is generally associated with diminished intensity of the valvular element referable to the mitral valves. Abnormal weakness of the tricuspid valvular element is not available as a physical sign of disease.

6. A *positive* increase of the intensity of the pulmonic second sound of the heart, is a sign of hypertrophy of the right ventricle, in the majority of cases dependent on mitral contraction or insufficiency, or both. A *relative* increase of this sound, i. e., as compared with the aortic second sound, may result from abnormal feebleness of the aortic sound, due to mitral obstruction or regurgitation.

7. Abnormal intensity of the aortic second sound, is not a sign of much importance. But non-diminution of its intensity, in cases in which a murmur referable to the aorta is present, is a sign of much value, indicating that, although aortic lesions exist, the integrity of the valves is not seriously compromised.

8. Diminished intensity of the aortic second sound, in cases in which a murmur referable to the aorta is present, is a sign that the aorta valves are damaged, provided that neither mitral obstruction nor regurgitation exists, an effect of the latter being abnormal feebleness of this sound. If the diminished intensity of the aortic sound be due to injury of the valves of the aorta, there will be likely to be present an aortic regurgitant murmur, in other words, a diastolic murmur referable to the aorta.

9. In cases in which a diastolic murmur is present, referable either to the direct current of blood through the mitral orifice, or to aortic regurgitation, a normal intensity of the aortic second sound is evidence that the lesions giving rise to the murmur, are seated at the mitral orifice.

Venturing to hope that the *résumé* which I have endeavored to present of the results of clinical study of the heart-sounds in health and disease, has not been without interest for some of the readers of the *Medical News and Hospital Gazette,* I remain, with much respect, truly yours,

A. F.

MENTAL EXCITEMENT A REMEDY FOR DISEASE—CURIOUS CASES.

By JOHN N. LEGRAND, M. D., of Harrison Co., Miss.

In the year 1829 I resided at Randolph, a small village on the bank of the Mississippi river, and sixty miles above Memphis. In the months of June and July the prevailing fevers were of the ordinary intermittent and remittent types. During the months of August and September, algid, congestive fever prevailed extensively. It is not my design to write the history and treatment of those different types of fever, but to publish some facts illustrative of the powerful influence of high mental excitement, in arousing the system from the worst forms of collapse, which so often occur, as the closing scenes of congestive fever.

Case First.—Mr. Good, an old Methodist preacher, was attacked with congestive fever in the absence of his family. Passing his house late in the night, my attention was arrested by hearing a deep groan. Entering the house, I found him pulseless and completely drenched with a cold perspiration. I had him removed to my office, and, with the assistance of my friend and partner, Dr. Robert Bedford, treated his case. All the ordinary remedies for producing reaction were used without the slightest benefit. He remained pulseless and cold, with blue shriveled skin, covered with cold sweat, for four days. His limbs lay motionless, wherever they were placed. Attempts to speak produced a noise like the blowing of a Muscovy drake. While I was expecting every moment to see him breathe his last, some ladies visited him, and standing around his bed, sang such hymns as they thought suitable to his condition. While singing one that he was notoriously fond of, he attempted to join them, but only made the old Muscovy drake noise. As they sang the last couplet, he sang with a loud and clear voice. When they had finished, he sprang up in bed and commenced clapping his hands and praising God more loudly than I ever heard any man in my life. This exercise continued more than a half hour. At its close, reaction was perfectly established. His pulse was full and firm, his skin warm, and his voice natural. He recovered rapidly. Without that mental excitement he must have died in an hour. I have never believed that Dr. Bedford and myself were entitled to the credit of curing old man Good. The singing and the joyful hopes of Eternity, which belong to the Christian, alone cured him.

Case Second.—In the year 1830, my own father was aroused from as deathly a collapse as Mr. Good's, by my reading to him a letter from my step-mother, in which she related the triumphant death of an old Christian friend. My brother, Dr. William S. LeGrand, now of Washington, Texas, and I were his physicians. The letter from my step-mother had been received two days, but we were afraid to speak to our father about it. At length, when all hope of recovery was abandoned, I proposed to my brother to read the letter to our father, that he might be happy. Before I reached the close of the letter he commenced rejoicing aloud. Reaction and rapid recovery were the results. The mental excitement produced by the Christian's hope of acceptance with God cured him

Case Third.—A similar case occurred in the hands of my brother, then residing in Madison County, Mississippi. His patient, Mr. William Moore, lay so long in a pulseless, collapsed condition, he thought all hope of recovery was gone. He thought it his duty to advise Mr. Moore to make a will, so as to secure into his wife's hands the means of supporting and educating his youngest children. After the will was written and signed by my brother taking Mr. Moore's fingers in his own hand and writing his name, the old gentleman asked for his wife and oldest children. He commenced conversing with them by saying, he was about to die, but, thanks to God, he was not afraid to die—that God, for Christ's sake, had forgiven his sins, and was now about to take him to Heaven. He then exhorted them to prepare to meet him in Heaven. In a short time he commenced rejoicing aloud and praising God for his goodness. The result was perfect reaction in a few minutes and speedy recovery.

Case Fourth.—Some years after the occurrence of the foregoing cases, my brother had one of similar character in the person of Mr. Simeon Stigler, of Holmes County, Miss. After having tried all the usual remedies for producing excitement, and despairing of success, and recollecting the happy results of religious excitement in the foregoing cases, he determined to try its effects on Stigler. He commenced by asking him, whether he thought he would know his wife in Heaven. He replied, certainly he should. My brother then continued to speak of the hopes of the Christian and the glorious prospects of Eternity, till he got his patient's feelings thoroughly enlisted. The result was reaction in a few minutes and perfect recovery in a few days.

I am satisfied the religious element furnishes the judicious physician with a stimulus better adapted to the treatment of collapse from congestive fever and cholera than any thing else on earth. I do not mean that religion alone furnishes the physician with the means of producing a high degree of mental excitement, but that, with religious men, there is nothing that so surely and perfectly rouses the whole man. If a very high degree of mental excitement can be produced by any thing else, greatly interesting the patient's feelings, the result will be the same. I once related the foregoing cases to an intelligent gentleman, who was not a physician, and was told by him he had once seen the same effects produced by sudden anger. The most astonishing case I have ever witnessed, occurred with a nephew of mine, a child not three years old. The child had been named for me and I had made a great pet of him. On arriving at my brother's house, he told me the child was in a perfect collapse and had not opened his eyes for a half a day. As I entered the room, my sister screamed and called my name. The child opened his eyes and smiled. I took him from his mother and exerted myself to the utmost to please him. In a very short time his pulse returned and his skin became warm. Perfect reaction took place in a short time followed by recovery without a particle of medicine being administered.

[The foregoing cases are certainly most interesting, and we record them in our journal more than willingly. The power of the mind over disease —for exerting both good and evil—has always been recognized by the profession, and no observing man can practice medicine five years without witnessing the effects of mental excitement. The abrupt entrance of a stranger into a lying-in room will put a stop to normal labor, and faith in a bread pill has been known to cure many an imaginary pain. Shrewd quacks often take advantage of, and the physician too often neglects, this valuable adjuvant.—Eds.]

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DYSMENORRHŒA CURED BY STRAMONIUM.

By A. P. Merrill, M. D., of Memphis, Tenn.

A married woman, aged about 30 years, applied to me for assistance some two years ago. She stated that she had suffered severely at every menstrual period from the age of 16 years; that she had been prescribed for by several eminent physicians, and had tried almost every form of quackery, and secret nostrum, without obtaining relief. Since her marriage about a year previously, she had suffered more than ever, and from a specimen of her sufferings which I then witnessed, I judged that her case was one of unusual severity. I prescribed the extract of stramonium, to be taken for several successive days, before the expected attack. She took the remedy only for a day or two, and suffering much from narcotism declined its further use, although she believed it had afforded her some degree of relief. About a year afterwards she consulted me again, stating that her sufferings at her regular period had greatly increased, so that in some instances her life had been supposed in great danger. As she had already tried a great variety of remedies without benefit, I determined to make a more thorough experiment with the stramonium, which I had used successfully in less obstinate cases. She was, therefore, directed to begin with the extract ten days before the expected paroxysm, and take one grain every three hours, diminishing the dose in case of too great narcotism, but without lessening the frequency of its repetition. The next period was passed with sufferings comparatively slight, and several succeeding periods with none whatever, with an increase in the menstrual secretion, and an anticipation of time. After five or six months of such treatment, the doses being reduced to half a grain, she became pregnant, and in excellent health.

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AN ESSAY ON THE CLIMATE AND FEVERS OF THE SOUTH-WESTERN, SOUTHERN ATLANTIC, AND GULF STATES.

[Revised and enlarged from the Memphis Medical Recorder.]

By JAMES C. HARRIS, M. D., of Wetumpka, Ala.

Under the impression that it is the duty of every physician to record, in some form or other, the result of his experience, we have occasionally, for the last seven or eight years, contributed through the Southern Medical press, brief notices of the climate and diseases of particular localities and regions. Of some of these crude attempts at authorship, not only the substance, but frequently the language, without further notice or acknowledgement, will be found scattered through the following pages. In addition to this, it will also be observed, that to enable us to illustrate some of our peculiar climatic features, we have been compelled to draw largely on the labors of others; but in every instance we hope it will be found that we have done so in an acceptable manner. With these brief declarations, we will now proceed, after first defining our geographical limits, and giving their general geological outline, to present as succinct but clear a view of particular sections and localities as the nature of our undertaking and materials will justify.

In the following remarks we comprehend as constituting the South-Western, Southern Atlantic and Gulf States, all that region of country extending from near the Tropic of Cancer, to a little above the thirty-sixth parallel of north latitude, and lying entirely between the first and twenty-fourth degrees of west longitude. The whole of this great region with its eastern, southern, and south-western limits resting on the Atlantic Ocean and Gulf of Mexico, and lying within the mountain ranges and high lands presently to be briefly sketched, if not naturally, may for the purpose of description, be divided into the lower or level, the middle or undulating, and the upper or mountainous districts. The level and undulating zones, with an average elevation above the Atlantic Ocean and Gulf of not more than four hundred feet, are composed *geologically* of the tertiary and cretaceous formations, to which succeed, in the mountainous regions, the metamorphic, carboniferous and primitive.*

Believing the relations existing between this region, the Gulf of Mexico, and Atlantic Ocean, are so intimate and natural, that we cannot properly understand, or fully appreciate the climate and endemic influences of the former, without at least a brief reference to some of the principal *currents* and *temperature* of the latter. Upon the first of these subjects, we are informed by marine hydrographers that a portion of the great western equatorial current, after striking against the eastern projection of South America, turns to the north, and passing through the Caribbean Sea, enters from one of the *hottest regions on the globe*, through the Straits of Yucatan, the Gulf of Mexico; where after mingling with its waters, and performing in it a kind of circuit, flows out to the north-east, through the Straits of Florida, at the rate of four or five miles an hour, as the well known and celebrated *Gulf stream.* As the waters of the northern shores of the Gulf are known to be *cooled* by the approach of winter, northern winds, and the influx of river currents from higher latitudes, upon the same principle the temperature of its southern and middle portions must be greatly *increased* by the unceasing introduction through, the Straits of Yucatan, of a large amount of warm water; and which we think more than probable, under the influence of a south-west wind, in passing out east, around the Cape of Florida, exerts, to a considerable extent, the same modifying influence on the climate of Georgia and the Carolinas* that the Gulf is known to do, under similar circumstances, on the more Southern States of the Mississippi Valley.

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* The geographical, topographical and geological descriptions contained in the following pages, besides the sources mentioned in the text, have been chiefly condensed from Colton's Atlas, Woodbridge's Geography, and the works of Drs. Forry and Drake.

MOUNTAIN RANGES.

Commencing on the east, nearly under the thirty-sixth parallel of north latitude, and perhaps not more than one hundred and fifty miles from the sea shore, a continuation of the Apalachian range, under the name of Blue Ridge, leaves the State of Virginia, and, after passing around through the Carolinas and Georgia, in a south-westerly direction, parallel with the ocean, and ranging in height from six hundred to three thousand feet above its level, on reaching North Alabama sinks down north of the thirty-third parallel to nothing more than an elevated range of hills, where they turn to the west, and approach the Mississippi in the direction of the Ozark mountains.

Turning now to the west, and commencing under the twenty-fourth meridian and twenty-eighth parallel, a spur of the Sierra Madre, proceeding from the junction of the Rio Puerco with the Rio Grande, enters Texas at the sources of the Neuces river, and, continuing in this direction, crosses the Colorado some distance below the mouth of the San Saba, and is finally lost in the undulating lands of the Brazos. To the north-east of this mountain range, with a supposed elevation of from fifteen to eighteen hundred feet, lie the table lands of Texas. Near the eastern margin of these, and of which they are nothing more than a rugged continuation, commence the Ozark mountains, which, after sending off some lateral spurs to north-western Louisiana, cross the upper portion of the State of Arkansas, and terminate near the Missouri river. From the eastern base of this range, the distance across to the first spurs of the Cumberland mountain, an outlier of the Apalachian range on the east side of the Mississippi, is not more than two hundred miles. Through this great gorge, the Mississippi river, a little to the west of the twelfth meridian, enters the central and northern portion of the region we have under consideration, and from which point, in its general course as far down as the mouth of Red river, it gradually inclines to the south-west, thence south-east to its confluence, nearly under the same meridian.

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* It appears that Dr. Forry [climate of the United States, pages 87 to 90] thought the influence of the Gulf stream, was nearly altogether expended in warming [from Cape Finistence to North Cape] the Western coasts of Europe.

Standing on the shores of the Gulf of Mexico, near the mouth of this great Father of Waters, with the face turned to the north, all that portion of this extended region lying to the south-east and north-east of this line will be recognized as the eastern portion; that to the south-west and north-west as the western.

The principal rivers, with their tributaries, washing the eastern section, commencing on the east, are the Cape Fear, Pedee, Santee, Savannah, Ogeechee, Altamaha, St. Marys, St. Johns, Suwanee, Apalachicola, Alabama, Mobile, Tombigbee, Pearl and Yazoo; all of which, excepting the St. Marys, St. Johns and Suwanee, have their sources either within the Apalachian range, or some one of the numerous outliers, and discharge their waters into the Atlantic Ocean, the Gulf of Mexico, or the Mississippi river. The St. Marys and Suwanee rivers of Georgia and Florida have a common source in the Okefenokee swamp; the St. Johns in an immense swamp in Orange county, Florida.

Those on the west, with their tributaries, commencing on the south-west and extending around to the north-west, are the Rio Grande, Neuces, San Antonio, Colorado, Brazos, Trinity, Sabine, Red, Wachita, Arkansas and White. Of these rivers the Rio Grande, the Red and Arkansas have their sources within the Rocky mountains; the Neuces, San Antonio, Colorado and Brazos, among the spurs of the Sierra Madre, and the Trinity, Wachita and White, from the table lands of Texas and spurs of the Ozark mountains; and they all discharge their waters into the Mississippi and Gulf of Mexico, either directly, or through large bays and estuaries.

SUBDIVISIONS.

The Carolinas and Georgia are naturally divided into three strongly-marked regions: the lower, the middle and upper. The first two of these lie on the great Atlantic plain. The lower country, extending from sixty to ninety miles from the sea shore, is nearly a dead level, traversed with sluggish streams, and filled with innumerable ponds and marshes. The soil is scanty and poor, excepting along the margins of the streams, where it is frequently rich. Back of this flat country, and extending to the lower falls of all the principle streams, is a belt of from forty to sixty miles in width, of moderately uneven surface and sandy soil. The low grounds between the sand hills of this zone are suitable for agriculture and pasturage; but with these exceptions, the country is scarcely worth cultivation. The natural growth of both of these districts is almost universally black-jack and pitch pine. Beyond this region, and above the falls, commences a beautiful country of hill and dale, and fine flowing streams of pure water. The whole of this region may be regarded as an elevated table-land, gradually rising to where the Apalachian range passes through these States. This up country has generally a strong and fertile soil, of a red color, mixed, in places, with a deep black mould, producing cotton, Indian corn, wheat, and other kinds of grain in great abundance. Its principal forest trees are the different varieties of oak, pine and hickory.

The southern coasts of these States are skirted by a range of islands separated from the main land by narrow channels of water. These islands, like the neighboring continent, are low and flat, and around the coasts of South Carolina and Georgia are covered with forests of live oak, pine and palmettoes.

Alabama.—All that portion of this State lying south of a line commencing at Columbus, Georgia, on the Chattahooche river, latitude 32° 25′ and running south-south-west by Wetumpka on the Coosa, Centerville on the Cahawba, Tuscaloosa on the Black Warrior, and terminating at or near Columbus on the Tombigbee, in Mississippi, in lat. 33° 30′, with its extreme southern limit resting on the Bay of Mobile and Gulf of Mexico, and extending through several degrees of longitude and parallels of latitude, will, in the succeeding descriptions, be considered as constituting South Alabama proper.

The southern portion of this region bordering on the Gulf of Mexico, for the space of fifty or sixty miles, is low and level—the shores of Mobile Bay being skirted with silt marshes and cypress swamps; beyond this, the banks and country become a little more elevated and are covered with pine forests. The remainder of this section of the State, off the water courses, may be said to consist of rolling uplands and prairies, and which, for the purpose of description, may be divided into the four following natural divisions—the *alluvial*, the *canebrake*, *prairie*, and *piny woods*. The first of these, lying along the rivers and creeks, and varying in width from a few yards to several miles, and in some places subject to inundation, was, in the first settlement of the country, covered with a dense growth of cane, interspersed with the palmetto, cypress and magnolia. The second and third being identical in geological composition, in reality constitute but one variety, and extend over a greater portion of the counties of Sumpter, Green, Marengo, Perry, Dallas, Wilcox, Lowndes, Montgomery, Macon and the southern part of Russell, with, perhaps, the best specimens in Green and Marengo; the soil of this region consisting of yellow, pulverable limestone, intermixed with recrements of animals and plants, and resting on a strata of rotten limestone, is extremely fertile, and, although presenting but little diversity of surface, is occasionally broken into rounded bald knolls,* as may be seen between Arcola and Demopolis, and between Livingston and Sumpterville. The summits of these hillocks are sometimes ornamented with cedars, but more frequently they are quite bare, or covered with but a scanty vegetation. Even where the surface is but slightly undulating, bald spots occur, where the rocks come up to the surface, and are exposed to view; but perhaps the most remarkable feature of this section is the extraordinary power of its soil to imbibe moisture from the atmosphere. The effects of this property, continues the same authority,† is so strikingly illustrated in the uncleared parts of the canebrakes that one can scarcely satisfy himself that he is not standing on the low grounds of some great river.

The Piny Woods.—This region, the soil of which is of a light sandy nature, abounds in springs of pure, clear soft water, and, as a general rule, is poor and unproductive. Notwithstanding, the pine flats of Autauga county, during the spring and early summer, contain considerable surface water, perhaps for the want of a sufficient supply of organic materials they are generally healthy.

To the north and north-east of the regions here described. the face of the country becomes more broken and somewhat mountainous, the Apalachian range terminating in the north-western section of the State by sinking down into nothing more than a range of elevated hills. The natural growths of the northern and middle sections is oak, hickory, chesnut and pine. Cotton is the staple production of the State, but Indian corn, rice, wheat, and other grains, are produced.

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* Tuomey's 1st Geological Report, page 134.

† *Ibid.*, 136.

Geological Outline.—To the *alluvial* and *post pliocene* deposits around the shores of the Gulf of Mexico, succeed the tertiary and cretaceous formations; of these the tertiary overlaps the southern edge of cretaceous in its entire extent across the State, "extending from the lower part of Sumpter, on the west, crossing the Alabama river near the mouth of Dixon's creek, and thence across to a point above Fort Gaines, on the Chattahooche. From this the cretaceous extends up to the lower falls of all the principal streams,"* meeting on the west the coal fields of the Cahawba and Coosa, and on the east the primitive formations, extending down from the mountain spurs.

The principal rivers of South Alabama are the Mobile, the Alabama, and their tributaries, the Tombigbee, the Cahawba, the Tallapoosa and the Coosa, the two latter forming the Alabama. The Mobile, about fifty miles above the city of Mobile, is formed by the union of the Tombigbee and Alabama, which latter is also a considerable stream, and is navigable for vessels drawing from five to six feet of water to Claiborne, sixty miles above its junction, and one hundred and fifty miles higher up, to the mouth of the Cahawba, it has four or five feet water, and from thence to the junction of the Tallapoosa and Coosa, and up the latter to Wetumpka, it is navigable for light draught steamboats, with few exceptions, at all seasons of the year. The Tombigbee is navigable for schooners one hundred and twenty miles to St. Stephens, and for steamboats, to Columbus, Mississippi. Of this stream the Black Warrior forms a large tributary, and is navigable to Tuscaloosa. The south-western portion is drained by the Coneche; and the eastern by the Chattahooche river. The Alabama river, in flowing down from the high lands through an alluvial valley, generally presents us with a bluff on one side and a low bottom, subject to inundation, on the other. In many places the bottoms are from two to three miles wide, and before being cleared up, were covered with a forest of cypress, sweet gum, magnolia and live oak.

Mobile, the chief commercial emporium of the State, is situated on the west bank of the Mobile river, at its entrance into Mobile bay, in lat. 30° 40′ north, long. 13° west. It is laid out on a beautiful and extended plain, elevated some ten or twelve feet above the highest tides, and extending back some six or seven miles to the commencement of the piny woods and tertiary plain. "Much of its site, which is somewhat terraced like the river bottoms of the interior of the great valley, is sandy, with beds of clay beneath, which prevent the rains from sinking into the earth, and lead to the formation of swales, or marshy grounds, that require ditching before they can be cultivated." To the south, adjoining the city, there is a cypress swamp, with its margin resting on an immense deposit of *silt* and drift-wood, and which presents a foul and suspicious appearance. On the upper and north side of the city, and constituting to some extent its boundary, is a small bayou called One-Mile Creek, and beyond it another, named Three-Mile Creek, designations which indicate their distance from the city. On each side of, and between these sluggish streams, there are swamps overshadowed with cypress, sweet gum, magnolia, and other shrubs, common in such localities of the south; these swamps never become dry even to the depth of two inches below the surface.

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* Tuomey's 1st Geological Report, page 144.

"In front of the city the bay abounds in islets and beds of alluvion, enveloping drift-wood, and covered with a heavy growth of reed grass, and other aquatic and sub-aquatic plants."

From the foregoing description, as might be expected, Mobile, besides being annually scourged with intermittent and remittent fevers of every type and grade of violence, has also experienced many yellow fever epidemics.

In ascending the Mobile river and its tributaries, we find standing amid the tertiary and cretaceous formations, St. Stephens, on the Tombigbee; Tuscaloosa, on the Black Warrior; Claiborne, Cahawba, Selma and Montgomery, on the Alabama; and Wetumpka, on the Coosa.

Cahawba.—This town, once the capital of the State, stands on the west bank of the Alabama river, immediately below the confluence of the Cahawba, in latitude 32° 20′ north, longitude 10° 10′ west. During high tide, the Alabama river overflows the entire town. To the north-west, at no great distance, extending across from one river to the other, is a considerable swamp. At present it is the seat of justice for Dallas county, and has never contained, since shortly after the removal of the capital to Tuscaloosa, in 1825, more than seven or eight hundred inhabitants.

From the first settlement of this country, this town has been subject to violent and often fatal intermittent and remittent fevers; and yellow fever, when the population reached about three thousand, is reported by Dr. Heustis to have prevailed there during the fall of 1821, and again in 1822, in an epidemic form ;* but as there are some who pretend to doubt the correctness of the doctor's diagnosis, it may not be amiss to state that he speaks† of the prevalence, in 1809, of yellow fever under his care, among the troops stationed at Terre aux Bœuffs, and in 1812, of a large portion of three companies of the first regiment of artillery, stationed also under his care at the barracks in New Orleans, dying of yellow fever ; hence we are irresistibly driven to the conclusion that he was not mistaken in the type of the fever that prevailed here during the above-mentioned period, and that it was nothing other than true, genuine and unmistakable yellow fever.

Selma.—This city stands on an elevated sandy plain, on the [right] west bank of the Alabama river, ten miles above Cahawba, and, on account of existing and anticipated railroad facilities, has received, within the last three or four years, an accession of perhaps more than two thousand inhabitants,and has become a point of considerable importance. Notwithstanding the plain on which the city stands is free from ponds, there is, to the north and east, at no great distance, quite an extensive swamp.

It appears from a comparison of all the information that Dr. Drake was able to collect at this place and Cahawba, concerning autumnal fever,‡ that he was brought to the conclusion that the disease prevails less here than there, which might be expected, he thinks, from the difference in their topography. It has not been visited by yellow fever The same authority continues :

" In a late paper by Dr. Harris, on the Medical Topography of South Alabama, I find the following paragraph :

" 'In 1824 the yellow fever appeared in Selma, and that, section of the country known as the Pleasant Valley, ten or twelve miles north. One case, under Dr. Phillips, terminated fatally on the third day after black vomit, and several cases under my inspection on the fifth and seventh days after the same ; some in collapse. There was no yellow fever in Mobile at the time.' "

"It is remarkable that Dr. Heustis, in his paper on the diseases of Cahawba, is silent on this alleged yellow fever, and that Dr. Lewis, in his

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* Observations on the Epidemic Fevers of the Southern States, p. 369.

† Medical Topography and Diseases of Louisiana, pages 89 and 117.

‡ Drake, Principal Diseases of the Valley of North America, p. 186–7.

Medical History of Alabama, has not adverted to it; and equally remarkable that, in the course of a rigid inquiry in 1848 into the fevers of that region, not one of its numerous physicians should have mentioned to me what Dr. Harris has since published."

As it is stated in the paper from which the foregoing paragraph was copied by Dr. Drake, that the information it contained was given on the authority of Dr. Edward Gantt, we consider it nothing more than justice to that gentleman to state that he had previously in Philadelphia, as a student of Medicine in the office of Dr. Rush, enjoyed opportunities of becoming familiar with the disease, and that he assured the writer he had never since met with, either in Mobile or New Orleans, better or more clearly-marked cases of yellow fever.

From this period this locality escaped a recurrence of yellow fever until the seasons 1854–5, when it again made its appearance and prevailed.

Montgomery.—This city, the capital of the State, stands amid the cretaceous formations on a sandy terrace, above high-water mark, on the east bank of the Alabama river, opposite the extreme south-east extremity of a horse-shoe bend of the same, in latitude 32° 10′ north, longitude 10° 12′ west. A range of hills, commencing rather abruptly on the south-eastern limits of the city, and running around to the east, form a kind of amphitheater of hill land in its rear, at some points of more than one hundred feet elevation, and giving to the principal part of the immediate plain on which the city stands, a decided inclination in the direction of the river. On the plain to the north-east, there are numerous ponds and marshes, which are thrown into forms more or less elongated and serpentine, by oak and pine ridges, or narrow plateaus, which gradually become more elevated and hill-like, but still embosom stagnant and swampy streams.

The upper stratum of this tract is a red sandy loam, with beds of silicious gravel. "To the west-north-west there is a margin of lower and wetter bottom land, on the upper end of which attempts were once made to build a town, but it proved too insalubrious."*

Since the permanent location of the capital here, in 1847, this city has rapidly increased in population and commercial importance.

Montgomery has been, we believe, from its earliest settlement, infested, to a considerable extent, with all the grades and varieties of intermittent and remittent fevers. In 1853 the yellow fever made its appearance here in a recognized and acknowledged form. It also prevailed the two succeeding years.

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* Drake, Principal Diseases of the valley of North America, p. 387.

Wetumpka.—The towns of East and West Wetumpka, connected by a fine and apparently durable bridge, are situated on both sides of the Coosa river, at the foot of the falls of the same name, and head of steamboat navigation, in latitude 32° 30′ north, and longitude 10° 15′ west. These have been, by legislative enactment, erected into and constitute one city, containing upward of twelve hundred inhabitants, several schools, three churches and the State prison.

The site of the western town is a sandy, level plain, cut and intespersed with an occasional ravine and swale, and terminating rather abruptly at the river bank, in a high bluff. A small portion of this plain in front of the lower part of the town is subject to an occasional inundation. The eastern portion of the city, standing on an elevated, narrow and rather rugged plain, is so completely hemmed in with a range of high hills, extending its whole length, and rising several hundred feet above the level of the river, that at many points there is scarcely space enough between their base and the water's edge for the erection of the necessary business buildings. These hills, composed of a gravelly, dry, micaceous, red loam, sloping back with gentle declivities, and terminating in level tops, surmounted with a forest of pines, chestnut and scrubby oak, afford most desirable sites for the erection of private residences, and from one of which, to the admirer of the works of nature, the prospect is most enchanting. To the south and south-west, as far as the eye can reach, nothing is to be seen but one extended landscape, interspersed with forests and farm houses; while at your feet dash and surge the gushing waters of the Coosa. The agitation of these waters in their passage over the falls causes the evolution of a large amount of vapor, which, descending at nightfall in copious showers of dew, gives to the air in the vicinity an unusual, and, at times, an unhealthy degree of dampness.

The river, after entering the limits of the city, runs for the first half mile in a south-westerly direction, when it passes under the bridge, and then shifts in its course more to the south-east; pursuing this direction for about a mile more, it then turns west, and runs in a devious line to its junction with the Tallapoosa.

At low stages of the water the current of the river above the bridge is thrown almost entirely against the western bank, leaving exposed to the action of the sun a large portion of vegetable matter. There are also at low stages of the river, among the rocks and shoals, many pools of stagnant water, in which a mass of organic matter is continually undergoing decomposition; one in particular, commencing not far above the bridge, and extending some hundred yards up the river, is a fruitful source of exhalation; a large ravine also extends from the water's edge in the rear of the buildings on Bridge street, nearly up to the market house, and receives the filth from the greater portion of this part of the city.

There are also other sources of disease of a local character, within the corporation, on both sides of the river, and also within the immediate vicinity, but as they are similar to those already described, and common to many southern towns and neighborhoods, we will pass them by without any further notice.

Notwithstanding Wetumpka stands on the last out crop of the cretaceous formations, and has been subject, from its first settlement in 1833, to intermittent and remittent fevers, yellow fever, in an epidemic form, has never prevailed. True, we have during our residence here, late in the fall of several years, met with an occasional case of remittent fever, not only on the city plat, but also in the low alluvial grounds, four or five miles south, and for eight or ten miles up the Tallapoosa river, of so malignant a type, that since we have had the opportunity of studying yellow fever in the wards of the Charity Hospital, New Orleans, we are satisfied were sporadic cases of that disease.

Mississippi.—The surface of this State has a general slope to the southwest and south, and its principal rivers, with the exception of a small section to the south, have their courses in the same direction.

The coast, which extends along the Gulf of Mexico for about sixty miles, has a chain of low islands, six or seven miles from the shore. These enclose several bays or sounds, the largest of which are Pascagoula Sound and Lake Borgne. The southern part of the State, for about one hundred miles from the Gulf of Mexico, is mostly a sandy, level pine forest, interspersed with cypress swamps, open prairie and inundated marshes, with a few hills of moderate elevation. This region, by cultivation, produces cotton, Indian corn, indigo, sugar, plums, peaches, figs, sour oranges and grapes. The western border, on the Mississippi, for about one hundred and seventy miles in length by fifty or sixty in breadth, and through which the Yazoo river flows, is an extensive swamp.

The prairie, or Tombigbee section, covering the north-eastern part of the State, and extending far down on the Alabama line, is uniformly level, with scarcely a tree, dotted here and there with pools and marshes, and intersected with sluggish streams. The soil of this region is a dark heavy loam of surpassing fertility, and of the same geological composition as that of the adjoining prairie country of Alabama.

The upper portion of the State, known as the Chicasaw Cession, has a rolling surface, and an open, champaign appearance, being free of undergrowth, and beautifully wooded with oak and hickory. The upland of this section produces abundantly, but the substratum of the country being sandy, they are soon ruined by the heavy rains. The valley lands are much more durable and productive, the soil being heavier and darker; but they are liable to be, and frequently are, submerged, acres at a time, under billows of sand washed from the uplands. Cotton is the stapl product.

Jackson.—This city, the capital of the State, we are informed by Dr. S. C. Farrar, stands on the west bank of Pearl river, in lat. 32° 20′ north, long. 13° 8′ west.* Geologically considered, it is near that well-marked boundary which separates the tertiary from the secondary formation, or, more particularly, the cretaceous beds of the latter from the eocene marl of the former. "Between the river and the town, to the north-east and south-west, lie extensive low grounds, covered by a dense forest, and subject to annual inundation."

For several years after the first settlement of this locality, it was subject to the most violent grades of intermittent and remittent fevers. Latterly, however, these have become more mild and manageable; but, notwithstanding this change, according to Dr. Farrar,† pneumonia and dysentery frequently exhibit a paroxysmal and strictly periodic type. Tertian intermittents are also sometimes complicated with fixed pains of the head, breast, back or limbs, so as to personate, with great exactness, a phrensy, pleurisy, hepatitis, or rheumatism—especially if the apyrexies are obscure or imperfect.

West Tennessee.—Near the sources of the Yazoo river, and on the northern boundary of the State of Mississippi, the Tennessee river approaches within 125 miles of the Mississippi river; here meeting the high lands of north-western Alabama, it turns almost directly north, and in this direction crosses the State of Tennessee, the western part of Kentucky, and empties into the Ohio river at Paducah. about thirty miles on a straight line from the Mississippi river.

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* Fenner's Southern Medical Reports, Vol. I, page 345.

† *Ibid.* p. 358.

As the water shed of this region is everywhere very near the Tennessee river, much the larger streams of the western district descend westwardly to the Mississippi. Of these the Wolf, the Big Hatchee, Forked Deer and Obion are the principal.

The surface of the whole district, every part of which belongs entirely to the cretaceous formation is either level or undulating, " except near the streams, and between their sources, and those of the tributaries of the Tennessee, where it becomes more elevated and hilly."

As a specimen of the level region, we are informed by Dr. Dashiells, of Jackson, that " the portion known as the Forked Deer Valley, and which extends from the counties of Hardeman and McNairy on the east to the Mississippi on the west, is, along the river, low, swampy, heavily timbered, and subject to annual inundation ; and on the recession of the water from these low grounds, after one of these spring overflows, numerous ponds are left partially filled with dead vegetable matter, to be carried off through the summer by the slow process of evaporation "* We are also further informed by this writer " that the spring season of this region is exceedingly variable, the summer and fall warm and sultry, and the winter moist and cold—the thermometer frequently indicating a variation of thirty degrees in twelve hours."†

Intermittent and remittent fevers of every grade and variety prevail throughout the entire district, occurring some autumns, in certain localities, with considerable violence.

Arkansas.—The surface of this State presents great variations in its physical configuration along the Mississippi river which marks its eastern boundary; and from ninety to one hundred miles inland, the country is low and widely interspersed with lakes and swamps, and, with inconsiderable exceptions, is annually overflowed by the floods of the Mississippi, Arkansas and St. Francis rivers. Further west the surface rises, toward the centre of the State becomes moderately hilly, and still further west rises into the Ozark mountains; beyond these the country spreads out into elevated and gradually rising plains, which terminate only with the

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* Memphis Med. Recorder, March No., 1855, page 202.

† *Ibid.* page 202.

Rocky Mountains. On the margin of all the rivers the soil deposited by the floods over thousands of acres is a rich alluvion, and very productive; back from these it is very sterile, being, in some parts, either from scarcity of water or metallic impregnation, unfit for cultivation. Of this State the principal forest growths are pine, cypress, sycamore and oak.

Fort Gibson.—This post is situated, measuring from a point near the mouth of the Sabine river, about four hundred and twenty-five miles north of the Gulf of Mexico, "on the east bank of the Neosho or Grand river, in the Indian territory, and about forty miles west of the western line of the State of Arkansas, in latitude 35° 47′ north, longitude 18° west. The site of the fort is about one hundred yards from the bank of the Neosho, and three from its mouth. About a mile and a half to the south-west, toward the Arkansas river, is a lake surrounded by marshes, and as its level varies little from that of the fort, the drainage of the latter is consequently very defective. As the fort was originally located in a canebrake, the soil partakes, in a very high degree, of what is designated in the language of the country, 'river-bottom land.' It is skirted on three sides by elevated prairie, about four miles in extent, environed by a chain of hills. The opposite side of the river presents a canebrake, extending a mile above and below the fort, interspersed with lakes and marshes toward the south-west. The soil is of a character admitting of the most prolific cultivation. Indian corn is the staple commodity; and of mineral productions, the principal are coal and salt.

"As regards thermometrical observations, it is found that the mercury rises higher at this point than at any other in the United States, with the exception, perhaps, of Fort Yuma, in Southern California. [See letter of Surgeon H. R. Wirtz, Army Medical Statistics, p. 437.] The mean annual quantity of rain, based on three years' observations, is 30:64 inches —one of the lowest averages among twenty-eight posts at which observations upon the rain-gauge have been made. The prevailing winds, which are southerly from the Gulf of Mexico, traverse the marshes and lakes above described.

"It thus appears that all the circumstances most conducive to the evolution of malaria are present; the soil is alluvion; solar heat is of the most intense character, and the quantity of rain, although adequate to the maintenance of a certain degree of moisture, is not sufficient to overflow the low lands during the summer season."*

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* Forry on the Climate of the United States, p. 184.

As the result, perhaps, of a general malarial impress, we are informed by Surgeons Pitcher and Wharton that cases of pleurisy, cholera-morbus, dysentery, diarrhea and rheumatism occurring at this post frequently assume a strictly periodical character, from which, together with their subjection to the same remedies that are found to arrest the course of intermittent fevers, their close alliance, if not common origin, is inferred.

As regards the prevalence of intermittent and remittent fevers, according to the army medical returns, this is the most unhealthy post permanently occupied.

Louisiana.—The surface of this State is low and generally level, with some hilly ranges of little elevation in the western part, and with numerous basins or depressions. The great delta of the Mississippi, included within the Atchafalaya and Iberville, and amounting to one-fourth part of the area of the State, is seldom elevated more than ten feet above the sea, and is annually inundated by the spring-floods. A great part of the delta is composed of sea-marsh, which also forms the whole southern coast to the Sabïne, and which, through its whole extent, is subject to inundation by the high tides. To the north of this marsh spreads out the vast level of the prairies, which is but little elevated above the former district. The western margin of the Mississippi is also a low country, intersected with numerous small rivers, and liable to inundation. To the west and north of these is an extensive region considerably broken, but nowhere exceeding two hundred feet in elevation. It consists mostly of pine barrens, interspersed with elms, cypress and honey-locust. The tract east of the Mississippi and north of the Iberville and the connected lakes closely resembles the last in surface and forest growths. It is, in fact, a part of the same upland plain, whose margin on the western side is separated from the river bed by the low inundated lands, but on the east comes up to the channel of the river, in many places forming those prominent bluffs on which stand Baton Rouge, St. Francisville, Fort Adams, Natchez and Vicksburg.

To conclude the foregoing geographical account, and present a full topographical outline of the States of Mississippi, Arkansas and Louisiana, we will now have to devote a separate section or two to a description of the Concordia, Yazoo and St. Francis bottoms.

The first of these, commencing at the mouth of Red river, under the thirty-first degree of north latitude, and extending up entirely between the Mississippi and the cretaceous piny-woods plain on the west, with its greatest breadth, where it is traversed by the Arkansas and White rivers, terminates at the high lands in the rear of the town of Helena, Arkansas. This bottom being traversed by the Washita and Tensas rivers, has scattered over its surface the beautiful lakes of Villemots, Providence, Joseph, Concordia, Lovelace and Catahoola, together with innumerable lagoons and extensive swamps.

Of this bottom the plantations along the Mississippi, the larger bayous and its numerous lakes, constitute, as yet, nearly all of its redeemed and habitable land. Near Helena, and between the mouths of the White and St. Francis rivers, where the Concordia bottom ends, commences the St. Francis bottom, which, after extending up the west side of the Mississippi to a little above the thirty-seventh parallel, terminates at the low hills about thirty miles above the mouth of the Ohio. This bottom, a greater portion of which is a forest of cotton-wood and canebrakes, like the one just described, also abounds in small lakes, lagoons and extensive swamps.

Returning now to a little below the thirty-second degree of north latitude, and crossing over to the east side of the Mississippi river, we strike the lower end of the Yazoo bottom, which commences where the bluffs begin to recede from the river just above Vicksburg, and terminates where they return to it a short distance below Memphis. The only river of this bottom is the Yazoo, which, after its formation in Carroll county, Mississippi, by the Tallahatchee and Yallabusha rivers, flows in a south-westerly direction into the Mississippi. Besides the Sun Flower, many smaller bayous flow off through the interior of the bottom, from Lakes Washington, Swan, Bolivar, Horseshoe and Horn.

This great alluvial region, extending from the mouth of Red river to the upper end of the St. Francis bottom, a distance of more than four hundred miles, with its greatest breadth (about ninety miles) opposite the mouth of the Arkansas, and supposed to contain more than twenty thousand square miles, is, notwithstanding the levees constructed for its protection, during the March and June freshets of the Mississippi and its tributaries, with few exceptions, annually submerged. On the subsidence of the water after one of these spring overflows, from topographical peculiarities, there still remains a large amount to be carried off by percolation and evaporation. Under these influences, although the surface in many places, before the first of September, becomes dry and cracked, there still remains enough of water in the streams and innumerable lakes to give, through the process of evaporation, to the air of the surrounding country, a considerable degree of dampness.

As might be expected, the inhabitants of these bottoms and their few villages, together with the towns and cities on their borders, or within their influence, are subject to malarial fevers of every variety and grade.

(*To be Continued.*)

SPECIAL SELECTIONS.

CONDITION OF THE URINE IN YELLOW FEVER.

By Dr. Blair.

[The late Dr. Blair, of Demarara, certainly carried his investigations into the pathology of yellow fever to a greater extent than any writer on the subject; having had at command the best appliance of modern science. The following extract is from the latest paper* from his pen we have seen, which was published as an Appendix to the January and April numbers, 1856, of the British and Foreign Medico-Chirurgical Review. We shall endeavor to give further extracts from this valuable paper in subsequent numbers of our Journal. The recent death of Dr. Blair is deeply deplored by all who have read his works.—E. D. F.]

"At page 93, in the 'Account of the last Yellow Fever Epidemic of British Guinia,' there is a note by Dr. Davy, in which is mentioned the discovery, by Dr. Collings, of albuminosity of urine as a characteristic of yellow fever. This important discovery was duly appreciated in the investigation of the phenomena of the present epidemic. The following remarks will embody the results of observations made on the urine of yellow fever generally, since the 6th of February, 1852. The urine is always *acid* in the first stage, and continues so generally till convalescence, when it becomes alkaline, or until it becomes heavily charged with bile. In the case of Macey (Seaman's Hospital), the urine in the advanced stages was neutral on being passed, and immediately became intensely alkaline. This happened also in the case of Ellwood (Seaman's Hospital). These instances of alkalinity in fresh urine seemed due entirely to the presence of ammonia. Numerous experiments on the specific gravity of the urine were made without any striking general result being elicited. During the early stage of the fever the urine is normal in color, clearness, and quantity. As the disease proceeds, about the third day, the color alters, and becomes that of sulphur, or primrose, or straw, or light gamboge, and is perhaps slightly turbid, with a little floating sediment. The color, during the progress of the disease, deepens, till it becomes yellow or orange; and if the case end in convalescence, the urine is very copious, and may appear *en masse*, black. As the color deepens the sediment becomes more decided, both in quantity and gravity. It is however, seldom very considerable in quantity, and might escape careless casual observation. But in one case, that of Theodore Ternaban (Seaman's Hospital), it contained a sediment which occupied one-half the urinal. If the case is going to terminate with *suppression*, it generally does so in an abrupt manner. At other times when the event is to be the same, the urine is expelled of an amber color and of an oily consistency, and in quantities of a drachm to an ounce, as if with some tenesmus of the bladder. In one case only was diuresis noticed (Barnet, Seaman's Hospital, March 1853) during the active course of the disease. This was after the use of a tobacco clyster, and was at last followed by fatal suppression. About the month of June 1853, the physical appearance of the urine was for a few weeks considerably altered. It then had a pale watery or smoky appearance, with a layer of blood corpuscles as a sediment; and in some cases the urine was *very bloody*. In uncomplicated yellow fever the urine is never buff nor red (unless from blood), and a glance of it at the bedside of the patient has been sufficient to correct at once erroneous impressions as to the nature of the case. Fevers of rheumatic and inflammatory origin have thus been discriminated from the epidemic. A pinkish sediment was observed in a few intermittent fever complications. Malingering is very rare during an epidemic; indeed, among the seamen, the chief difficulty lay in inducing them to enter the hospital early enough. There was one exception however, and he was detected at once by the urine. Quillan, on the 9th of February, 1853, returned to the hospital after having been discharged cured of an attack of yellow fever. He complained much of nausea, and showed copious vomit in his basin. Pulse was very quick, and he looked ill and prostrated. On looking at his pot, full of pale, non-coagulable urine, his case was seen through forthwith. He was partly malingering, and partly suffering from hysterical excitement. On further inquiry, I found that he came from the *Copia*, the master of which we had had fined for neglect of his sick men, and who had the very worst reputation among the seamen for his severity or brutality. The *Copia* was about to sail, and Quillan's plan for leaving the vessel was to sham sickness. The crisis of his fate rendered him nervous, and his urine betrayed him. The copiousness and wateriness of hysterical urine contrasts well with that of yellow fever. During the progress of the disease *retention* sometimes occurs from apathy. The patient does not pass it, either because he thinks he cannot, or he feels no impulse. But in such cases, if he is told to do so authoritatively, it will be done. Retention requiring catheterism occurred in six individuals in both hospitals. In one of these (Swede Anderson, Seaman's Hospital), the retention occurred in convalescence, and required three operations. When *suppression* occurs in the course of the disease, it may be regarded as the most fatal sign. In one case however (Barkway, Seaman's Hospital), the secretion was restored, and the patient recovered. About the period when the urine changes its color, and particularly if there be turbidity, if in quantity more than three or four ounces, it will, when recently passed, appear *frothy*. It then contains albumen—for ascertaining which the double test of heat and nitric acid should always be used. It is also well to be apprized that urine was observed about the month of June, 1853, in which albumen, though present, did not answer to the double test till the specimen was put aside, and suffered to *cool*. The cases wherein this happened were chiefly dissipated subjects, but not in all instances. Albumen appears on the second or third day generally; but in a few days it has been found as early as the first day of illness; and in a few cases it did not appear till the day of death and after black vomit had set in. In several instances on the fourth day, when the tongue was completely denuded, the urine was not yet coagulable. Albumen was seldom seen in aborted cases. In a few of these it appeared during their convalescence—for instance, the following may be cited: Peter Kayle, 19th of January, 1853; Henry Russel, 27th of November, 1852; M'Grigor, 19th of October, 1852; John Smith, 29th of October, 1852; Poole, 7th of December, 1852—all of Seamen's Hospital. During that period of the epidemic when torpidity of the bowels was observed, and croton oil was occasionally required as an early purge, the urine was later in becoming albuminous. In three cases the albuminosity was *intermittent* for one or two days. Albumen appeared in every fatal case of normal duration. It sometimes ceased in convalescence suddenly, always before the yellow suffusion of skin and eye, or bile in the the urine, disappeared, except in the single case of of Manuel de Nobriga, in whom Bright's disease seemed to be a sequel of yellow fever, and who, after remaining in hospital upwards of two months, left with his urine still albuminous. Between the eleventh and twentieth day of gravior cases, it generally disappeared, and its disappearance formed the criterion for the discharge of the patient from the hospital. The color of the precipitated albumen was never *white*, as it is in our cases of Bright's disease. It is doubtful if the primrose or sulphur color is due to bile. In several such specimens the nitric acid failed to bring out the bile tint, although the urine was coagulable. But the gamboge yellow and orange color were clearly referable to that source, as daily experience with Heffer's test demonstrated. As before mentioned, bile was thus occasionally detected in the urine before the eye or skin was appreciably discolored. In one case (that of Bevan, Seaman's Hospital, 1853) the urine remained abilious till the period of his death.

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* Report on the first eighteen months of the fourth yellow fever epidemic of British Guiana: By Daniel Blair, M. D., Surgeon-General of British Guiana.

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* Dr. Blair's *abortive treatment* is 24 grains of quinine and 20 grains of calomel, to be repeated every six hours till the fever is broken. This, he says, is "the greatest resolvent of fever that was ever used."

"The *turbidity* of the urine was not necessarily connected with its albuminosity. The urine may be deeply tinted with bile, and highly albuminous and yet clear. The turbidity of the urine was caused by the presence of mucous epithelial matter, coagulated albumen, and casts of the urinary tubuli. It is probable that the free acid of the urine has a coagulating power, and sometimes communicates turbidity. The presence of mucus will have a similar effect, but the turbidity then is not general, but occupies a lower stratum of the fluid, and is light and floating there, while the supernatant liquid is clear. There is nothing distinctive or of importance in this mucous condition when the urinary secretion is copious. Perfect epithelial scales are rarely found in the sediment, but broken up epithelial matter is abundant. In the case of Ternaban, before referred to, the appearance of the sediment to the naked eye was that of pus. The microscope showed this enormous mass to be broken-up epithelial matter. It was all soluble in *liquor potassæ*.

" There is a variety of urinary sediment which appears of a yellowish brown color, a little darker than the fluid in which it is contained, and exists in small curdy looking masses. It is only partly soluble in liquor potassæ or nitric acid. Under the microscope it has a fibrillated appearance and it entangles numerous tube-casts and large organic globules, and and epithelial scales. This variety is rarely seen, but when it is, it bears the most fatal import. There is another variety of the "curdy sediment" in which this particular material is deficient ; but it also entangles tube-casts, and it appears to be composed of amorphous epithelial matter, and when treated with acetic acid, shows large and small organic globules—the latter about the size of mucous corpuscles ; and these bodies seem to constitute the mass of this curdy sediment. In all cases, except the two now mentioned, the tube-casts roll separately and detached in the urinary sediment. Probably one of these may be the matter which was seen by Dr. Collings, and was considered by him to be of the nature of *casein*. I was impressed with the opinion that fibrine entered into the composition of the curdy sediment, and also that the same material constituted the basement membrane of the tube-casts.

" Although albuminosity is almost always the antecedent to the presence of tube-casts, a case (that of William Narro, November, 1852, Seaman's Hospital) occurred in which they were found in non-albuminous urine. The tube-casts are generally short, thick, and club shaped, and nearly opaque. Along with them, also, there are frequently amorphous bodies, apparently of the same material. The casts consists of a basement membrane, and are covered with minute pavement epithelial scales, between the interstices of which there is some translucency. At one end they have generally a broken-off appearance, but some are round at both ends. Few are equal in diameter throughout their whole length : some are sacculated, some fusiform. They are sometimes slightly stained with hermatosine or bile. At the broken end they are frequently destitute of epithelial covering. A long tortuous cast is occasionally seen, but being nearly transparent and without epithelium, may escape observation. As tube-casts are so soluble, not only in liquor potassæ, but also in ammonia (though more slowly), it is necessary to look for them before decomposition of the urine takes place. The scanty, acid, amber-colored urine, of oily consistency, in the last stages of yellow fever, is always highly coagulable, but contains no tube-casts, and is loaded with mucous corpuscles.

"It is very common before the urine becomes albuminous, in using the nitric acid test to perceive considerable effervescence, although the urine be acid, and no carbonate of ammonia can be suspected as being present. It is likely due to the decomposition of uric acid, or urea, by nitric acid, while the urine is heated. Is the effervescence in such cases due to an excess of one or other of these; or is it a normal condition, and are the instances of non-effervescence due to the deficiency of either? Whenever urine has been set up and examined for uric acid, it has always been found. But, except in the case of Profine Martinez, before referred to, I recollect of no instance in which it has been found as a sediment. In Martinez' case it was on the tenth day of his illness on which it was observed, and the deposit was copious. In the case of Morgan (Seaman's Hospital, September, 1852), his post-mortem urine was examined both for urea and uric acid. The hydrochloric acid test discovered the latter, but no urea could be detected, although in the single opportunity for experiment which occurred during life, it was found to be copious. The specific gravity of the urine during life was 1·023 at 85°. The sediment was loaded with tube-casts. When heated, the coagulum occupied more than a quarter of the space in the tube. When the albumen was separated by filtration, the urine yielded nitrate of urea so copiously, that it became solid. Yet the urine found in his bladder after death did not yield a trace. On the same day that Morgan's urine was examined for urea, Gilmey's, also, was tested for the same. The coagulum of his urine occupied upwards of a third of the space in the tube, and about one-half of the specimen tried for nitrate of urea became solid: it appearing, from these two experiments, as if the quantity of urea present was in inverse ratio to the albumen. In two or three experiments for phosphoric acid, it was found abundant in yellow fever urine.

"The observer of this description of urine cannot but be struck by the rarity of the presence of crystalline bodies in it. After a time he ceases to expect them. There were only six cases in which, during the active course of the disease, triple phosphates were found. There was one case in which a copious sediment of urate of ammonia was present, with the urine still acid. This was in one of the two cases of the "Livonia" before referred to, in which the red patches appeared on the skin. After convalescence, the albumen was generally replaced by the earthy salts and triple phosphates and urate of ammonia, and the tribasic triple prosphates were frequently seen. On the 4th of December, 1852, however, while examining the urine of Chugg, before referred to, I discovered distinct and well defined minute octahedra of oxalate of lime in the sediment, which also contained numerous casts of tubes, epithelium, and apparently coagulated albumen. Associated with these were vibriones and moving monads. About the same time, in W. Bertie's urine, I discovered oxalate of lime crystals and vibriones. The urine on both occasions had stood for twenty-four hours. As yellow-fever urine is such as, *à priori*, oxalate of lime might be expected in, I surmised that it had previously escaped our observation by our neglect of the rules given for detaching it, and by observing the urine too soon after emission. Some of Robert Forsyth's urine was, consequently, put by the same day for examination for this salt of lime. After remaining twenty-four hours, the following were the result of observation: "Coagulable; numerous casts of tubes, most of them perfect, of average size, a little yellowish in color. With the one-eighth inch object-glass I can detect no animalcules nor crystals. It is still slightly acid." Twenty-four hours afterwards, the phial having, in the mean time, been kept firmly corked, "sediment loaded with well-defined triple phosphates; two quartz-like uric acid crystals under the glass, slightly clarettinted. The casts have nearly disappeared. The two in view are thin and wasted, but still retaining the yellow tint. The urine is now strongly alkaline." Subsequently to this experiment, I set aside seven different samples of yellow fever urine for the purpose of examining it for oxalate of lime, under the guidance of Dr. Golding Bird's directions. The first four numbered specimens remained well corked and undisturbed fifty hours, for the deposition of crystals. The last three remained twenty-eight hours. Each specimen was first examined by a half-inch, and subsequently, by a ⅛th inch object-glass of Ross. 1st. "Alexander Muschard: Urine still acid; pellicle of fungus on surface; a few tube-casts observed, and much comminuted matter, probably of the same material; numerous sporules; no crystals; no vibriones. 2nd. George Thompson: Urine still acid; several organic globules; much epithelial matter; a very few casts of tubes, tinged yellow, sediment copious but flocculent, and floating; no crystals, no vibriones 3rd. Michael Flynn: Sediment heavy; urine fetid; slightly alkaline; four crystals of triple phosphates in drop under observation; a few small casts of tubes; numerous amorphous pieces, probably of the same material; numerous luminous little spheres which, under a ⅛th inch glass, appear to be oval sporules; some vibriones. 4th.

Antonio Ballabatch: Urine still acid; sediment dense; very numerous tube-casts, faintly yellow; three of them in drop under observation deeply blood-tinted; some blue pieces, possibly extraneous; a few organic globules; minute luminous points, which under high power proved to be vibriones; no crystals. 5th. Michael Flynn (same patient as gave number 3): Urine fetid; sediment heavy; still acid; numerous casts; amorphous pieces; two large casts, deeply-colored orange; one amorphous piece, the same; several straight transparent casts without epithelial covering; many comminuted pieces of yellow tint; no crystals; vibriones seen by high power. 6th. Robert Forsyth: Urine alkaline; copious buff sediment, part of which adheres to sides of phial; no casts; sediment consists of triple phosphates and urate of ammonia; no oxalates. 7th. Adam Smith: Urine neutral or slightly alkaline, of vinous-urinous odor; sediment half dense; triple phosphates very numerous; casts of tubes becoming thin; no oxalates." Vibriones were seen on several occasions in fresh urine; but all in the most violent cases. For instance, in the case of Bruce (Seaman's Hospital, October, 1852), three hours after the emission of the urine; and in the recovered case of the master of the *Margaret Poynter* (private lodgings), after the occurrence of black vomit, on the fifth day of his illness, a peculiarly dark sediment of fresh urine.

"On the 17th of May, 1852, the first sporadic case of *bloody urine* was noticed. A year afterwards, it became a symptom of frequent occurrence, grouping, and giving a character to the cases, and then disappearing, as has been already noticed. In a few cases, such as that of Johnston (Seaman's Hospital, October, 1852), it assumed the form of active hæmorrhage. It appeared on the first day of fever in the case of Farish (Seaman's Hospital, June, 1853). In many of these cases of bloody urine there were no casts of tubes or of epithelium; and in a few cases there was "smoky" urine with a thin layer of blood corpuscles as a sediment, when the supernatant fluid showed only a mere trace of albumen (as in the boy Alger, Seaman's Hospital, 5th of January, 1853). In some few cases the blood intermitted, as in the cases of King and M'Call (Seaman's Hospital, January, 1853). In the latter case, the bloody urine of the morning was succeeded by pale urine, with cloudy sediment, which consisted of mucous corpuscles and organic globules, in the evening. The bloody urine, in many cases, seemed a favorable sign, and the interpretation of it probably was, that the hæmorrhage, proceeding from the calyces or pelvis of the kidneys, tended to relieve the congestion of the secreting apparatus—such as in the interesting case of King above referred to, wherein the urine was bloody, highly bilious, and copious.

"These observations on the urine of yellow fever refer chiefly to that of males. With that of females, the difficulty of obtaining pure specimens was almost insurmountable. This arose, not only from the action of the bowels, but at that stage when the urine should become a study of great value and interest, the catamenia were sure to appear, whether due or not, and thus effectually embarrass the examination of the urine. My impression, however, is, from the few imperfect observations that could be obtained, that the urine is found less frequently and less highly albuminous than in males; that it is more quickly and abruptly terminated in convalescence; and that the flow of urine is fuller throughout. If the numerical method of induction applied to this subject should confirm this opinion, what is the rationale of the fact? Is the female urinary apparatus better endowed than that of the male? Is there any relation between the tendency to suppression and the calibre of the tubuli uriniferi? Or does the vaginal or catamenial hæmorrhage tend to relieve the renal congestion?

"While our experiments and observations on the urinary symptoms were going on, they were extended to such cases of intermittent fever as presented themselves. In about twenty cases of this disease, contemporaneous with the epidemic, the urine was examined for albumen, and in no instance was it found present. One of these cases was that of Josefa de Suza, who had been previously in the hospital with yellow fever. She had had copious black vomit; a bloody furuncle then appeared on the cheek, and terminated by an abscess of the left parotid gland. A singular circumstance occurred in this case. The operation of opening the abscess caused nausea and vomiting, and remained, for a day during convalescence, the stage of acid elimination. This woman returned to the hospital about two months afterwards, suffering with intermittent fever, and her urine was examined for albumen, without detecting a trace.

"On the 3d of May, a mixed case of yellow fever and small-pox occurred in a Portuguese boy, named Manuel Gomes. He had been treated in town by an experienced practitioner for yellow fever, and sent to the hospital, in which the treatment was continued. He had the red-tipped and red-edged tongue on admission, irritability of stomach, and greenish, acid ejections, flushed face, and *albuminous urine.* He had been ill three days before admission; and the day after his admission the first rash of small-pox eruption came out; and subsequently he was removed to the small-pox branch, were he recovered in due time. This case brought attention to the necessity of ascertaining the state of the urine in small-pox, in order to estimate the value of albuminosity of urine in differential diagnosis. Several experiments were then made on the urine of small-pox patients, but no albumen was found. No record was kept of these extemporaneous experiments. But on the 10th of May, all the patients in the small-pox wards were subjected to examination, with the following results :

Name.	*No. of days ill.*	*Condition of urine*	*Specific Gravity.*
—Cuffy	42	Non-coagulable	1·014
Henry Cumings	14	do.	1·012
Tom. Manning	22	do.	1·016
Manuel Gomes (above)	8	do. effervescences.	too scanty for urinometer
George Warren	8	do.	1·008
Zacharias	8	do.	too scanty.
Maria Lewis	14	do.	1·017
Elizabeth	22	do.	1·008
Diana Sam	10	do.	1·008
Eliza Grant,	8	do.	1·022
Juliana di Silva	12	do.	1·023
Susan Ward, no urine at the time of observation.			
Stephen (negro, 2 years old) eruption just out on third day		do.	
Mary M'Crae, admitted yesterday, eruption just appearing		do.	

“ At the end of April, 1853, a Portuguese child was admitted with purpura. She recovered, without medication, in about two weeks. She had no fever, nor irritation of mucous membranes, nor hæmorrhage; but her skin was thickly studded with petechiæ. This is a very rare disease in Demerara. I have no recollection of more than three cases within my experience, and this is the only one which had occurred during several years. The urine was albuminous in this case.

“ While on the subject of the urinary symptoms, it may be remarked that the report of the members of the Council of Health of Cayenne to the Minister of Marine, on the subject of the epidemic of French Guiana, announces that they carefully examined the urine, and discovered in it and the kidneys a notable quantity of *pus*. The following is an extract from the report :

> “ Reins.—Les désordres des organes urinaires ont été constant. Les reins avaient perdu leur consistance ; on distinguait cependant les deux substances, et la corticale semblait plus molle ; toutes deux étaient décolorées; ces organes étaient souvent gorgés d'un sang diffluent et contenaient un fluide lactescent ressemblant a du pus, et même du pus chez un grand nombre de sujets. Un fait digne de remarque, c'est la promptitude avec

laquelle le pus disparaissait quelques secondes apres l'incision de cet organe : il faut observer très attentivement pour constater sa presence. au moment de l'incision, car il se confond immediatement avec les autres liquides." . . "Les urines avaient le plus souvent un teint jaune verdâtre qui leur était communiqué par la bile, de la presence de laquelle nous nous sommes assurés au moyen du réactif signalé par M. Dutrouleau. Elles contenaient aussi du pus qu'il fallait de grandes précautions pour apercevoir. car il se mêlait a l'urine avec la plus grand facilité. Venait-on le filtrer. il restait sur le papier, et alors, au moyen de l'acide nitrique et de l'ammoniaque, on en constatait facilement la nature."

"Notwithstanding the great precautions taken, this statement as to the existence of pus in the kidney and urine, is clearly founded on fallacious observation; and the error is due, no doubt, to the want of microscopic aid in the investigation. If a section of a yellow-fever kidney be made, and a papilla be uncovered from its calyx, and, either with the back of your knife, or finger and thumb, you compress this papilla, a purulent looking drop will be expelled, which under the microscope will show a multitude of epithelial granules, and some more or less perfect tube-casts. Now, undoubtedly, this was the fluid which the Cayenne observers mistook for puss. The extract given was from the chapter on the "*kidneys.*" The following is the entire chapter devoted to the subject of the urine, and it will be seen from it, that they were not aware of the albuminosity of the urine in yellow fever nor the nature of its sediment, and how easily, therefore, they fell into the mistake: "URINES. La suppression complète des urines ne s'est montrée que rarement et seulement au début de l'épidémie. Quand il y avait suspension on pouvait le plus souvent, a l'aide du cathétérisme, en évacuer une notable quantité Elles étaient peu colorées ou légèrement tientes de jaune-verdâtre par la bile, quelque fois sedimenteuses. Chez les convalescents, elles devenaient parfois abondantes et colorées."

"It is almost superfluous to remark on the important indications that arise out of the study of the urine symptoms in the medical management of yellow fever. Placed in a ward of such patients, and led to the expectation of seeing no other kind of case presented to you for treatment, yet a glance at the urine at the bed-side of the patient may enable you to decide at once that the case in hand is *not yellow fever.* Such instances have happened with the fevers of peritonitis, rheumatism, and pneumonia. Not only are these symptoms extensively useful in clinical diagnosis, but they will probably be found the characteristic of the specific differences of tropical fevers, and may transfer yellow fever to an entirely new place in the classification of disease. The importance of these symptoms, as an auxiliary to defective *surface* symptoms, in identifying the true nature of the ailment, is at once apparent. The albuminosity also furnishes one of the most obvious manifestations of the disease entering its second stage, and its extension to the great solid viscera. Cases have died, as will be explained, even when the urine was full and free; but (as in the case of the mate of the *Sobraon*) life is prolonged thereby; and no guarantee of safety in one epidemic was so sure as an unobstructed action of the kidneys; and no sign, not even black vomit, so dooming as a suppression of urine. Hope then was gone. No matter how desperate the condition otherwise, if there was copious transparent urine, though ever so coagulable, and black as ink from bile, the struggle was hopefully maintained. For it was felt that the system was still competent to the elimination or decomposition of the yellow fever poison. But *suppression* after the abundant appearance, or curdy appearance of albumen and tube-casts rendered despair reasonable. The scanty oily-looking urine was generally present in cases that might be abandoned. The tube-casts had disappeared—for the capability *of washing out these plugs of the urinary tubules* no longer existed: they were irrecoverably choked; and the bulk of the scanty secretion seemed to be derived, not from the kidney, but the bladder itself. It may be safely affirmed, that for a correct knowledge of the progress, diagnosis, and the prognosis of yellow fever, the close observation of the condition of the urine is indispensable; and that after the attempt to abort the disease has failed, the prime object of solicitude and of treatment is the function of the kidney.

EDITORIAL AND MISCELLANEOUS.

HEALTH OF OUR CITY.—Hope, improperly founded, has led our citizens to anticipate a decided abatement of the prevailing epidemic during the past few weeks (we write on the 25th), but if we recur to the history of yellow fever in times past, we shall see that it has been, and is only pursuing the even tenor of its way. As far as we are able to judge of it by the past, it has unerring laws, and thus far it has not deviated from them this season. From the reports of mortality put forth in the daily prints, with the editorial observations thereto attached, our people have been led to anticipate a premature decline of the epidemic—*premature,* we say, as applied to the test of past experience. For instance, we have been told that when the eighth week of the epidemic was reached, there would be a decline, and there comes the reckoning as follows:

"1st week, 2 deaths; 2nd week, 8; 3rd, 9; 4th, 20; 5th, 25; 6th, 70; 7th, 140; 8th, 286; 9th, 312"

Surely, it is not fair or philosophical to include these first three weeks in the calculation of the progress of the *epidemic.* In all these three weeks there was a total of 19 deaths, or an averageof 8⅓—less than the total of deaths by other acute diseases existing simultaneously. Go back to the great epidemic of 1853, and we find the first death by yellow fever on May 28th; then, for the week ending June 4th, 1 death; for the week ending June 11th, 4 deaths; for the week ending June 18th, 7 deaths; for the week ending June 25th, 9 deaths; for the week ending July 2nd, 25 deaths.*

We will not stop here to discuss the definition of the term *epidemic.* We will only say that it seems to us anything but reasonable to calculate the absolute rise and progress of a so-called epidemic on any other basis than the *prevalence* of the disease in question, and a disease is only *prevailing* when it is attacking more persons in a community than any other existing disease. At the very time, however, that yellow fever was killing two, eight and nine a week, there were other diseases which claimed as many or more victims. But the moment yellow fever numbered twenty victims in a week, it had outstripped all other acute diseases—it was *prevailing.*

Beginning, then, with the week ending the 18th of July, when there were 20 deaths, and we find that the epidemics of the two years, 1853 and 1858, make nearly an even start in point of mortality—a great difference between them, however, being that the epidemic of 1853 commenced on the 2nd of July, while that of 1858 commenced on the 18th, or more than two weeks later.

For the week ending August 27th, the yellow fever culminates; 1365 persons die of yellow fever. The greatest mortality for any one day being Monday, August 22nd, when 239 died. In other words, the epidemic culminates in the eighth week.

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* History of the epidemic yellow fever, by E. D. Fenner, M. D.

Compare the results this year with those just stated, and we shall find the epidemic law faithfully observed. In 1853 the epidemic culminated in the eighth week, ending August 27th; this year the epidemic culminated in the eighth week, ending September 12th--the total of deaths by yellow fever being 472.

For the better information of the reader, however, we place before him a tabular view of the two epidemics. He who reads must understand.

1853.

DATES.	DEATHS BY YELLOW FEVER.
May 28th	1
June 4th	1
June 11th	4
June 18th	7
June 25th	9
July 2nd	25
July 9th	59
July 16th	204
July 23rd	429
July 30th	555
August 6th	947
August 13th	1262
August 20th	1302
August 27th	1682
September 3rd	749
September 10th	421
September 17th	221
September 24th	125
October 1st	85
October 8th	42

1858.

DATES.	DEATHS BY YELLOW FEVER.
June 27th	2
July 4th	8
July 11th	9
July 18th	20
July 25th	25
August 1st	70
August 8th	140
August 15th	286
August 22nd	312
August 29th	402
September 5th	448
September 12th	472
September 19th	460

Close comparison will discover to the reader some variations between the two years in the details, but the general results are the same thus far, and we confidently look forward to the fulfillment of the prophecy made by these figures. No frost is required to stop yellow fever here; the epidemic will decline under the weight of its own strange and hidden laws. Surely, no man will say that there had been a frost in 1853 previous to October 8th, and yet for the week ending on that day, there were only 42 deaths, or an average of 6 per diem. More than this, on the 13th of October, the Board of Health of our city publicly announced that "*there is now no epidemic* in the city," and yet the first frost (and that only seen on the outskirts of the city) occurred on the 25th, or twelve days after this announcement. The epidemics of Havana, of Rio Janeiro, of Vera Cruz, etc., decline without frosts, and our epidemics are as independent of their influence. The observing medical men of our city will not soon forget that yellow fever *commenced* here, in 1857, on the 10*th of September*—and we had the disease (almost wholly confined to the Hospital, and nearly all the patients coming from one circumscribed district), during the months of October, November and December; and, if we are not greatly mistaken, there were cases in January. After the time of frost in 1853, however, the disease was at its highest point, as is shown by the following statistics taken from the books of the Hospital.

In the month of September, there were admitted into the Hospital, 21 cases, of whom 15 died. In the month of October, there were admitted 145 cases, of whom 90 died. In the month of November, there were admitted 46 cases, of whom 35 died. In the month of December, there were admitted 8 cases, of whom 6 died. The first case appeared in the Hospital on the 10th of September; it was a well marked and fatal case, but was not so recorded. But on the 17th another case was admitted, about which there was no difference of opinion, and then the disease steadily increased until the 29th of October, when we find the greatest number of admissions (8) for one day. From this date the disease rapidly declined.

But, if positive evidence is required to show that frosts are not necessary to the decline of yellow fever, or that they will not stop the disease, we have only to ask of Vicksburg, Natchez, and other places North of this. Heavy white frosts there have failed to stop the disease.

We have frequently been asked by our non-professional friends if the yellow fever of this season is not worse than usual—indeed, if it is not a "different disease." We have invariably replied, *no;* and we here declare, that so far as we are capable of judging, it is the same old disease with which we became personally acquainted in 1848. It begins the same and ends the same. True, all cases are not alike; nor were they so in 1848. As long as men are found of different temperament, of different habits, and of different constitutions; as long as the conditions called affluence and poverty exist; as long as men are well informed and ignorant, just so long will yellow fever continue to exhibit itself in all its protean forms; we shall have it, first simple, complicated and malignant, and then we shall have it in all the variations of these three conditions.

Again, we have looked on with wonder at the wide limit for specifics for yellow fever, and we have been mortified to see members of the profession promulgating through the medium of the daily press, their particular plans of treatment, for the benefit of—whom? Surely not of the profession. And if not for the benefit of the profession, surely not for the benefit of the people, for it is just as impossible that every man can be his own doctor as it is that he can be his own watchmaker, shoemaker, tailor, etc. There *must* be a division of labor, physical and intellectual, and men *must* be dependent on each other. Let all lawyers and doctors and merchants, etc., turn out to making their own shoes, and we shall soon have poor lawyers, doctors and merchants, as well as shoemakers; and *vice versa.* It is utter madness, if nothing worse, for medical men to attempt thus to popularize medicine. They mistake their aim entirely, and only degrade themselves and the noblest and most difficult pursuit to which man lends himself. We are only surprised that an otherwise intelligent press lends itself to a purpose so unworthy. There are some rare exceptions to the rule, however, as will be shown by the following extract from the Bee of this date. The expression of such sentiments as these will arouse the good and true men to labor in search of truth, while the opposite course is only calculated to make the most zealous turn away from his labor in shame and disgust.

> " Seriously, we think that these newspaper essays on the causes and cure of yellow fever are not of the slightest benefit, and should rather be discountenanced than encouraged. The non-medical part of the community might be induced by their perusal to undertake their own treatment, if attacked—a policy which, we need hardly say, would be apt to prove fatal."
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> " Learned men of the loftiest intellectual attributes, and a profound and unwearied spirit of investigation, have for half a century been engaged in the effort to explore the mysterious etiology of yellow fever, and to discover the most efficient means for its prevention and cure. If their researches have effected little more than to render darkness visible, it is not likely that in the ephemeral lucubrations of a daily newspaper we are to look with confidence for a solution of the enigma. Our advice to the unacclimated is to pay no attention whatever to these publications, but if seized with symptoms of the disease, immediately to send for a physician of reputation, and minutely to obey his directions."

Through these public prints, too, the world is told that yellow fever so baffles the skill of medical men, that one might readily conclude that death is the rule—recovering the exception. Yet, how far is this from the truth ! While the intelligent, well educated and honest physician is willing to admit that there are cases of this disease which will march directly on to death in spite of him and of all the care and attention which can be bestowed, still he knows, in all truth, that when he is called to a patient recently seized with the disease, and who is to be *well* nursed, he *expects* recovery as the rule, death as the exception : nor is he deceived. On the other hand, if he is called where he can have for his patient no good nursing, nor reliable medicines, the order of things is reversed—he *expects* death to be the result. We honestly believe that the mortality under the first named circumstances does not reach more than 10 per cent., whereas, in the last case, we only wonder that it stops at all short of 100. If any reasonable man will go with us into the lanes and alleys of this place, where the mass of those sick with yellow fever lie, he will, with us, wonder that any at all get well. Again, if any interested parties will go with us to some other places where the poor sick lie, we will *make* them wonder why all do not die. It is impossible that the people can judge of cause and effect by a superficial glance at things—by merely reading the daily mortality in the papers; consequently an opinion expressed by them is nothing worth. We can to-morrow show all the benevolent associations of this city a field for the exercise of their praiseworthy exertions which is not only wholly uncultivated, but which if cultivated, at half the expenditure of money they are now making, would embrace every indigent sick man and woman in the city, and reduce our mortality to a point which would not only be more creditable to us as a civilized people, but would be satisfactory to every reasonable man.

P. S. September 27th. At this late hour we annex a postscript to say, that through the polite attention of Dr. Baldwin, Secretary of the Board of Health, we are enabled to lay before our readers the total mortality for the week ending this day—444 by yellow fever. This shows clearly that the epidemic continues to decline. In other words, we have seen the worst for this season, and may confidently predict the rapid disappearance of the disease.

THE COMPARATIVE COST OF ATTENDING MEDICAL LECTURES NORTH AND SOUTH.—All those who read our pages know full well that we warmly advocate the patronage of Southern Medical Colleges by Southern young men. This we always expect to do, and without saying one word in the way of detraction from the real merits of institutions abroad We heartily believe that New Orleans this day offers greater advantages to the student of medicine than any other city in the Union, and the most rigid scrutiny of the skeptic can only tend to corroborate the opinion. There are those, however, who induce the student to believe that the Northern or Western colleges are more desirable on account of the cheap rate of living, and the result is that many a young man, to whom even a very small sum of money is a great object, passes by our very door in search of that which is cheaper. If these young men are simply told that board and lodging in a Northern city can be procured at sixteen to eighteen dollars, while in New Orleans the same costs twenty to twenty-five dollars, then he has merely to strike the difference, and he finds that he can save (?) from twenty to thirty dollars, or even forty dollars.

But let us go a little farther into the matter, and perhaps we shall find that it is absolute economy to stop at, or come to, our city. On an average, it will cost the students who leave the States of Texas, Louisiana, Mississippi, Alabama, Florida, Georgia and Arkansas, fifty dollars to reach the Northern medical schools (some, of course, paying rather more; others less, according to locality). Then they must pay fifty dollars to get back home again—making one hundred dollars for the round trip. Repeat this the second session, and there is a sum total of two hundred dollars for travelling expenses.

Now, from any of the States named, (Tennessee included) the student may reach New Orleans at an average cost of certainly not more than twenty dollars—we think fifteen dollars. A great number can reach the city by the Mississippi river, and by our railroads, for five to ten dollars; while those most remote would not have to pay more than twenty-five dollars. The difference, then, in the travelling expenses of the two sessions is in favor of New Orleans to the extent of one hundred and twenty dollars—or an absolute saving of forty to sixty dollars. Everything else, except board and lodging, the student can get as cheap in New Orleans as elsewhere—text books, fuel, lights, clothing, etc. More than this, he pays no hospital fee of ten dollars, and yet has access to the very best hospital in the land; and he pays two dollars and a half, instead of ten, for subjects.

Such is the result of facts and figures accurately set forth. It is cheaper (if this be the great object) to attend lectures in New Orleans.

ALBUMINOUS URINE A DIAGNOSTIC SYMPTOM OF YELLOW FEVER.—The late Dr. Blair called the attention of the profession to a diagnostic symptom of yellow fever which is likely to prove of very great value. He says that the urine in the first stage is invariably acid, and that when the second stage sets in, generally the second or third day, it becomes highly albuminous, and continues more or less so till the disease runs its entire course, when, if it terminate in recovery, the albumen disappears and the urine becomes alkaline. Dr. Blair states further, that he applied this test in various other fevers, such as intermittents and remittents, the fever of small-pox and rheumatism, and that albumen was not detected in any of them.

I have recently tested these observations in a number of cases at the Charity Hospital, and found them uniformly correct, so far at least as relates to the acid and albuminosity. The value of this discovery cannot fail to be appreciated when we recollect what a diversity of opinion among physicians is always exhibited in regard to the first cases of yellow fever that make their appearance at places where this disease appears and disappears periodically, and at times too, when various other types of fever are prevalent. At this very moment, we are told an animated discussion is going on among our French *confreres* in the lower part of the city, in respect to the type of the prevailing epidemic, especially amongst *creole children.* The fallacious popular idea that creoles are entirely exempt from yellow fever—an error which we exposed more than ten years ago, and which we thought would certainly be abandoned after the numerous cases that occurred in the great epidemic of 1853, seems still to be tenaciously clung to, and although native infants are now dying daily of fever terminating in yellowness, black vomit and other hæmorrhages, the disease is termed *pernicious fever* for no better reason than because, forsooth, creoles do not have the yellow fever. We hope Dr. Blair's diagnostic test will at once be applied to these cases, and the question definitely settled. The settlement of this question affirmatively need not add to the existing terrors of yellow fever. It will not make the state of things a whit worse than it has always been; on the contrary, it may save the lives of many children who might otherwise be the victims of a false diagnosis and a pernicious treatment. We have long been convinced that all children born in this city are liable to yellow fever; but they seldom have it to a dangerous degree—in fact the cases are generally so mild as to attract but little attention. The greater malignancy of late epidemics is shown by the increased mortality among native children as well as by several other facts.

Another *popular error* which we pointed out in 1847, is now almost entirely abandoned—it relates to the susceptibility of the negro race to the poison of yellow fever. Formerly, it was supposed that negroes, like city creoles, were exempt from this disease, but we soon became convinced that this opinion was erroneous—in fact, that the unacclimated negro is fully as susceptible as the white man, though it affects him much less seriously. In former times it was considered highly derogatory to the skill of a physician to suffer a negro to die of yellow fever; but lately, from some cause or other—either the greater malignity of the disease or improper treatment, we hear of numerous deaths among the blacks; not always thus marked, it is true, but most probably from yellow fever. We repeat, that should the diagnostic symptom brought into notice by Dr. Blair be fully confirmed by further observation, it will prove of immense value, not only to the profession, but to the community. The attention of the reader is specially directed to the observations of Dr. Blair on this subject, to be found in another part of this number.

We shall give our own observations in our next number. F.

CONTAGIOUSNESS OF YELLOW FEVER.—Speaking of the health of St. Louis, the editors of the St. Louis Medical Journal say: "Our Hospitals, too, have been well filled with patients, and occasionally a death has taken place from yellow fever imported from New Orleans, but in no instance has this disease originated here, nor spread even when introduced into a crowded ward, showing most conclusively its non-contagiousness. This being the case, not only during the present summer, but for years past, the question arises, where is the necessity of our pretended quarantine? In our opinion, it is altogether useless, an unnecessary expense to the city, and an unjustifiable interference with free commerce, and we would therefore gladly see it abolished."

Elsewhere, in the same number, the editors say: "The profession in New Orleans, we believe, are pretty well united in rejecting the idea that the disease is imported, and have settled down in the belief of its local origin; hence, they oppose the quarantine system as an unnecessary restriction to commerce, and as affording no protection against fever. This is sensible."

As no man's say-so is worth a copper except it is found on paper, where it stands in evidence against him, and as very few of "the profession in New Orleans" have ever put their opinion on this interesting subject on paper, we think our friends of St. Louis assume too broad a ground when they say that there is anything like unity of sentiment here. The opinion of the "profession" here is something like the Irishman's flea, "when you put your finger on him, he is not there."

While we firmly believe that yellow fever can and does originate here, still we know there have been seasons of exemption from the disease, and if we could in such seasons prevent the introduction of cases from abroad it would certainly be well; for, as sure as there is such a disease, yellow fever is communicable from one person to another—reliable testimony in favor of which point can be freely adduced. But any man who will look at the geographical position of New Orleans, and then consider the characteristic disposition of our people to evade laws which are inconvenient in a business point of view, will see at once that it is impossible to quarantine the place. No one has been more anxious to see New Orleans rigidly quarantined than ourself, for we earnestly desire to see the vexed question permanently settled; but we despair: it cannot, for reasons as plenty as blackberries, be accomplished. For instance, quarantine against yellow fever ports was declared on the 15th of June of this year; yet what inquirer does not know that yellow fever was in Havana long before that time? If yellow fever is contagious on and after the 15th of June, why is it not so previous to that date? But it is useless to talk about it; this is a "free country," and our people will claim the privilege of carrying yellow fever about. Last summer New Orleans was quarantined; yet passengers from Havana by the New York and Mobile steamer would take the New Orleans and Mobile mail boat in Mobile Bay for our city. Of course, the Captain of the mail boat would tell them they could not go to New Orleans; that they must "get off" at Pass Christian. "Certainly, Captain, we will get off there," and when the boat arrived at the Pass they would walk ashore at the bow of the boat ("get off"), and then *go on board again by the stern!* They had obeyed the Captain; he had told them to "get off," but had not told them that they must no get on again. Pass Christian was not an infected port, and passengers had a right to travel thence to New Orleans.

And so it goes; quarantine can only be instituted by human ingenuity, and this ingenuity is not all on one side. There will always be strong opposition to it, and hence all the ingenuity of the opponents is taxed to evade the law.

Still, yellow fever is communicable (all negative evidence to the contrary notwithstanding), though not in all latitudes at the same time, nor under all circumstances.

Books and Pamphlets Received.—To Thomas L. White, Bookseller, Canal street, we are indebted for copies of the following works from the press of Samuel S. and Wm. Wood, New York:

"The Uramic Convulsions of Pregnancy, Parturition, and Child-bed. By Dr. Carl R. Braun, Professor of Midwifery, Vienna. Translated from the German, with notes, By J. Mathews Duncan, F. R. C. P. E., Lecturer on Midwifery, etc., etc." We most cordially recommend this little book to our readers. No thinking man can arise from its perusal without feeling that he is more enlightened on the subjects of which it treats than when he began. The translator tells us that it is merely an extract from the new text on midwifery of Dr. Braun. If this entire text book is as complete in all its parts as this single is, it is indeed a pity that we had not the entire work in our libraries.

"Mind and Matter: or Physiological Inquiries; in a series of essays, intended to illustrate the mutual relations of the Physical Organization and the Mental Faculties. By Sir Benjamin Brodie, Bart, D. C. L., etc. etc; with additional notes by an American Editor." It is refreshing to read a book like this. The abstruse and technical style usually adopted by writers on this and kindred subjects frightens off many readers; for the mass of those who do read like to comprehend without putting the mind to a stretch which speedily wearies it. It has certainly been ordained that the few shall deeply study for the benefit of the many, and it is a great pity when the student at last puts forth his lesson couched in such language and style of expression that laborious thought and a dictionary are indispensable to a proper comprehension of his meaning. The book before us is simple, and yet forcible in style, and, so far from tiring, it really captivates the reader and pursuades him to reflection. We do not so much like the colloquial style adopted by the author, inasmuch as a man is not apt to argue both sides of any question with equal severity, and he who thinks but superficially while reading is apt to imagine the argument exhausted, when it may be that the batteries on one side are only smothered.

"Of Nature and Art in the Cure of Disease. By Sir John Forbes, M. D., D. C. L., etc., etc. From the second London edition." Here is a very interesting little book, which purports to tell how far nature and art, together and separately, may be relied on for effecting the cure of disease. More, the book is not only intended for the use of medical men, but the most prominent object of its mission is to enlighten the unprofessional masses on these important points. We wish we could spare the space to notice the book *in extenso*, but we must content ourself with both complimenting and condemning it in general terms. In the way of compliment, we can with all candor say, that the perusal will amply repay any one, as there are certainly many valuable hints. But we feel bound to say that we think the author does great injustice to the profession when he utters such sentiments as the following:

"It has not, I apprehend, been shown by positive data of sufficient authority to be trustworthy, that in the epidemic visitations of such violent diseases as the plague, cholera, small-pox, typhus, the yellow fever, the amount of the natural mortality has been in any material degree altered, on the large scale, by the interference of Art; and I confess that, from all that I have witnessed myself, and from all that has been authentically recorded by others, on this matter, either in the present or in the past time, I have come to the conclusion that such a result can hardly be shown. Not that I deny absolutely the power of Art to some individual lives in such cases, but merely assert that the proportion of lives saved by it is too small to modify the general results in a way recognizable by statistics."

When Mr. Forbes will tell us what the "natural mortality" in such epidemics is, then we shall have the proper basis for statistical calculations of the results of the interferance of Art. In every such epidemic, in civilized communities, Art, in every shape, from the man of science down to the one-idead homœopathist, hydropathist, or charm doctor; is at work amongst the sick; no community is left alone to demonstrate the terrible problem of the "natural mortality"—then, where is the starting point from which the author works out a conclusion which he would so freely disseminate for the enlightenment of the world? Again, if the author does not "deny absolutely the power of Art to save individual lives in such cases," on what bases does he stand when he asserts that Art does not affect the "natural mortality"? If Art can save one life, we have the best starting point in the world from which to argue that she can, in proper hands, save two, and if two, then four, and so on. In saving one life the so-called "natural mortality" is affected, notwithstanding the standard of that mortality cannot be fixed. We will not talk about *curing* disease; for that is a word which scarcely admits of definition; but we are sure there is no intelligent physician who has not observed in all epidemics of yellow fever, the soothing effect of a foot bath, or of ice applied to the head, or of sponging the surface under the bed clothes when the skin is dry, or of perspiration promoted by a draught of hot tea, or of the thousand and one little things which may be done for the sufferer; and where these things are promptly done, as occasion may require, we find our patients recovering in far greater number than when the same attention is wanting. What common observer is there who has not long ago acknowledged the truism that the mortality in all epidemics is far greater in the lower than in the higher grades of society? So true is this, the physician absolutely expects his patients in the one sphere to recover, while in the other, for the want of many, or even all of these attentions, he expects them to die. We do not pretend to say that cold lotions to the head, which so often relieve intense head-ache, *cure* yellow fever; or that hot pediluvia, which afford so much relief by promoting perspiration, *cure* yellow fever; but we do assert, that these and many other agents which may be skillfully brought to bear on the patient do so modify and ameliorate symptoms as to lead many on to recovery who would, and do otherwise, die. If every man, woman and child taken with yellow fever could be subjected, first, to skillful advice as to the application of simple remedies, and then to a most rigid and faithful system of nursing, as surely as yellow fever exists as an epidemic it would be largely disarmed of its terrors; but this is impossible under the natural organization of society, and hence the unfortunate results. So far from there being any just ground for reproaching the Medical Art, we think it is only to be praised for what it does. The root of the evil lies in the natural social organization of society then, and the mental obliquities of the poor mortals who compose it. While the ranks of the profession itself, are but two freely filled with ignorant pretenders (which is an evil great enough for an afflicted community to groan under), every man and woman conceives him or herself to be in some degree competent to treat the prevailing disease, on the plea of experience derived from having lived through one or more epidemics, and hence they either assume the exclusive control of the sick during the precious moments for proper treatment, or they so interfere with even the most reliable medical men as to counteract all good. If these and other like difficulties, together with the greatest lever of destruction, poverty, could be removed, then should we surely see a vast diminution of the mortality. Whether the half of this is to be hoped for, philosophers must determine.

The fact is, Dr. Forbes seems to have set out with the object of exhausting his subject, and we think he has succeeded, but with the unfortunate result of having awarded too much power to Nature and not enough to Art.

Every intelligent and candid medical man will admit that he encounters cases of acute disease, on which, from the very onset, he can read the impress of the hand of death, and to these the medical art is of no avail; but he knows, too, that there are a vast number which, if left to Nature, would perish, but which, if judiciously managed, will be safely conducted back to a state of health. Whether this is to be called *curing* them, must be determined by those who like to split hairs.

"Tilden and Company's Book of Formulæ." "Journal of Materia Medica and Pharmaceutic Formulary." To Tilden & Co., manufacturers of medicines, New Lebanon, New York, we are indebted for these two precious books. Perhaps we may be, by some of the puffers of Tilden & Co.'s extracts, etc., considered guilty of an unpardonable heresy, when we say that we consider these prints, like their medicines, catch-penny in every sense of the word. Long have we looked on with wonder and amazement at the praises bestowed by our medical journals on these so-called American medicines and their manufacturers, and in vain have we awaited the beginning of that general expression of condemnation which is their inevitable doom when honest opinion, based on actual personal observation, shall burst from the strange meshes which entangle it.

It is a matter of history, that the establishment of Tilden & Co. originated in the law passed by Congress some ten years ago, which subjected all imported drugs and medicines to a rigid system of inspection before they could be put upon the market. Our people had long been groaning under a monstrous and most iniquitous trade in largely adulterated, or wholly artificial drugs, and the evil had at length arrived at such a point as really to induce our president-making Senators and Representatives to give the subject their attention. The law was passed and inspectors of drugs and medicines appointed at the different ports, with how much benefit under our loose system of observing such laws, perhaps somebody will be bold enough, ere many more years, to openly demonstrate. The passage of this law was a pregnant theme for all the medical editors in the country, and there was a great stir generally amongst medical men, who now began to hope to be able to give occasional doses of pure medicines. What an auspicious moment for the establishment of an *American* manufactory of extracts, etc., etc.! And Tilden & Co. were astute enough to discern the happy star. They began the business; they puffed themselves, and the press, medical and non-medical, puffed them. The very atmosphere rang with the universal cry of "Tilden's Extracts." And who was more readily captivated with the song than ourself? We are an American—a genuine Native American (though not in the now political sense of the term), and we patriotically prescribed Tilden's Extracts. But, about the time that our enthusiasm, or patriotism, was at the highest point, we removed to the country to pursue the practice of medicine. Of course, we carried pots on pots of Tilden's Extracts with us, but ere a month had elapsed the pots were opening spontaneously, and then our eyes began to open too. Before we had had an opportunity to prescribe a single dose of a single extract, they were all in a state of *fruitful fermentation*, and, doctor as we were, we were compelled to throw, at last, this physic to the dogs—though, if we recollect aright, the very *hogs* refused to eat them.

But, badinage aside: we can honestly affirm, that from that day to this we have on many occasions tested Tilden's extracts, and in all that pertains to a genuine article of medicine, they are so far inferior to the English extracts that it is an insult to the English manufacturer to institute a comparison. If Tilden & Co. could have seen what we saw one day in the store of one of our most respectable druggists, we are sure they would have blushed for very shame. Not only were their extracts fermenting and bursting the tops off the pots which contained them, but the tops of the wooden boxes in which the pots were packed by the dozen were being lifted from their position! And if Tilden & Co. desire to know what we think of the *potency* of their extracts, as compared with the English, we unhesitatingly say, that while they are infinitely superior to the latter for their lifting capacity, they are comparatively worthless as medicines. We have yet to see a genuine English extract fermenting in our city, while it is the exception to see a Tilden extract free from fermentation. More than this, to our taste, smell and sight, Tilden's extracts never come up to the standard prescribed, while all the English do. And, lastly, we have with impunity (and with disappointment) prescribed and administered Tilden's extracts in four times the quantity that we would dare to prescribe or administer the English.

Perhaps we shall encounter the displeasure of Tilden & Co. for giving expression to these opinions of their wares, but we deem it the imperative duty of every man, whether professional or other, to openly condemn spurious medicines wherever found, or from whatsoever source they may emanate. We regard traffic in worthless medicines as little better than highway robbery, or even murder itself, and we can never, either directly or indirectly, countenance the same. He who sells a bad drug is committing two damnable acts—he is compromising the life of his fellow man, and he is working the ruin of the noblest Art to which man applies himself; and he should be unqualifiedly condemned. We are more than surprised when we look on and see with what indifference medical men administer worthless medicine, and thus daily help to crush the very Art they would exalt. At this very moment, we will venture to say that a prescription for "Morson's Aconitine" would be filled at almost any apothecary shop in New Orleans, and yet all that Morson could spare a medical friend last summer, who visited his laboratory in person, was 45 grains; and one might, almost with certainty say, that the genuine article is not to be found in five shops in the city. Morson, himself, was amused and surprised when our friend told him of the abundance of his aconitine in the United States, for well he knew that he had sent but little to the country.

When Tilden & Co., or any other American house, will furnish medicines equal to those imported, we shall be the first to give them credit for the same. We do not mean *samples* for inspection, but an article uniformly good in every pot sent into the market. It is one thing for our importers to hand the inspector a lump of genuine opium for inspection, and another to smuggle through a thousand pounds of the spurious article. Will not somebody now call the attention of Congress to the important matter of regulating the home manufacture of medicines? We want inspectors of the home products as badly as we ever did of the imported.

P. S. We had just folded away the above for the printer, when here comes from the post-office, a letter from Tilden & Co., asking us to stitch into our journal their advertisement of "*Tilden & Co.'s Sugar Coated Improved Compound Cathartic Pills.* (WITHOUT CALOMEL)." The advertisement tells us that the formula of this pill is "known to physicians"—that it is the same as the compound cathartic pill of the Pharmacopæia, *except* the calomel—this being substituted by something else, in order to please those who are prejudiced against its use.

If this is not a practical illustration of "adding insult to injury," then we are complimented. No, Messrs. Tilden & Co., go to those who puff your extracts; we have never endorsed you, and cannot advertise your quack medicines.

"Physiology, Pathology and Therapeutics of Muscular Exercise: a Paper read before the Cook County Medical Society, and published at their request. By W. H. Byford, M. D., Professor of Obstetrics, etc., in Rush Medical College. Chicago: 1858." From the author, and a very interesting and instructive pamphlet.

NEW REMEDIES IN YELLOW FEVER—CHLORINE AND VERATRUM VIRIDE.

By E. D. FENNER, M. D., Edr.

Up to the present time the treatment of yellow fever may be said to have been almost entirely empirical or based on experience alone; but empiricism may work on till doomsday and in the hands of skillful practitioners, achieve great success, yet fail to satisfy the demands of the inquiring and philosophic mind. *Science* alone can do this; by which I mean a knowledge of the morbific cause and an explanation of its *modus operandi* in the production of all the phenomena of the disease, as well when it terminates in recovery as in death. In ignorance of these fundamental desiderata, the practice must necessarily be empirical—like one groping in the dark, who might stumble upon a jewel, but at the same time be liable to fall into a pit—a thrust with a dangerous weapon by the looker-on of a personal combat, equally liable to strike friend and foe. By observation, much valuable knowledge has doubtless been attained, and likewise, great skill in practice has been acquired from experience; but all this is still unsatisfactory and incapable of being taught or transmitted to those who are to come after us.

Various plans of treatment and all sorts of remedies have been resorted to in yellow fever, but neither plan nor remedy has ever yet attained universal approbation and acceptance. Cases have been known to recover under every plan, and others to die in spite of every known remedy; but the most important fact of all is, that persons may recover from the disease without any treatment at all—a problem full of instruction, but as yet, remaining unsolved.

Of all the remedies for this disease that have gained notoriety, the following four are the only ones that have been supposed to exert a special action on the morbific cause or *materies morbi*, viz: *mercury*, *quinine*, *salines and the tincture of the muriate of iron.* The virtues of all the rest, such as emetics, cathartics, diaphoretics, diuretics, blood-letting, counter-irritants, etc., are attributable to their generally known therapeutic action.

Of the four remedies just named, not a single one has succeeded in commanding general confidence in the powers claimed for it. Mercurial ptyalism is no longer relied on as a protection against the dangers of yellow fever; the introduction of salines into the system for the purpose of counteracting the morbid changes of the blood, has been tried in vain; the once vaunted powers of quinine are fading away, and the tincture of iron has fallen into probably unmerited neglect, after a brief but brilliant career.

A correct theory is absolutely necessary to scientific and successful practice; and the physician, if there be one, who has *no theory*, is a mere empiric. In a case of yellow fever, we see a *contest between the inherent conservative powers of the body and a lethific agent;* be it a poison, a ferment, animalcule or vegetable cryptogam. In the course of this conflict, we may suppose that one or more of the following occurrences will happen: The morbific agent may be only capable of causing a perturbation or disease which can be overcome by the conservative powers of the system, with or without medical aid; or it may be so powerful as to defy the conservative powers as well as any known remedies, and prove *inevitably fatal.* 2. It may be neutralized in the system by remedial agents, in the way that certain poisons are disarmed by certain known antidotes. 3. It may be eliminated from the system by the emunctories, or secreting and excreting organs. 4. By the disturbance of the functions which it produces, it may cause the retention of elements in the system which have fulfilled their legitimate purposes and can no longer be retained with impunity; thus adding fuel to the descructive fire that is already raging.

The physician's theory of each individual case, based on its history and symptoms, will direct his practice towards the accomplishment of one or more of the above objects. If nature is doing all that is necessary, of course he will not interfere; but when she appears to be unequal to the task, it becomes his duty to render such assistance as the indications of the case appear to clearly point out.

Here we might pause and inquire, *what, that is really beneficial*, has as yet been discovered? *and wherein* lie the principal defects of our practice in this disease? The answers to these questions would require a critical review of all the various plans and remedies that have been resorted to in the treatment of yellow fever; a task which we have neither time nor space to perform at present.

The foregoing remarks have been thought necessary as a prelude to the introduction of two new remedies in the treatment of yellow fever, which, so far as a limited experience can testify, promise to fulfill in a most satisfactory manner, three of the most important indications, viz: 1. The controlling of excessive febrile excitement: 2. Maintaining the free and continued action of the great eliminating organs, the skin, liver and kidneys: 3. And consequent thereon, the preservation of the integrity of the blood and tissues. These remedies are *chlorine, and Norwood's tincture of the veratrum viride.*

I brought myself to the experiment of chlorine by reflecting on the course and nature of this fever, and the want, long felt, of something that is capable of neutralizing the action of the morbific cause, or of assisting the efforts of nature to eliminate it from the system. Secondly. By recollecting the happy effects of the muriated tincture of iron which I witnessed in the epidemics of 1854 and 1855. Thirdly. By the flattering testimony in favor of chlorine in malignant scarlet fever, to be found in Watson's Lectures; and lastly, by the therapeutic action of chlorine and the chlorate of potass, as recorded in Pereira's great work, and other late writers. I resolved on using the chlorine mixture mentioned in Watson's Lectures, p. 1002, third Amer. edition. The plan adopted was, first to purge the patient moderately with castor oil or some other simple cathartic, and bring on free perspiration by means of the hot mustard foot bath, warm orange-leaf tea and covering with a blanket; then to give to an adult two tablespoonsfull of the chlorine mixture every two hours.

My first experiments succeeded beyond my most sanguine hopes, and every case in which it was timely applied, excepting one, recovered in a most satisfactory manner. The medicine sat well on the stomach, but occasionally caused griping, which disappeared on omitting it for five or six hours or reducing the dose to a single tablespoonfull. Under its use the febrile excitement soon declined, headache disappeared, and the kidneys and liver acted freely. The dose was diminished or the interval prolonged as the fever went down. In most cases, no other medicine was given till the end of the third or critical day, when the chlorine was stopped and I gave what is called here the Sydenham Mixture, composed of the phosphate of lime and gum arabic suspended in equal parts of orange-flower water and distilled water. The bowels were kept open by enemata, as occasion required, and the patient was allowed to drink orange-leaf tea and barley water *ad libitum.*

In this manner, I had treated some eight or ten cases with satisfactory success, and was applying it to all my cases, both in private practice and the Charity Hospital, when I happened to meet one of the most extensive practitioners of our city, who stopped to relate to me a very extraordinary case he had just been attending. He then asked me whether I had ever tried veratrum viride in yellow fever? I told him I had not, but had long intended to do so. He begged me to try it in the Hospital, as he did not like to experiment on patients in private practice, yet felt the necessity of our endeavoring to find out some more valuable remedies than any we now have. I promised him to make the experiment very soon, and we parted. On the same day, I learned through the newspapers, that Dr. White and another physician of Charleston, had met with extraordinary success in the treatment of yellow fever with the veratrum viride. In the evening, I went to the Charity Hospital for the purpose of commencing my experiments with this medicine. This was on the 20th instant.

In Ward 16, (under my charge,) I selected three cases, two of which had entered since my morning visit.

Case I—Was a fine young Irish sailor, aged 16, who had arrived here from Liverpool only five days previously, and was attacked this morning; entered the Hospital at 5 P. M; had taken a mustard foot-bath and a dose of castor oil, which had moved his bowels freely. He had headache, injected eyes, skin very hot and sweating; great thirst; pulse 120. Ordered cold applications to head, orange-leaf tea, and five drops of Norwood's tincture V. V. every three hours.

September 21st. Found patient sound asleep and sweating freely, eyes less injected, headache much relieved, tongue moist, less thirst, pulse 100. Continued the V. V. every four hours.

5, P. M. Very quiet, but pulse up to 112.

Semptember 22d. Cool, quiet and moist, pulse 88. Found the urine acid, but not coagulable. Continue treatment; move bowels by enema.

September 23d. Cool and quiet; rested well and apparently convalescent; pulse 70. Stopped the V. V. and allowed chicken water. On testing the urine, found it albuminous.

On the 25th, the patient had slight exacerbation of fever in the evening and took neutral mixture, tablespoonfull once an hour. Urine more albuminous.

September 26th. Patient does not improve; has exacerbations in the evening; bowels open, sweats moderately; urine now highly albuminous and bilious; threatened with hæmorrhage from gums. Put him on my chlorine mixture, tablespoonful every four hours.

27th. Has slight hæmorrhage; is improving.

28th. Rested well; is cool, quiet and hungry. Now fairly convalescent.

Case 2.—Young Irishman, aged 19, was attacked on the morning of the 19th September, and went immediately to the hospital. I saw him at my morning visit and prescribed hot mustard foot bath, and a dose of castor oil. After the bowels are well purged, to take two tablespoonsfull of the chlorine mixture every two hours. On the morning of the 20th he was doing well, and ordered to continue the mixture.

At my evening visit I resolved to put him on the V. V., and found his condition as follows: Considerable fever; skin hot and rather dry; pulse 100; tongue white, with red edges, and moist; considerable thirst; pain in the head and back; bowels rather loose and uneasy. Ordered five drops of the V. V. every three hours and the chlorine mixture immediately.

September 21st. Much better; face and eyes less injected; skin cooler and sweating freely; pulse 72; headache relieved; has some pain in the back; thinks the chlorine gripes. Ordered to prolong the interval of the V. V., and to take less of the chlorine. His urine is acid but not coagulable.

September 22d. Completely relieved; clear of fever and pain; pulse down to 72; is quite hungry; urine acid but not yet coagulable. He continued to improve steadily, and was discharged at his own request on the morning of the 26th. His urine was then beginning to show albumen.

Case 3.—Was a German aged about 30. Entered hospital this evening, September 20th, in the second stage of the disease, (third day) having been vomiting and purging from the commencement of the attack. He was very weak; eyes much injected; headache; sweating; tongue moist; still vomits; pulse 100 and feeble. Wishing to see the effects of the V. V. in a desperate case, I prescribed five drops every three hours; sinapism to epigast.

September 21st. Pulse 80; skin cool, moist and of deep red color; has no pain; attempts to vomit; very thirsty; urine abundant, acid and highly coagulable. At noon his pulse was down to 54; has hiccup; stop the V. V. 5, P. M., hiccup checked. Contrary to orders he got out of bed and the hiccup returned. He lingered till the 23d and died. He passed urine to the last day, and it was highly albuminous.

In reflecting on these first cases, it appeared to me that case one had such a lingering convalescence from the want of something besides the veratrum. This medicine had displayed its wonderful power of subduing febrile excitement, but there was something wanted to act upon the blood, liver and kidneys; that the rapid convalescense of case two was probably due in some measure to the good effects of the chlorine mixture he took for about thirty hours; and that case three could hardly be expected to recover after removal to a considerable distance, in the second stage of the disease.

From that time I have continued to use these two remedies in nearly all cases that have come to my charge in the first stage, and at the hospital in all stages while there was febrile excitement. I directed the remedies to be taken alternately, at intervals of two hours, i. e., first the veratrum, and two hours afterwards the chlorine; and so on till the febrile excitement is sufficiently reduced, the pulse of the adult brought down to 70 beats in the minute, when the former is stopped, but the latter continued in smaller doses. Previous to giving these medicines, the bowels should be well evacuated, but not severely purged. Subsequently, they may be kept sufficiently open by enemata. Sweating should be promoted by mustard foot baths, warm teas and covering with one or two blankets. Yellow fever patients are frequently sweated too severely, but I have hardly ever known one to do well without maintaining a good perspiration for three days.

It is proper that I should mention some of the troubles that may arise in the use of these remedies, for there is no medicine that possesses great remedial powers without being likewise capable of doing injury if improperly administered.

The veratrum viride, as is well known, is a very powerful medicine and liable, when taken in excess, to produce great prostration of the vital powers. It should therefore be very cautiously given, and its action watched with the utmost care. There should be a considerable interval between the doses, so as to allow time for the medicine to display its effects. It will surely reduce the frequency of the pulse and febrile excitement generally; but if carried too far, it produces alarming prostration, great restlessness and a peculiar wild delirium, all of which will pass off in a few hours and may be relieved by a little paregoric and brandy. I have never heard of its producing fatal effects, although it has recently come into very general use in all parts of the country as a remedy in pneumonia and typhoid fever. The dose I have directed is of course intended for adults, and must be varied according to the age of the patient. It should be stopped gradually after the pulse falls to 72; and the remainder of the cure left to the chlorine mixture, with such adjuvants as the particular case may require.

In respect to the use of the chlorine mixture, I have as yet met with no other trouble than the occasional griping before mentioned, which has never proved at all serious. This medicine certainly acts finely on the liver and kidneys. My experience with this remedy as yet only covers some twenty-five or thirty cases, an extent too limited to justify any further remarks at this time.

In offering these hasty observations to the profession, at the present time, I beg leave to say that my only object is to call attention to some new remedies, which I have reason to hope will prove to be highly beneficial in the treatment of this terrible Southern pestilence, and to allow physicians residing at places where yellow fever is now prevailing, the opportunity to test their virtues at once. This, I hope, they will do at once, and let us know the results of their observation at their earliest convenience.

In conclusion, I must be permitted to say, that if the veratrum viride should ultimately turn out to be as valuable a remedy in yellow fever, as it unquestionably is in pneumonia and typhoid fever, Dr. Norwood will be entitled to the meed of being ranked among the greatest benefactors of mankind.

THE NASHVILLE MONTHLY RECORD OF MEDICAL SCIENCE is a new medical journal which has found its way to our table since our last issue, and we welcome it to our list of exchanges, wishing it the fullest measure of success. We predicted that the union of Drs. Wright and Curry would bring forth a new journal, and while we were calculating the probabilities of the period of incubation, sure enough here it is. Drs. W. & C. are fully able to put forth one of the best journals in the country, and if we are to judge from the present number, we say they will do it. There is one objectionable feature in the journal, however, and in this it is by no means singular. It is utterly impossible to make any journal pay a cent for the labor bestowed on it (even if it pays the printer) at $2 per annum; and as surely as we live, medical editors throughout the land who adopt this cheap plan are thereby doing no good and much injury. Why our medical men should be furnished with periodicals at a price which does not pay for the paper and ink and press work, we cannot understand. We edit a monthy journal of seventy-two pages at $5 per annum, and it is in its fifth volume. If any man thinks it pays, let him come and try it. But while we write, here comes

The Medical Journal of North Carolina, a bi-monthly of ninety-six pages, at $3 per annum, in advance. It is ably edited by Dr. Edward Warren, of Edenton, N. C., and is "published under the auspices of the State Medical Society." We congratulate Dr. Warren on this most excellent first number, and welcome him to the corps editorial He has taken on a galling harness, but if he gets a good list of subscribers he may be half paid for his valuable services at $3 for a bi-monthly of 96 pages.

Professional.—Dr. Jas. B. McCaw, who has long and ably edited the Virginia Medical Journal, has been elected to the chair of Chemistry, in the Medical College of Virginia, lately held by Dr. Martin P. Scott.

Blockley Alms House Again.—Who will not be happy to learn that the notorious McClintock has been turned out of the Blockley Alms House, Philadelphia, and a legitimate physician, Dr. Robt. K. Smith, put in his place. No doubt the worthy Governors of the Alms House found public opinion too strong a current for them to stem. Amen!

A Severe Comment.—The Naval Board lately assembled at Philadelphia, selected but seven out of twenty-seven candidates for the post of Assistant Surgeon in the United States Navy; and the Army Board, which met at Richmond in April last, selected but *two* of twenty-seven candidates for the same post in the Army.

Report of Deaths in the City of New Orleans, from the 22nd of August to the 20th of September, 1858.—Men, 1317; women, 473; boys, 397; girls, 309; males, 1714; females, 782; adults, 1790; children, 706; total, 2496. Under 1 year, 161; 1 to 2 years, 105; 2 to 5 years, 184; 5 to 10 years, 69; 10 to 20 years, 207; 20 to 40 years, 1448; 40 to 60 years, 254; 60 to 80 years, 63; 80 to 100 years, 4; over 100 years, 1.

Principal Diseases.—Yellow fever, 1782; consumption, 56; infantile convulsions, 70; teething, '20; still-born, 41; trismus nacentium, 23; congestive fever, 95.

Nativities.—United States, 849; foreigners, 1318; not stated, 329. Blacks, 62; mulattoes, 25. Of these—free, 34; slaves, 53.

List of Payments to September 23, 1858.—Dr. W. H. Ruffin, vol. 5, $5; A. B. Cabaniss, vol. 5, $5; Mr. W. L. Robinson, vols. 3, 4, 5, $15; Dr. E. Hyde, vol. 5, $5; Mr. G. L. A. Davis, vol. 5, $5; Dr. E. J. Capell, to August, 1859, $5; E. E. Smart, vols. 4, 5, $10; F. W. Barnes, vols, 2, 3, 4, 5, $18; Wood, (N. O.) vols. 4, 5, $10; Ridgeley, (N. O.) vol. 5, $5, J. C. Cummings, vol. 5, $5; Zehender, (N. O.) vol. 4, $5.

With many thanks to the few who have so kindly contributed towards paying the printer, and with assurances that we are still far behind, we beg leave to jog the memories of many delinquents once more. By very slightly disturbing the equilibrium of your purses, gentlemen, you will at once enable us to balance accounts with both yourselves and the printer. This is all we ask. We do not think of your paying us for our share of the labor.

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EXCERPTA.

Observations on Malarious Fever.—*By Professor Joseph Jones, of Georgia.*—Professor Jones has been publishing in the late numbers of the *Southern Medical and Surgical Journal*, a series of interesting observations and analyses of the blood and urine in intermittent and remittent fevers which are worthy of farther notice than we are able to give at the present time. We offer the following extracts illustrative of certain points in pathology and treatment:

"*Case IX.*—October 9th, 8 o'clcok, p. m. Seaman: age 40, height 5 feet 10½ inches, weight 170 lbs. Auburn hair, blue eyes, florid complexion; sanguine temperament; well developed muscular man. Native of Sweden. Has been in New Orleans, Panama, and other southern ports, but has never been sick before.

"This is his first trip to Savannah; has been sleeping on the bay; was taken with chill and fever two days ago, and has suffered much with pain in his head. Yesterday vomited bile. Tongue slightly coated with white fur. Now his skin is in a good perspiration. Twenty minutes ago, his skin felt very hot and dry to the touch. Supposing that his temperature was several degrees above the normal standard, I hastened to the laboratory to obtain my thermometer: I was detained twenty minutes, and when I returned, to my suprise, his skin felt moist and cool under the hand, in comparison with the previous observation.

Pulse	86	Temperature of Atmosphere	72° F.
		" " Hand	100
Respiration	23	" under Tongue	101° 75′

The rise of the themometer in his hand (the hand as usual was kept carefully closed around the bulb.) was irregular. It rose to 100°, and at this point was stationary for several mintues: it then fell gradually to 98°, and again rose gradually to 100°. In this case we see that the reduction of temperature was simultaneous with the relaxation of the skin and flow of perspiration.

"Is the remission of the fever due to the restoration of the functions of the sudoriparous glands, which collectively expose a surface of tubing 1,570,000 inches, or nearly 28 miles in length? Is the morbific agent, or agents, which have disturbed the chemical actions and correlation of the forces eliminated by these glands?

"If the intermission is due to the restoration of the functions of the sudoriparous glands what excited them to action? Is the phenomenon connected with the nervous system alone, or with chemical and physical changes in the structure of the morbific agents, and of the blood and secretion and excretions? If the reduction of temperature be not dependent upon the restoration of the functions of the sudoriparous glands, what retarded the chemical actions by which the physical forces are generated? If the chemical actions developing an unusual amount of heat were excited by the introduction of foreign elements, may not the foreign elements themselves have entered into these chemical actions, and been so altered, that they have been for a time rendered inert?

"That a special end is accomplished, in malarial fever, by an elevation of temperature is proved by the fact that the cases which manifest the highest temperatures, are as a general rule attended with little or no danger, whilst in those cases, as congestive fever, where there is a depression of temperature, the danger is always imminent.

"It is true that the sudoriparous glands have much to do with the regulation of the temperature, for the water which they eliminate from the blood during its evaporation, abstracts one thousand degrees of heat from the surface of the body and the surrounding atmosphere. This heat is expended in the mechanical action of keeping asunder the particles of water, and is hence insensible to the thermometer.

The experiments of Dr. Southwood Smith, (2) at the Phœnix Gas Works, and of MM. Berger, Delaroche, (3) Fordice, Blagden, (4) and others, (5) have shown that when animals and man were subjected to great external degrees of heat, the temperature of the body was regulated by the evaporation from the surface of the skin and lungs. When the air was dry, individuals were able to endure, for a considerable length of time, a temperature of from 250 to 350 degrees, without injurious effects, and without any great elevation of temperature. The loss of water from the surface of the body was correspondingly great, and by its evaporation maintained the temperature of the interior at the normal standard. If, however, this evaporation be interfered with, by saturating the air with aqueous vapor, the temperature rose rapidly, and the individuals died in a short time.

" The rapidity and force of the circulation and respiration also, have much to do with the increase or diminution of temperature in the active stages and in the intermission of malarial fever. This will be established in future by numerous examples.

" *Case XIX.* — English seaman: age 25; weight 160 lbs; height 5 feet 6 inches; black hair; florid complexion; muscular system well developed; powerful chest; short stout neck. Has just returned from Jacksonville, Fla. Has been sick one week.

" August 18th, 11 o'clock, A. M. Skin hot and dry; face very red; pulse 106; respiration thoracic; temperature of atmosphere 88°; temperature of hand 105°. Complains of pain in his head. Urine high colored. Specific gravity 1021. ℞ Bloodletting in the standing posture to produce relaxation of the system.

"2 o'clock, P. M. Preparations were made for blood-letting. The patient appeared to be alarmed by the preparations; and when I appeared with the specific gravity bottles, beaker glasses and capsules, to receive the blood, he fell back into the arms of the assistant, and in a few moments before the lancet was applied, the perspiration stood in large drops upon his face and hands. During the bleeding, he perspired freely, and fainted before f ℥ x, were abstracted. The pulse and respiration were diminished in frequency and force, and the patient fell into a profound slumber, during which his clothes were completely saturated with a profuse perspiration.

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(2) " Philosophy of health," vol. II. pp. 391—396.

(3) " Experiences sur les Effets qu'une forte Chaleur produit sur l'Economie." Paris, 1805, and Journal de Physique, tomes lxxiii., lxxi., et lxxvii.

(4) "Philosphical Transactions." 1775.

(5) Magendie's Experiments upon the influence of Hot air on Animal Life.—Am. Jour. Med. Sciences, January 1845, p. 183.

M. Constantine James, on the effects of the hot moist air of the baths, or stoves, of Nero, at Pozzui, Gazett Medicale, 27th Avril, 1844. W. F. Edwards on Animal Heat. Cyclopedia of Anatomy and Physioloy, vol. ii. pp. 649—684. John Davy, on Animal Temperature. Philosophical Transactions, 1814. Edinburgh Philosophical Journal, Jan. 1826 See also, Researches Physiological and Anatomical, by Dr. John Davy, London, 1839, vol. i. pp. 141—248. Experiments of Tillet and Duhamel, experiments on the servants of a baker at Rochefoucault in Angoumois. Mem. Acad. Scien pour 1764, p. 186, et seq. Experiments of Dobson at Liverpool; Phil. Trans. for 1775, p. 463, et seq. Observations on the effects of High Temperatures, by Bell of Manchester. Manchester Mem. vol. i. p. 1, et seq. Currie on application of Water at different Temperatures. Phil. Trans. for 1792, p. 199, et seq. Experiments of Delaroche, Jour. Phys., t. lxiii. p. 207. Nicholson's Journ., vol. xvii. p. 142, 215. Jour. Phys., t. lxxi. p. 289, and t. lxxvii. 1. Lavoisier on Transpiration, Mem. Acad., pour 1790. John Reid on Respiration, Cycloped. of Anatomy and Physiology, vol. iv. pp. 325.—368.

" *Examination of Blood.*—Blood of a dark, almost black color, commenced to coagulate in about ten minutes. After remaining for two hours exposed to the atmosphere, the surface of the blood exposed, changed to a bright red arterial hue.

" Specific gravity of blood, 1042; specific gravity of serum, 1018. Serum of a light golden color. The color was not so deep as in severer cases of malarial fever.

" 8 o'clock, P. M. Much better; pain in his head has disappeared; in a profuse perspiration; skin much cooler. Pulse 96 to the minute, soft and natural. Respiration regular and normal.

" August 19th, 10 o'clock, P. M. Skin moist; urine high colored; pulse 82; respiration 24. Temperature of atmosphere 89°F. Temperature of hand 100°. Temperature under tongue 102° 25′.

" August 20th, 12 o'clock, M. Skin and tongue normal; urine high colored. Specific gravity, 1015. Pulse 80; respiration 21. Temperature of atmosphere 85°F. Temperature under tongue 101°.

" August 21st. Pulse 80; respiration 20. Temperature of atmosphrere 80°. Temperature under tongue 98° 8′.

" This case yielded readily to the action of sulphate of quinia, and the patient was discharged on the 23d. This has been a mild attack of malarial fever, and the blood corpuscles were but slightly diminished.

" At first sight this case would seem to prove the efficacy of blood letting in malarial fever. The fact that the mere preparations for bleeding caused the patient " to break out into a profuse perspiration," demonstrated that the sweating stage was nigh at hand and the bleeding was the occasion and not the cause of its appearance.

" It is probable that this patient would have done just as well, without, as with this bleeding. The next patient suffering with malarial fever, which I bled, was a stout German, with high fever, scarlet face, and " distracting pain in his head," presenting similar only severer symptoms. This case terminated fatally. I have used local blood-letting (cut cups) in scores of cases of malarial fever, and always with apparent benefit. Over the epigastrium it often arrests obstinate vomiting, and over the temples and back of neck, and over the spine, it often relieves distressing pain. Local differs from general blood-letting, in two essential degrees. First, the quantity of blood abstracted is much less, and second, the number of colored blood corpuscles is less in proportion to the amout of blood abstracted, in local than in general blood letting. The colored blood corpuscles rush along principally in the centre of the streams, and in general blood-letting they are lost more rapidly than the other elements of the blood. The malarial poison, whatever it be, destroys rapidly the colored blood corpuscles. Whatever, therefore, diminishes the colored blood corpuscles acts in concert with the malarial poison. The malarial poison reduces rapidly the forces. General blood-letting reduces rapidly the forces. The two, in this particular, again act in concert These views will be substantiated by facts, and more fully discussed hereafter."

After giving a detail of the symptoms and treatment of an exceedingly interesting case of remittent and intermittent fever in the same individual, he makes the following remarks:

"The occurrence of intermittent fever after an attack of remittent fever, was due in this case to one or both of two causes. The malarial poison may not have been completely broken up and removed during the attack of remittent fever, or a fresh dose of the malarial poiso may have been received subsequently to the complete elimination of the disturbing element.

"Both these causes may have acted simultaneously. The hospital is situated in a malarious district, and a dose of the poison might have been received at any time.

"A comparison of the phenomena of this case of remittent fever with those of health and intermittent fever, establish the following conclusions:

"(1). The secretions of the mouth are more completely checked, and the tongue is dryer, reder and rougher to the feeling in remittent, than in intermittent fever.

"(2). The glowing tongue of remittent fever, is not an index of inflammation. It indicates a want of circulation in the capillaries of the superficial parts of the tongue. The secretions of the mucous membrane have been checked; the moisture is evaporated by the elevated temperature; the circulation in the superficial capillaries, is thus retarded, and they become filled with colored blood corpuscles, which give the bright color to the tongue.

" The question arises, what checked the secretions of the mucous membrane of the mouth?

" The consideration of the chemical alterations of many elements and secretions enter into the solution of this question. We will mention only five and acknowledge that this is nothing but a statement of the manner in which the changes may take place, and not a direct absolute answer to the question.

" 1. The cells of the mucous membrane. 2. The capilliaries of the mucous membrane. 3. The serum of the blood. 4. The blood corpuscles. 5. The nervous system presiding over the elaboration of the blood, the circulation of the blood in the capilliaries, and the formation of the secretions.

" We have before demonstrated that the malarial poison, produces profound chemical and physical alterations in the colored blood corpuscles, destroying many, and in all inducing chemical changes which result in a great dimunition of the fixed saline constituents.

" The blood corpuscles, collectively form an immense gland, which elaborates the materials of the serum into compounds for the nervous and muscular system, and probably for the various secretions. Whatever therefore destroys or alters the chemical changes of the colored blood corpuscles, must produce corresponding changes, in the secretions and excretions, and in the structure and action of the nervous system.

" There is a portion of the nervous system which presides over the circulation and chemical changes in the capilliaries, and thus over the secretions. The malarial poison may act upon the nervous system directly, or indirectly, by the changes induced in the blood corpuscles and the constituents of the serum, or it may act upon the nervous system in both ways combined at the same time. Whatever acts directly or indirectly upon the nervous system, will produce corresponding changes in the secretions, excretions and nutrition.

" 3. In this case we cast aside the advice of many of the older writers and administered the sulphate of quinia freely in the outset of the disease, regardless of the glowing parched tongue, tenderness upon pressure of the epigastrium, and severe headache and high fever, and rapid bounding pulse, thoracic respiration, and hot dry skin. Under the action of the sulphate of quinia, the dry red tongue became moist, clear and pale; the circulation and respiration abated in force and frequency, the dry harsh skin was covered with perspiration, and all the symptoms subsided· The advantage of this mode of treatment will be illustrated subsequently.

" The increase of the action of the pulse and respiration was attended by an elevation of the temperature. The elevation of the temperature corresponded more accurately with the increased action of the circulatory and respiratory systems in intermittent fever, than in this case of remittent fever. That is, the pulse and respiration were more accelerated in remittent fever, whilst the temperature did not rise higher than that of intemittent fever.

" 5. The urea was increased during the active stages of the fever, not only above the normal standard during rest and a deprivation of food, but also above the standard of intermittent fever.

" 6. The increased temperature and correspondingly increased chemical changes, were attended by an increase of the urea. When the temperature fell below the normal standard, in both intermittent and remittent fever, the urea was decreased in amount.

" 7. The uric acid was diminished in the active stages of intermittent and remittent fevers, and increased as the diseases subsided.

" 8. The changes in the acidity of the urine in this case, resembled in all respects, those during intermittent fever.

" 9. The coloring and extractive matters, were diminished during the active stages, and increased during the subsidence of the fever."

CHLORATE OF SODA AS A SUBSTITUTE FOR CHLORATE OF POTASH. M. Guéneau de Mussy states, in the *Revue Médicale*, that, struck by the little solubility of chlorate of potash, he substituted chlorate of soda for it, as the latter salt is much more soluble than the former. The taste of the chlorate of soda is, besides, less disagreeable than that of the other salt, and can also be given in a small quantity of vehicle. M. de Mussy has given the chlorate of soda in several cases of diptheria with uniform success.—*London Lancet.*

NEW ORLEANS

MEDICAL NEWS AND HOSPITAL GAZETTE.

VOL. V. NOVEMBER, 1858. NO. 9.

TWO CASES OF VESICO-VAGINAL FISTULA CURED.

By D. WARREN BRICKELL, M. D., Professor of Obstetrics, New Orleans School of Medicine.

About three years ago, my surprise was excited by the observation of a medical friend, to the effect, that not a case of Vesico Vaginal Fistula had ever been cured in New Orleans—that not a few operations had been undertaken by different surgeons here, but no good results had ever ensued. Considering this rather a stigma on the profession of so large a city, where medicine, in all its branches, should be as well cultivated as in any other portion of the world, and more especially since the operation of Sims had been known to the world, I determined to apply myself to the subject. The first thing was to get all the necessary instruments, and these I procured from Tiemann & Co., of New York. Then I must get a case on which to operate.

In a private Hospital in this city, I was allowed to operate on a young negro woman, from Red River, who was considered incurable. On examination, I found a very large fistula indeed—so large, that when the patient would lie on her back, the inverted bladder would protrude through the vulva. This was an ugly case for a beginner, but the subject was otherwise healthy, and I determined to try it. Suffice it to say, that at various times I operated, by the methods of both Sims and Bozeman; that is, I used the silver wire, (claimed as entirely original by Sims,) with and without the clamps, and I used the wire, together with Bozeman's "button." I failed signally—for causes that I will hereafter mention. I then tried the operation for closure of the vulva, and in this I failed too. The patient was now carried to Dr. Bozeman, of Montgomery, Alabama, and I see that he has since reported her *cured*. It gives me the greatest pleasure to say here, that for curing this case, Dr. Bozeman deserves the greatest credit, and no one could more willingly award him all praise than I now do. That it was a bad case, there can be no doubt, and when I abandoned her, I thought her incurable; but with the experience I now have, I believe I could cure as bad a case—though I have since seen one which has proved too much for me, the whole vesico-vaginal septum being wanting.

My second case was an Irishwoman of this city, with a fistula fully an inch and a quarter long, (across the septum,) and when exposed to view, nearly three quarters of an inch wide, and very high up. I operated on this woman three times, at intervals of about six weeks to three months, the sutures on the upper edge of the fistula passing through the posterior lip of the uterus, and at the end of six weeks after the third operation, the fistula was only the size of a crow quill. Of course, I expected to cure this case, and in order to insure this end, I determined to wait two or three months, and let the parts become firm and solid. At the end of four months, I called to operate, and, to my surprise, found that she was more than three months advanced in pregnancy. She was averse to having anything done now, as she was anxious to have a living child, (both she and her previous child having been butchered by a so-called medical man); but, as there was extensive cicatrization of the vagina, independent of the seat of operation, I advised premature delivery. All advice was useless, however, and she went to full term, and had a fine living child, but with the deplorable result of bursting the cicatrix in the seat of operation. A few weeks after delivery, I examined her, and found the fistula nearly as large as when I first undertook the case. She promised to submit to operation again as soon as her babe was old enough to spare her, and I still hope to cure her. Of the three operations performed on her, one was with the wire and clamps of Sims, one with the wire alone, and one with the wire in conjunction with Bozeman's button. The clamp operation effected little, and the other two operations appeared to be followed by equally beneficial results. My object was to test all the best modes recommended.

First Case Cured.—On Christmas day, 1857, I received a very fine looking negro woman from Mr. Yeamans, of Vicksburg, Mississippi, said to be laboring under Vesico-Vaginal Fistula. If I am not mistaken, my friend, Dr. Harper, of Vicksburg, had seen her, and recommended Mr. Y. to send her to me.

I will not detain the reader with the minutiæ of her previous history. Suffice it to say, that she was a stout, athletic woman, in good general health, the mother of nine or ten children, and about thirty-five years of age. In her last confinement, she had been attended only by an ignorant negro midwife, (like the midwives in general, the curses of the communities in which they are found,) and, from long detention of the head of the child in the pelvic cavity, sloughing of the vesico-vaginal septum had taken place, leaving a permanent fistula, now about three quarters of an inch long, and seated transversely across the septum, about midway between the neck of the bladder and the uterus.

On the 30th December, 1857, I operated with the wire suture alone, (I mean always, tying the wire with the shot of Sims); no disagreeable symptoms followed; urine flowed through the catheter only; and on the ninth day, I removed the sutures, finding an apparently perfect cicatrix in place of the fistula. Of course, I used the catheter for several days, to draw off the water, not wishing to allow either distension of the bladder, or to tax it with expelling its contents. Notwithstanding this, however, the very first time an accumulation of water took place, and she endeavored to expel it herself, rupture of the cicatrix was the unfortunute result. Reflection on the subject, and subsequent experience, however, have satisfied me that I did not denude the edges of the fistula sufficiently, and that my cicatrix was too frail.

As soon as the parts were all firmly healed and in good condition, I operated again, this time using Bozeman's button, with the wire and shot. On the ninth day, I removed the sutures, and perfect union had taken place. This time, I had denuded the parts extensively. The woman is still perfectly well.

Case Second Cured.—Early in January, 1858, Mrs. Mary M., native of Ireland, came to me from St. Louis. I found her in my ward in the Charity Hospital one morning. She said that about a year before she had been delivered of twins, the labor resulting in the establishment of vesico-vaginal fistula. Early in November, 1857, she was carried by her husband to the Hospital in St. Louis, and there she remained during the months of November and December. Previous to entering the Hospital, one of the most prominent surgeons of St. Louis had operated on her once, and after entering, the same surgeon had operated three times, but with no beneficial result; on the contrary, she thought the "hole was much larger than ever." While in the Hospital, she encountered a woman who had been an inmate of the Charity Hospital, New Orleans, and who told her that I could cure her. On this vague information, she sold her feather bed (for her husband had abandoned her and left her penniless) for nine dollars, and took a steamboat for this city.

When I encountered her in my ward, she was one of the most miserable looking objects imaginable. She was not exceedingly emaciated, but she was pale and dejected, without hope and without courage—the urine all the time pouring down her limbs, and the parts excoriated. I examined her, and found a fistula one inch and a quarter, by measurement, across the vesico-vaginal septum, and, as the patient assumed the position on the hands and knees, three quarters of an inch in width—a huge hole into the bladder, and through which the inverted bladder rolled through the vulva, so as at all times to be seen by merely separating the limbs a little. Indeed, it was only by passing a sound through the urethra, and with it pushing the bladder upwards, (thus preventing inversion,) that the fistula could be properly defined.

As unpromising as things appeared, I cheered her with assurances that every effort would be made to cure her—that, if she would be patient, I would never give her up as incurable, inasmuch as worse cases were reported as having been cured. She was put on good diet, furnished a good bed, etc., etc., and on the 16th of the same month was ready for operation.

I denuded the edges of the fistula extensively, (the consequence of which was very free hemorrhage,) and then passed firm sutures through, freely embracing the posterior lip of the uterus, (the fistula being pretty high up.) Bozeman's button was then fitted on, and fastened down with the leaden shot, as usual. She bled but little after the operation was completed, the water ran by the catheter, and when, on the tenth day, the sutures were removed, perfect union had taken place, though at one end of the seat of operation the cicatrix appeared rather frail. It was a very small spot, however, and I hoped that it would grow stronger as contraction took place. For several days the catheter was used, and it was not until the fifteenth day that she was allowed to get out of bed, or to pass water naturally. She improved daily, but one morning when I visited my ward, I found her dejected again, and she told me that she was again "leaking a little." On inquiry, I found that a little dispute had arisen between her and the nurse, a scuffle ensued, and the nurse pushed her down violently, the consequence of which was immediate leakage of urine from the vagina. On examination, I discovered a small hole, about an eighth of an inch in diameter, and on the left extremity of the cicatrix. I put her to bed at once, placing her on her right side, and introducing a permanent catheter into the bladder—still hoping that the parts would heal firmly. In this, however, I was disappointed. The result was the establishment of a small fistula, from which there was constant slight leakage, though it was too small to allow the urine to flow from the bladder as fast as secreted, and the consequence was that she would pass considerable quantities of urine *per vias naturales*, and ten or twelve times during the twenty-four hours—the bladder being, of course, still quite irritated.

About this time, I found myself encountering another obstacle. Mine was a *medical* ward, and this was a *surgical* case. Under the technicalities of the Hospital, I could not operate again. But my patient had come all the way from St. Louis to be operated on by me especially; no other medical man in the city was making any effort to cure such cases; all efforts previously made here had failed; she declared no one else should touch her; and I determined not to abandon her on any terms. She was too poor to hire either apartments or a nurse, so I took her to my residence and placed her in comfortable quarters. On the 18th of March, I operated on her again. I denuded the edges of the little fistula freely, thereby making the opening considerably larger, and passed through two silver wire sutures, fastening them with the perforated shot. On the ninth day, I removed these sutures, and union was complete. I now write on the 22d of September. Day before yesterday, my patient was in my office to see me. She is as well as she ever was in her life—she is well in spite of every obstacle, real and factitious, that has been cast in her way. She is not only well, but she is making her own living, and may some day assist in erecting a Hospital which shall know no limits to the encouragement it gives to every medical man who aspires to the exaltation of his profession above the standard of a mere mode of gaining a living.

Observations.—In the treatment of case No. one, I labored under great disadvantages. First, I had no experience in the matter of operating, as well as in the management of such cases after operation—which I find to be quite as important as the proper performance of the operation itself. The patient, herself, was anything but obedient, (on several occasions, I found her out of bed within three days after operations), and she was so far removed from me, that I could not see her oftener than twice in twenty-four hours. To Dr. Bickham, Resident Surgeon of the Hospital, I am deeply indebted for his ready assistance, both during and after operations, but, like myself, he was inexperienced in the management of such cases. After my operation, my great trouble was to keep the catheter clear, and in every instance, I now believe, that my operation failed because the urine accumulated in the bladder—the catheter having been plugged up by the phosphatic deposit which was abundantly poured out of the bladder. Had I just such a case in my own yard now, I believe I could cure it, but I would not undertake it under anything like as disadvantageous circumstances as I have related. He who undertakes to cure a case of Vesico-Vaginal Fistula, must not deceive himself with the idea that the *operation* is the great point. To be sure, this must be properly performed, but I care not how well it is done, it will fail if the subsequent treatment is not properly conducted. And herein consists the actual labor in these cases; labor, too, of the most disagreeable kind imaginable.

I confidently anticipate curing the second case mentioned, if she returns to me. She is a fine, healthy woman, and, with my present experience, I believe I can relieve her with half the labor I have already expended on her. I was bitterly disappointed in the ultimate result in her case. I could scarcely have imagined that one in her situation would have subjected herself to the chance of becoming pregnant. But there is no accounting for tastes.

Case third—the first cured—was certainly the most obedient patient I ever met. She never for a moment deviated from the rules laid down, and I am satisfied that the cure was much facilitated on this account.

Case four was, also, very patient, but never exhibited the same utter submission to rules that case three did; consequently, I had a great deal more trouble with her. I mention these little matters for the benefit of those who may undertake the operation. It is certainly a most important point to instill the mind of the patient with the importance of utter submission to directions. I have never experienced any professional labor so disagreeable and unsatisfactory as that bestowed on an obstreperous patient of this kind.

Mode of Operation.—Just at this time, it would seem almost like walking over new incrustations consequent on volcanic eruption, to institute a comparison between the more popular operations of the day; the surface is still pretty hot, and wo be unto him who happens to break through! However, I have nothing original to claim, and may, therefore, be pardoned for expressing my honest convictions in relation to the proposed plans of others. I think I have tried the "clamp" of Sims and the "button" of Bozeman sufficiently often, and have watched their operation sufficiently closely, to be able to judge of their value. The clamp, I do not think I shall ever try again, because I agree with its author, that it is not essential to the cure, and unless a great deal of skill is exercised in its application, it is dangerous. In a few words, I believe the "clamp" will soon be obsolete. The "button" I have applied with a great deal of care, and I have tried to convince myself that its *modus operandi* is what its author claims for it; but I am obliged to say that I cannot concede to it anything like the influence claimed. For one simple reason, I shall always use it, or something like it—viz.: the plate interposed between the tissues and the shot used for tying the sutures, prevents the shot from becoming imbedded, and sometimes even encysted, in the tissues, which I have found rather a troublesome complication. That the concave plate or button exercises any influence in keeping the lips of the wound in apposition, I confess I am at a loss to conceive; my conception of the physical laws here involved does not lead me to award such an influence. The wires alone exert the power of adapting the denuded surfaces, the shot tie the wires most efficiently and simply, and the "button" keeps the shot from being imbedded in the tissues. As to the salutary influence claimed for the button, in keeping the secretions of the vagina from coming in contact with the wound, I cannot attach any importance whatever to that; the only substance that need give the operator any anxiety in this respect, is the urine, which is in direct contact with the inner edges of the wound. If the lips of the wound are well adapted, infiltration of urine is prevented; then what cause is there for anxiety about the passage of the vaginal secretion opposite? More than this: the button itself covers a certain portion of the vaginal surface, which portion is exhaling the same matter, and this matter is absolutely held by the button in contact with the wound. In a few words, I am satisfied that to the silver wire used we chiefly owe our success in these operations, though I am not prepared to say that *silver* wire alone will answer the purpose. On the contrary, the success of Mettauer, of Virginia, proves conclusively that the leaden suture is quite reliable. With my present experience, I would not like to exchange a good silver wire for a leaden one; I would prefer the perforated shot to simply twisting the suture, and I would use the silver plate or "button" to prevent the shot from becoming imbedded in the tissues. I would thus combine the operations of Sims and Bozeman, regarding the latter as a very useful modification of the former.

Position.—Position, both during and after the operation, is a matter of great importance. I prefer the position on the hands and knees during the operation, because I have found that a more complete exposure of the seat of operation can be obtained in this way than in any other. I do not believe, however, in the idea of Dr. Sims, that the vagina is opened by *atmospheric pressure.* There are many cases in which no view can be had of the os uteri in this position, without great assistance from the speculum, and I have seen no case wherein the operation could be performed without the speculum.

After the operation, I have found the position on the side the best that can be assumed by the patient, and, with efficient assistance, the patient can change from one side to the other without endangering the success of the operation. As the patient lies on the side, the attendant can more readily attend to cleansing and replacing the catheter, etc., etc.

Although I have been obliged to contrive things to meet the exigencies of my cases, I claim no originality. I have a bed with a hole in the mattress, under which is placed a chamber vessel. I use an ordinary female cathether, with a piece of India-rubber tubing, about a foot long, attached to it with adhesive strips; the urine flows through the cathether into the tube, and thence into the vessel under the bed—thus effectually protecting the person of the patient from contact with the urine, and keeping the bedding perfectly clean. I have faithfully tried Sims's catheter, but cannot get it to answer half so well as the ordinary female catheter, with a few extra holes drilled into the sides. This latter has a twine passed through its eyes, and this is made fast to another string passed around the hips of the patient. With this arrangement, the *inner* end of the catheter can be elevated or depressed at will, and the catheter can be made to penetrate the bladder to any desired depth.

Phosphatic Deposits.—I have been greatly annoyed by the phosphatic deposits in all my cases, but latterly found great relief from the free use of Benzoic Acid. This I have given internally, and have injected it into the bladder, and with very beneficial results. Sometimes it has entirely corrected the tendency to deposition of the posphates; at other times, only partially.

Denudation and Passage of Sutures.—I have found that any other than free denudation of the lips of the fistula, is unreliable, and I do not spare the tissues where there is plenty at disposal. As to the passage of the sutures, instead of taking care to avoid penetration of the mucous membrane of the bladder, as earnestly advised by some writers, I take especial pains to pass the needle clear through. I thus know that I have a firm hold, and have yet to see the slightest evil result. On one occasion, when the operation had failed to effect union of the edges of the fistula, I allowed several sutures to remain penetrating the entire walls of the bladder, and not the least irritation occurred. All observation has taught me that the silver wire is harmless.

I have thus given a cursory history of the first case of this disease ever cured in New Orleans. While I was operating on my second case, Dr. Schuppert, of this city, operated on and cured one, which was the second ever cured here, and my St. Louis case is the third. A few years ago, one of our most respectable surgeons reported a case in this Journal, but the fistula was reëstablished a short time afterwards. I am deeply indebted to many of my medical friends, both graduates and undergraduates, for their valuable assistance in my operations. They have worked hard with me, and often have I seen skepticism and disappointment on their faces when I would report failures. But we have succeeded at last, our path is now comparatively clear, and I hope to call on them again.

EXTRACTION IN CATARACT, WITH REMARKS AND CASES.

By C. Beard, M. D., Visiting Physician, Charity Hospital.

It is useless to recapitulate the hackneyed arguments in favor of the operation of extraction in cataract. Surgeons of the present day, who operate often upon the eye, are almost unanimous in their preference of extraction; and I am inclined to think that the time is not remote, when this operation, in skillful hands, will supercede all others—whether the opaque lens be hard or semi-hard, soft or fluid, capsular, or simply lenticular.

Up to within a few years, extraction has been resorted to as a rule in those cases only where the opaque body was supposed to present a certain degree of consistency; in other words, in hard cataract; the soft variety, with or without central nuclei, was abandoned to the needle. Græfe, the Berlin oculist, has within the present decade revived the operation of linear extraction, and applied it to the removal of all cases of soft cataract, and with such success, that it bids fair to do in such cases what the flap operation has done in the hard cataract, and as it does not offer the same objections to the use of chloroform which we find in the flap operation, it is applicable to the case of children or timid persons.

Thus we have brought to bear in the treatment of cataract, two operative methods, which fulfill every possible requisite if properly performed, and are adapted to every case; which rid the patient of his malady more rapidly, more completely, and with less danger to the organ than any other, and which are to the surgeon infinitely more satisfactory than any operation which can be performed by the needle.

There exists a morbid antipathy on the part of some individuals who operate on the eye, to opening the cornea. They use a needle with indifference, puncture the more readily inflammable sclerotic, push onwards through the ciliary ligament perhaps, wound the ciliary processes, transfix the lens or iris, and yet recoil from making a clean cut in the cornea, a tissue possessing but little sensibility, and in which union takes place readily and rapidly. Is it, perhaps, because a blunder is more easily taken note of in one case than the other? If this is not the reason, I know of no other. The vitreous body in a healthy eye is not readily forced through the pupil by pressure, and there is but little danger of loss of this substance—the great bug-bear of the operation—by extraction. Any one can be convinced of this by removing the whole of the cornea of a recent and healthy eye, pressing on the organ, and testing the resistance offered by the iris, even when relaxed by death, to the exit of the lens and vitreous body. It will be observed that a considerable degree of pressure is requisite to effect the expulsion of either, much more than is ever brought to bear upon the globe by the most exaggerated spasmodic muscular action, consequent upon extraction. The iris in the living, during an extraction, contracting under the stimulus of light, and as the result of the flow of the aqueous humor, offers a still greater resistance to the passage through the pupil of any thing situated behind it. I doubt much if any part of the vitreous humor is ever lost in extraction, where the eye is healthy, and the surgeon at all careful and skillful.

Finally, my experience has certainly convinced me, that the wound in the cornea, necessary in extraction, gives rise, as a general rule, to much less inflammation than that of the sclerotic in puncture with the needle, and that the pain, not only during the operation, but in the after treatment, is incomparably less in the extraction than in the sclerotonixis.

In the cases of extraction reported below, I cite one in which there was uncomplicated, hard lenticular cataract; one in which there existed hard lenticular cataract, with an opaline fluid in the capsule and possibly capsular opacity; one in which the cataract was almost entirely of the soft variety; one in which a considerable hernia of the iris followed the exit of the lens, and in which there were a number of contra indication to the operation chosen; and lastly, one in which softening of the vitreous body existed, with loss of more than one half of this substance during the operation, in which almost perfect vision was restored, and no tendency to atrophy resulted.

Each of these cases, a type in itself, speaks volumes in favor of the operation of corneal section.

Case I.—S. F., an old citizen of this city, aged sixty-three years, was first affected with cataract in the right eye about twelve years since ; finding a few years later that the other was becoming defective, he decided upon having an operation performed on the first. The person who operated used the needle. It is impossible to say whether or not the operation was well done, but the eye finally suppurated after the patient had suffered severe inflammation for nearly a year. The progress of the second cataract was rapid, until it finally completely extinguished useful vision. Mr. F., fearful of a renewal of suffering with an other operation, chose to remain blind for nearly seven years. He was finally persuaded to submit to an operation on the left eye, on the 19th of June last.

On examination, I found the eye to all appearances healthy, with the exception of the lenticular opacity—a condition of things more rare than is usually supposed. The patient was bold and firm, and had good control over the movements of the eye, which submitted to be touched with the handle of an instrument or the fingers, without the spasmodic nervous movements, usual in timorous subjects. Diagnosis hard lenticular cataract.

Mr. F.'s general health was good and his habits regular, but being somewhat plethoric, it was deemed prudent to administer a few laxatives a day or two previous to the operation. His pupil was also dilated with atropine.

On the 19th of June, assisted by Dr. Choppin of this city, I extracted by the superior corneal section. The patient being placed in a sitting posture in front of me. No accident happened during the operation; the corneal section was almost completed without the escape of any aqueous fluid, and the lens, after rupture of its capsule, was easily extracted. The eye-lids were brought together with the Liston plaster, and the patient was removed carefully to his bed. Iced applications were assiduously made for five or six days, by means of small compresses, over which was sprinkled a strong solution of atropine. A little pain and redness, with tumefaction of the edges of the eye-lids, was observed about the seventh day. This yielded to the use of a few leeches applied to the external angle of the lids, with frictions of mercurial ointment and belladonna to the orbit.

The eye was opened on the tenth day; the pupil looked healthy; the corneal flap had united, with the exception of a very small point at the inner and upper part; aqueous fluid continued to exude through this for several days, the globe remaining softer than usual in consequence. Complete union did not take place until about the fifteenth day, a circumstance I attributed to the patient having been placed on too low a diet. With an improvement in this, the eye mended rapidly, and about the twenty fifth day it could be used freely without inconvenience in a moderately lighted apartment.

The present condition of the eye is very satisfactory. The corneal section is completely hidden by a well worked *arcus senilis*, in which the cicatrix is lost—the pupil is moveable and perfectly black, and with the exception of a small piece of capsule which is observable at its outer and under portion, is faultless. The cornea is of the proper curve, and the anterior chamber normal; vision good at all distances with appropriate glasses.

Case II.—Mrs. L., a lady of this city, aged about sixty-two years; delicate; of a nervous temperament, but of general good health; has had cataract of the right eye for several years; useful vision abolished entirely. In left eye cataract almost complete; sees large objects in a darkened room indistinctly. Diagnosis hard lenticular cataract both eyes.

Mrs. L. has been subject to a spasmodic affection of the right eye for some time, a species of temporary internal and superior strabismus, which is not remarked by the patient herself. There is no other visible condition of the globe worthy of notice. Although nervous, Mrs. L. was taught by a little handling of the eye, to keep it very steady under the touch of the fingers or an instrument. On the 11th of August, it was decided to operate by extraction, superior corneal section. I performed this operation, assisted by Dr. Grall, of this city, the patient being in a recumbent posture; I standing at her head. There was no difficulty during the operation, and the lens was easily extracted.

At the moment of rupturing the capsule with the needle, an opaline fluid exuded in quantity sufficient to give a milky hue to the iris, over which it spread. The lens itself was a type of a hard cataract, amber grey in color, and the hardest I think I ever saw. It was also of the usual size and shape. The fluid had evidently been contained within the capsule. What was it? A softening to solution of the cortical layer of the lens? Then this body should have been smaller than usual, which it was not. Was it an opalescence of the liquor Morgagni? Græfe says this fluid is mythical—does not exist. I am yet at a loss to explain it. At the time I opened the capsule I was unable to say whether it was opaque or not, in consequence of the whitish effusion from it; I endeavored, therefore, during the second period of the operation, to break it up as completely as possible, in order that its remains might the more readily be removed by absorbtion.

This case progressed well under a treatment similar to that of Mr F.'s—applications of cold and the constant use of atropine. There was some inflammation about the tenth day, which subsided after some trouble· The patient could use the eye in the darkened room, about the twentieth day after the operation; tried glasses some ten days later. In this case also, the cornea was marked by a well defined *arcus senilis,* which not only shows itself at the upper portion of the cornea as is usual, but completely encircles it. In this the cicatrix is concealed. The present condition of the eye is as follows: The cornea and anterior chamber are well formed and of the natural appearance; iris unaltered in color; the pupil is very slightly displaced to the nasal side, and a little irregular; it contains at its outer part a remnant of the capsule, which diminishes in size from week to week; much the larger portion of the pupil is entirely black and natural.

The patient reads ordinary type fluently, and saw the comet a few nights ago very plainly.

The other eye will be operated on in the course of the next week.

Case III.—Mrs. G., a lady of this city, aged seventy-two years, of robust health, (weighs about 300 pounds) has had cataract for a number of years; was operated upon right eye some years ago with the needle. Result: destruction of globe. The left eye presented a hard lenticular cataract; amber grey color; patient almost totally deprived of light, in consequence of the completeness of the opacity. The globe appeared otherwise healthy; the movements of the iris were somewhat impaired, probably from diminished retinial impression. Mrs. G. was also affected with a species of nystagmus, or uneasy rolling about of the eye in its orbit; the movement always upward and outwards, and simultaneous with a touch upon the lid, or excited by a request to direct the organ in any particular direction. I failed in all my attempts to teach her by practice to keep the globe steady, and concluded that I had found at last a case in which one of the innumerable ophthalmostats must be brought into play, to render extraction possible. Of these, I tried two, Jager's and Nelaton's, previous to preparing her for operation, but without success in keeping the globe in a proper position, and finally concluded that I should have to make use of the ordinary toothed forceps, in order to effect my purpose.

After having administered several purgatives during the three preceding days, I operated on the 9th of September by extraction, the inferior section of the cornea being made. The difficulties which presented themselves in this case will be readily understood, and would have deterred me from attempting an extraction had I not long since conceived such an unconquerable repugnance to the needle operation in hard cataract. My patient's eye was unsteady, and required one assistant and two forceps to keep it even moderately quiet. From her immense size it was impracticable to operate with the right hand in the sitting posture. I could not, thus placed, have reached her head, moreover, it would have been impossible to have kept the eye steady in such a position. Lying down and standing behind her, the patient could have been operated upon, but I should have been forced to use my left hand, and the only available light in the room would have been obscured by a bed-post. The superior corneal section, therefore, could not be conveniently made, and I had recourse to the inferior. The patient was laid across a wide bed, a window being to her left side. My assistant stood behind her head, raised the upper lid with the index and median fingers of the left hand, while with the toothed forceps in his right, he dragged the cornea downwards and inwards. I, partly sitting, partly lying, by the side of the patient, assisted in fixing the globe with another forceps fastened into the conjunctiva just below the margin of the cornea, and with a very short and broad cataract knife, completed the inferior section as rapidly as consistent with safety. The lens was removed before the forceps left their hold upon the conjunctiva and consequently the lids were not closed at the termination of the first period of the operation as is advised.

The patient bore the operation, which was much more than usually painful, without flinching. The eye was closed with adhesive straps, and the ice and atropine applied. No unfavorable symptom showed itself during the subsequent treatment; a small portion of the iris protruded through the wound in the cornea, as it is prone to do in the inferior flap operation. This, however, has only had the effect of rendering the pupil oblong, and has in no wise, injured or impaired vision. The patient has excellent sight; can see to read with the ordinary biconvex glass, and recognizes acquaintances at a distance with the periscopic. The cornea in this instance, is a little projecting at its inferior part; the anterior chamber somewhat deformed in consequence. None of these defects have, however, rendered vision less perfect than it would have been had they not existed.

Case IV.—Aggy, slave, aged about 60 years, robust and healthy, entered my Infirmary on the 6th of August, with hard lenticular cataract, right eye. I had operated on this patient some five weeks before for soft cataract of the left eye, the needle having been used to break it up.

The right eye presented a tolerably healthy appearance, the pupil being moveable, perception of light good. The iris, however, was somewhat unsteady, floating as it were, to and fro in the anterior chamber, a sign characteristic of softened vitreous humor. As the result of the needle operation, the patient had suffered a prolonged and painful inflammation, notwithstanding that the case had appeared most favorable for the operation chosen, and no untoward event took place during its performance. I determined in the right eye to venture extraction, although I was aware there was some risk. On the 2d of September, assisted by Dr. Grall, I operated on the right eye, the patient reclining on a sofa, I sitting behind her.

The superior section was accomplished without difficulty, and the lids closed as is usual previous to the second period of the operation. While waiting a few seconds for the patient to recover from the shock, as is advised, I noticed a glairy looking fluid slowly exuding from the external canthus, which, on examination, proved to be vitreous humor softened to a syrupy consistence. Of this, a considerable quantity flowed down the cheek before I had decided whether or not to proceed with the second period of the operation, or that of rupturing the capsule. To undertake this under the circumstances, was to risk the evacuation of the globe, a most unpleasant termination to an operation for extraction. It is advised in such cases, to close the lids, permit union of the flap to take place, and have recourse to a different operation at some future time, but I concluded to proceed with the extraction. The lids were again separated, the needle introduced to the capsule, which being ruptured, gave passage to the opaque lens. During the whole of this time, the vitreous humor was slowly flowing from the globe, which already had the appearance of a squeezed orange. The lids were closed with the isinglass plaster, and the patient removed to her bed some distance off. As she laid down, more of the fluid in question escaped from the internal canthus, and we came to the conclusion that the eye was pretty well emptied. Iced applications were made constantly for some five days, during which time, the globe gradually recovered its form and natural tension: On the twelfth day the eye was opened and examined; to my surprise, union had taken place, and every thing looked healthy, while perception of light, was perfect.

To-day, six weeks since the extraction, Aggy can see to recognize faces, notices the colors and figures of a pocket handkerchief, the numbers on the watch-dial. And this, nothwithstanding more than *one-half* of the vitreous body was lost during the operation. The eye presents to the touch the natural tension, there is no softness, no shrinking, or anything like atrophy. It will be remembered that authors mention these pathological changes as constant sequelæ of a loss of vitreous humor to the extent of even one-third. The glass Aggy finds best adapted to her eye is a number two and a half, not by any means as convex as is sometimes used after the operation. This circumstance would tend to show that the refractive properties of the vitreous body play an insignificant rôle in the performance of the phenomena of vision.

Case V.—Mrs. M., of Galveston, Texas. entered my Infirmary on the 12th of last September, has been blind twelve years in the right eye, only six months in the left, although useful vision has been lost much longer.

On examination, the patient presented two lenticular opacities. In the right eye, the cataract was semi-hard, in the left quite soft throughout its cortical portion, but presenting apparently a very small hard nucleus; capsules transparent in each case. Mrs. M. is delicate; of a nervous temperament, but quite firm and determined. The eye submits very kindly to the touch of the fingers or knife; no spasmodic movements whatever. I determined to extract in the right eye. Mrs. M. required no constitutional preparation. On the 14th September, I operated by extraction on the right eye; patient reclining on a bed; I standing behind and operating with right hand; owing to a sudden movement of the globe, counter puncture was not made properly, and a very small portion of the iris was excised with the knife in its passage outwards. The lens was extracted without difficulty; there was no hæmorrhage from the minute wound in the iris. The eye was closed and the usual treatment made use of. All went on well until the eighth day when she complaned of violent pain. There was also some tumefaction and redness of the edges of the lids; both of these symptoms disappeared after the application of a few leeches, but the pain returned again at intervals, notwithstanding the further use of leeches, morphia and belladonna. It finally subsided after five or six accesses in as many days. On the fifteenth day the eye was thoroughly examined and found to be in a perfectly satisfactory condition. To-day, five weeks after the operation. the patient sees every thing perfectly with her glasses; the pupil is nearly round and perfect in its action, and the cornea and anterior chamber normal in size and appearance.

I operated on the left eye by extraction on the sixth of the present month; no accident occurred. Much the larger portion of the cataract was as soft as it could be without being fluid; there was a very small and moderately hard nucleus. No inflammatory symptoms manifested themselves in this instance. On the tenth day, as I write, the corneal section has entirely healed; the anterior chamber is perfect; the iris and pupil natural and vision distinct.

WARNING TO INVALIDS VISITING THE ISLAND OF CUBA.

[We beg leave to present our readers the following extract from a letter received from an esteemed correspondent in Havana. It speaks for itself, and all medical men who send patients to Cuba for relief, should be careful to make every effort to secure them from the imposition pointed out.—Eds.]

If you like to take the trouble to give notice to your readers, that such of their patients and other invalids as come to Havana shall be counseled in respect to localities best suited to their conditions, or if so ill as to require private lodgings, or residences separate from the crowd and discomfort of hotels, it will give me much satisfaction to be of service to them in any way that may be in my power, and to carry out every judicious plan of treatment that may be recommended by their physicians.

I do not wish to be understood as advertising myself, or as soliciting cases, but the custom with infirmaries and several physicians, of employing runners and of paying commissions to catch strangers, renders it suitable that the profession be advised of the indecent usage and its objects; and the more so, that the parties guarantee quick and radical cures of diseases in the most desperate circumstances, stipulating beforehand the amount of money to be paid, usually from one to five thousand dollars, and the largest part in advance that can possibly be obtained. The patient is always required, under the pretext of air, diet, or something else, to take lodgings in some family in which the physician confides, and the members of which, and also their friends, are instructed in the part they are to act in the conspiracy. At the proper time, an interpreter is at hand; at other times, the patient is surrounded by persons he cannot understand or make understand him: the thieves soon size his pile, and proceed in the process of fleecing their victim, whose effects in the end fall short of paying his bills; the flattering arrangement has cost the unfortunate patient his last dollar, with no result whatever but a heartless robbery, and his chagrin and bitter disappointment. Instances of this kind of financiering occur every season, and it happens, also, if the funds of the victim do not hold out to pay every dollar, and the regular course of the disease, or some other cause, has ended in the death of the patient, his acquaintances and countrymen must provide for his interment, or see his remains conveyed in the common dead-cart to the cemetery, and deposited without coffin or any covering in the ditch or common receptacle of the dead from hospitals, prisons, and places of execution. Strangers who are invalids, should consult their Consul, or some countryman of established character, in respect to whatever they propose to do, particularly in respect to medical attendants, and all matters involving important interests, with which they themselves are not perfectly familiar. There is no need of any person falling into the hands of medical thieves and robbers. In Havana, there are great numbers of professional gentlemen, whose spotless honor and high attainments would lose nothing by any test or comparison, and whom money could not induce to guarantee the cure of a disease already advanced to the point which makes it necessarily mortal; but these do not employ runners or whippers-in, or divide their fees in commissions.

The credulity of invalids is notorious. Men of the best capacity, the highest attainments and stations, heads of the law and church, allow themselves to fall victims to the most grovelling impostures, and dupes of the vilest quackery. The best understandings are betrayed by credulity and fear, or importuned out of their senses by designing rogues, officious spectators, as well as by honest-meaning friends. If we consider how intensely life and health engage the hopes and fears of all, the blindest judgment in her vain groping for something to ward off despair, and her idiotic trust in ignorance and knavery ought rather to be matter of regret and commisseration, than of surprise and derision; and allowance should be made for the effects of sickness, for impatience from sufferings unrelieved, perhaps unrelievable by any skill.

REMEDY FOR DISMENORRHŒA, ETC.

By W. O. Barker, of Omega, Texas.

For the New Orleans Medical News and Hospital Gazette.—Under the caption, "A valuable remedy for Dysmenorrhœa and consequent sterility," I find a recipe and directions for its use. I have substituted the remedy since July for one which I have been using for ten years, with the same result—entire success.

From the nature and cause of the disease, I was induced to believe that it partook strongly of the nature of rheumatism. I therefore prepared the following:

℞ Gum. Guiac..............................℥i.
Pottassæ Nit..............................ʒi.
Flor. Sulphur..............................ʒi. M.

All to be well ground in a mortar and put into one pint of brandy or good whisky, and, after standing a few days, I directed it to be taken in tablespoonful doses morning and night, commencing one week before the menstrual period. A perfect cure has been the result of each trial with the above remedy. In a very few cases the remedy had to be continued.

In cases of retention or suppression of the catamenia, when the subject was otherwise healthy, I have used the following with good success:

℞ Ol. Terebinth..............................℥i.
Ol. Sassafras..............................℥ss.
Spt. Vini. Rect. (Alcohol)..............℥viii. M.
To be given in some simple tea in doses of f ʒi. *pro re nata.*

I am satisfied that the recipe given by Dr. Fenner in the July Number, above referred to, is all that is claimed for it, yet I can see no reason for withholding remedies equally efficacious.

MEMOIR ON SOME POINTS OF THE NORMAL AND PATHOLOGICAL ANATOMY OF THE MUCOUS MEMBRANE OF THE UTERUS.

By DOCTOR CHARLES ROBIN, Professor Agregé of the Faculty of Paris, Member of the Academy of Medicine, etc.

Written expressly for the New Orleans Medical News and Hospital Gazette, and translated by ANTHONY PENISTON, M. D., Professor of Physiology, New Orleans School of Medicine.

PART SECOND.—*Continued from the August Number.*

Of the modifications of the Uterus during Pregnancy.

The epithelium which lines the cavity of the neck of the uterus retains the cylindrical form during the whole period of gestation; nevertheless, it looses, on most of the cells, the ciliary processes with which they are lined. But such is not the case with the body of the uterus. Having had an opportunity to examine several pregnant uteri, at two and a half, three, five, and six months, I have seen that this epithelium gradually change from the prismatic cylindrical form to the pavement variety.

There is no proof that the prismatic cells are flattened out into the pavement form; but every thing goes to show that, shortly after fecundation, the epithelium lining the cavity of the uterus exfoliates, either cell after cell, or in small patches, and it is afterwards replaced by the pavement epithelium, having cells from twelve to fourteen millimetres in width, of a polyhedral shape, lying in contiguity to èach other. These cells have a spherical ovoid nucleus, of about the size of a red globule of blood. This cell is finely granular, and generally contains no nucleolus The substance of the cell is almost entirely filled with dark, yellow granulations, regularly disposed around the nucleus. In fact, many of these cells contain no nucleus, and are filled with yellow, fatty granulations. The cells which are thus constructed, are often detached and found floating in the mucus on the surface of the uterine cavity; these are generally spherical, larger and more granular than those which are attached and which have the pavement form.

This form of cell is most usually found from the sixth week to the second month, both on the decidua vera, and the reflexa. But, even at this early period, we find, here and there, irregular patches which are entirely denuded of their epithelium. From the second month, the regular cells which we have described are replaced by others which are much larger and longer; these new cells are thin, pale and flattened; they measure from four to nine hundredths of a millimetre. Their form is irregular, and they are generally variously pointed at different angles. The nucleus is larger than in the preceding cells, and it presents an ovoid, finely granular appearance. The cells themselves are but slightly granular, and the fat granulations which they contain are separated from each other. These elongated cells gradually increase in number, and preponderate over the others near the end of pregnancy. At the same time, the denuded portions increase in number and extent; so that, on the free surface of the decidua uterina, very little epithelium is found at all. During the progress of pregnancy, we have also found that the same cells which, at the second month, had no nucleolus, now have one or two which are yellow, with a brilliant centre, and a clear, dark outline; the nucleus itself becomes much larger in proportion. In the vicinity of the internal orifice of the uterus, at the eighth month of pregnancy, the decidua reflexa is not adherent to that portion of the mucous membrane, or decidua uterina, which approaches to within three centimetres of the os internum. Cells similar to those which we have described are found in this portion of the uterus. Some are regularly polyhedral, the others are somewhat elongated, and they have all one or two ovoid nuclei, measuring from twelve to fifteen thousandths of a millimetre in length. These cells generally have one or two brilliant nuclei, of a yellowish appearance; they also contain fine granular matter, both in the cell and in the nucleus; the latter, however, contains fewer of these granulations. In this region, a few cells still retain their prismatic shape, especially towards the free extremity which is cut at right angles, but the adherent extremity is generally sound, and somewhat larger than the rest of the cell. Here also we generally find that the cells are accompanied with many free nuclei and some very finely granular amorphous matter. At this period of pregnancy, the Fallopian tubes have a nucleolar epithelium, with ovoid nuclei, though sometimes spherical, and united together by a small quantity of amorphous matter. This epithelium is generally accompanied with oval or prismatic cells, having no regular shape, though generally containing a nucleus similar to the free nuclei above mentioned.

Section 2. Of the principal changes presented by the Epithelial Cells of the Uterus which correspond to the Decidua Inter-utero Placental.

The epithelial cells which are comprised or situated between the growing placenta and the highly vascular mucous membrane which becomes deciduous, constituting what is called the decidua inter-utero placental, undergo a series of changes which are exceedingly interesting and instructive. The metamorphoses which these cells undergo, teach us the limits between which certain anatomical elements may vary, according to the normal or pathological condition in which they may be placed, and from these now under consideration, we may form an estimate of their transformations in some tumors and diseases of this organ. The same changes also take place in the cells which line the decidua uterina and reflexta, and which are therefore placed between those two membranes, so that when the ovum increases in size, they are brought in immediate juxtaposition, and finally become soldered together.

We have thus an explanation of the fact that epithelial cells are found in the substance of the decidua, and yet this membrane, when examined after delivery, appears simple and homogeneous, so complete is the fusion between the two folds of which it is constituted. So that the anatomical disposition of the parts at once explains a fact which would otherwise appear a pathological circumstance. The epithelium which is found in the regions I have just described, is partly composed of free nuclei, and of entire cells, the latter being by far the most numerous. When the specimen is recently separated, they can be examined in the shape of thin lamelli, generally lying in juxtaposition to each other. The cells themselves are similar to those described in the preceding section, though rather less granular. Some of these cells are hypertrophied and double the normal size, yet they still retain the prismatic shape, with the rounded or the narrow extremity of this variety of epithelium. Some become distinctly polyhedral. In almost all, the nucleus is found to have increased in proportion with the cell, and contains one or two nuclei, with a brilliant centre and dark outline. But at the same time, we find in their vicinity other cells aggregated together in thin lamellæ, and which are even more hypertrophied and deformed than the former. Instead of their normal size, which is from two to three hundredths of a millimetre in length by eight thousandths of a millimetre in width, they reach the size of one-tenth of a millimetre in length, and could be seen with the naked eye, if they were wider and less transparent. Between these extremes all the intermediate sizes may be found. They rarely exceed in thickness from eight to welve thousandths of a millimetre. The shape of these cells is exceedingly diversified, according as the hypertrophied condition has taken place in a single direction or on every side. In the latter case, the cells become almost spherical, but these are comparatively rare, and they do not exceed from two to three hundredths of a millimetre in size. The most of the cells which have become altered in shape and size, are generally elongated at both extremities, but irregularly truncated; very few being symetrically pointed. At times this prolongation takes places only on one side, the other still remaining round or polyhedral. The nucleus is generally contained in the widest part of the cell, so that the shape of the latter is generally dependent upon the contained nucleus, being either fusiform or club-shaped according as the nucleus is situated in the middle or at one extremity; sometimes the cells have various processes on different sides, especially from the angles of the cells which are more distinctly polyhedral, so that the cells assume various strange shapes, and not commonly found among epithelial cells. The nature of these transformations would be certainly unknown, if they had not been studied in their various stages of development. Some of the hypertrophied cells contain two or three nuclei, though not frequently. There is generally only one nucleus, but of large size, with a clear, shining centre, slightly granular, with a distinct outline. The nucleus is generally of an ovoid shape, as in the normal state, but greatly hypertrophied. Its size varies from 12 to 18 thousandths of a millimetre, by 5 or 6 thousandths in width. Each nucleus contains one or two nucleoli, having from one to two thousandths of a millimetre in width. These nucleoli have a brilliant dark centre, with clear and distinct outlines. The free nuclei which we described above are similar to those of which we are now speaking. They are manifestly similar or analogous to those which have been described and delineated under the name of cancerous nuclei, and would doubtless be classed with those pathological productions by any one who had not observed their successive stages of development. But this condition which is here normal may be a morbid product in other parts of the body.

Therefore it is that a complete description of the histological elements of any portion of the body, must not only include the disposition of those parts in adult life, but also their different phases of development, both normal and pathological. I must also observe that some of the cells thus changed and hypertrophied remain finely granular, as in the normal state. They are then very pale and transparent, but many other cells are filled with fatty granulations, with a brilliant yellow centre and deep outline, as we often see in tumors and epithelial cells pathologically enlarged. These granulations are generally more numerous around the nucleus, or at its extremities, than throughout the remainder of the cell; but wherever they are more thickly accumulated, the cells are more opaque, and contrast with the other cells, which are highly transparent.

SECTION 3. Of the principal changes presented in some Mammiferous Animals by the Epithelial Cells of the Uterus between the Placenta and the Allantois.

During gestation the epithelial cells of the uterus present, in both domestic and wild animals, successive changes, which are analogous to those which I have described in woman.

In the sow the bifurcations of the uterus contain epithelial cells which vary at different periods of pregnancy. In the intervals between the places occupied by each egg, the cells maintain their prismatic shape, and their nuclei, which is more generally round than oval, and which has no nucleus; but none of them retain the vibrating cilia. In those places where the allantois is applied against the mucous membrane of the uterus, the epithelial cells belonging to both of these tissues become hypertrophied, at the same time that they assume the pavement form. It is most interesting to follow the different modifications in the transition from the prismatic shape to the pavement variety of epithelium, as we gradually reach those parts of mucous membrane which are in contact with an ovum. For the cells can be seen to be gradually shorter, and thicker at the same time. They do not immediately become granular, but their nucleus gradually assumes the ovoid shape, and receives a nucleolus which is generally proportionate to the nucleus. In those places where the mucous membrane is in immediate contact with the ovum, the cells become polyhedral, but with rounded angles and curved sides, and they generally have from two to three hundredths of a millimetre in diameter. But many become spherical or oval as soon as they are separated from each other and become free. Some of these contain from two to three nuclei, with a brilliant, yellow nucleolus. With these cells are mixed some free nuclei, similar to those which are found in the cells themselves.

Epithelial cells, such as we have described, are either of the prismatic or pavement variety, and form, in the regions which they occupy, a sheath to the capillary vessels of the mucous coat. The remains of these epithelial sheaths are often found in the mucoid secretions of the uterus, or in the greyish coat interposed between the mucous membrane and the allantois. These sheaths are easily distinguished from the coverings of the uterine follicles, for the epithelial coat of the latter is nucleolar, with ovoid nuclei, somewhat larger than the blood corpuscles of the adult, and somewhat separated from each other by a quantity of amorphous matter, with exceedingly fine granulations. In the rabbit and the Guinea pig, those epithelial cells undergo changes even more interesting than in the pig: for even the cells of the mucous membrane which are not in contact with the placenta go through certain changes which are different from those of the epithelium contained in the inter-utero-placental membrane.

1st. When we examine those positions of uterine mucous membrane situated between the ovi, almost all the cells retain their prismatic shape, though they are generally twice or three times their former size. They vary but little in length. A few cells, which reach three or four times their former size, assume a cubic or polyhedral shape. The juxtaposition of these cells forms epithelial scales of beautiful texture when examined under the microscope, especially the highly vascular sheaths which cover the villi in this region. These sheaths retaining the shape of the villi themselves, can be easily recognized, and afford an excellent opportunity of studying this variety of epithelium. Each cell, whether hypertrophied or not, contains several nuclei. The position of these nuclei varies in the different cells, having at times a crucial form, at others being disposed regularly like beads on a string, and at other times having no symetrical arrangement whatsoever. The appearance of these cells is indeed quite remarkable, and different from its usual aspect when not under the influence of gestation.

The grayish, soft substance which forms the inter-utero-placental membrane, undergoes changes which are different from those which I have just described. There, the cells are both hypertrophied and altered in shape, somewhat like the cells of the decidua of the human species, described above. But they are even more changed in proportion, so that they do not at all resemble the cells of the empty uterus : they are all of the pavement variety. Besides these, we find here and there isolated cells, which are much smaller than the preceding variety, and retain the shape of the epithelium which is found in the intervals between the ovi, and contain several nuclei. It is especially on the border of the placenta that the grayish, intermediate coat contains cells similar to those which are found in the intervals between the ovi. Between those cells which attain their greatest hypertrophied size, almost all the intermediate shapes can be found, without any regular disposition. The largest cells are flattened, and contain many nuclei, some having as many as sixteen and eighteen nuclei, which vary in shape from round to oval. Some of these nuclei vary from six to eighteen thousandths of a millimetre, the usual size being about twelve thousandths of a millimetre in length. There is generally no nucleolus. The cell itself is usually granular around the nucleus, and derives from this cause a grayish aspect. In fact, many cells appear quite opaque, from the large number of fatty granulations which they contain. This fatty granular matter is sometimes accumulated around the nucleus, or scattered indiscriminately, and is more abundant than in the human species.

In the Guinea pig, we find facts analogous to the above; but the surface of the placenta, and especially that thick, gluey coat on its surface, contains many cells which are regularly spherical or oval, among others which are polyhedral or irregular. These cells vary in size, and contain several nuclei which are remarkable by their large size.

We have thus been enabled to see that no histological study can be complete unless it comprehends a description of the tissues, of the embryo and the adult, together with their transformations in pathological conditions. Nor can any deductions be drawn in morbid cases unless the normal conditions be perfectly ascertained. I have therefore directed my attention as much to the study of the pathological anatomy of the fœtus, as to its normal condition, and the numerous practical applications which we find of this knowledge are among the most satisfactory results of modern investigation.

From this kind of study we have already derived much more certainty in our knowledge of the relation between the functional derangement of an organ and the nature of that lesion. In many of the diseases belonging to the domain of internal pathology, which are characterized by an apparent disproportion between the intensity of the symptoms, and the extent of the lesion observable after death, there the alteration of the different liquids will be found to afford a more satisfactory explanation of the nature of the disease: for a change in the nature of a secretion leads us at once to suspect a disorganization of the secreting organ. On the other hand, an intimate knowledge of the minute anatomy of the different parenchymatous structures can alone enlighten us on the nature and progress of the various lesions to which they are liable.

When we pass to those numerous morbid products which belong to the class of tumors, and are in the domain of surgery, there again we must have a complete knowledge of the elements, both in fœtal and adult life; and this is especially necessary for an accurate investigation into the nature of the various glandular enlargements, and their departure from the normal state. All these investigations into microscopical anatomy are replete with interest, and promise the surest guide for the future advancement of medical science.

SPECIAL SELECTIONS.

THE CONDITION OF THE BLOOD IN YELLOW FEVER.

By Dr. D. Blair, Surgeon-General of British Guiana.

[We continue our extract from the last paper of the lamented Dr. Blair (*Brit. and For. Medico-Chirurg. Rev., Jan.*, 1856). Having in our last number given his observations *on the condition of the urine*, we offer now his researches into the *condition of the blood*, which will doubtless be read with interest. It has long been suspected that the blood is the principal seat of the disease in yellow fever, but no one has investigated this point so fully as Dr. Blair. Let us see what he says about it ?—E. D. F.]

"There were many opportunities for becoming acquainted with the condition of the blood during this epidemic. Cupping, the use of artificial and natural leech, arteriotomy, in a few cases venesection, epistaxis, and other hæmorrhages gave ample opportunity during life for examining its physical, chemical, and microscopic qualities. In no instance could we discover any really abnormal condition of color, corpuscles, serum, and crassamentum during the first stage, except sometimes the presence of bile. In Mr. Dod's case before mentioned, in consequence of the intense congestion of his face, I opened the temporal artery on the second day of his illness. The blood (three ounces) was florid and coagulated well. The serum alkaline. A little nitric acid dropped into a small portion of it caused instant coagulation, at first opaque white, but after a few minutes a bright yellow, with a *ring of purple.* Bile had passed off freely, and until the fifth day, that of his death, decided yellow suffusion could not be observed. The artery burst open several times before his death, and could be with difficulty restrained, but the blood itself showed no abnormal appearance, except that the bile-test became more decisive up to the time of his death. In every instance in the first stage, the blood retained its normal alkalinity. The changes in the blood therefore were found only in the last stages, and in the post-mortem blood. And yet, cases terminating fatally after normal black vomit and hæmorrhages, as in Flynn (Seaman's Hospital, 14th of December, 1852), G. Ball (Seaman's Hospital, 13th of November, 1852), John Knowles (Seaman's Hospital, 13th of November, 1852), H. Stewart, (Seaman's Hospital, 16th of February, 1853,) are numerous, in which no unhealthy appearance of blood after death could be noticed, except as to the bile-tinge. The following are samples of the entries in my note-book on this subject:

"'*Blood in Yellow Fever.*—Francis Mitchel, to-day, in Seaman's Hospital, had free epistaxis. The blood was florid, and formed a good clot. Under Ross' 1-8th inch object glass, found the discs normal. This was his second day of admission to hospital, 5th of December, 1852. . . . *Blood on second day of Yellow Fever.*—To-day I examined the blood of Mitchel, of the *Lucy* (Seaman's Hospital). The appearance of the blood was healthy, so also under the microscope. The *rouleaus* stood almost perpendicular. The blood was obtained by cupping the neck; 7th November, 1852. . . . *Blood on the fourth day of Yellow Fever*—(*After black vomit*).—To-day I had a few drops of blood drawn by an artificial leech from the nates of Charles Mitchel. The blood was rich and florid to the eye, and under the microscope was filled with apparently healthy corpuscles. So numerous were they that they much impeded the transmission of light, and for the better observing of them, the thinnest portion of the blood film on the glass had to be selected. They stood up erect in rouleaus; 9th of November, 1852. . . . . *Blood in Yellow Fever after Death.*—In the case of Charles Mitchel, before referred to, there were two considerably sized fibrinous coagula in the heart. On examining the blood taken from the heart, by the microscope, the corpuscles seemed perfectly healthy and numerous. They arranged themselves here and there in rouleaus. The blood was neutral to the test-paper.'

"The appearance of yellow fibrinous coagula in the heart was frequent after the worst cases, and in fact what may be termed the texture of the blood often remained good. The following is my note on the case of Michael Flynn:

"'*Blood in Yellow Fever.*—To-day, three hours after death of Michael Flynn, I had an ounce-and-half phial four-fifths filled with blood from his heart. Two hours afterwards I examined the phial, and found the crassamentum so firm as to be unmoved by inverting the phial. On its surface was a bright crimson pellicle, concave, with the limits of the concavity extending about one-fifth of an inch up the side of the phial and covered with serum to the same depth The serum was slightly alkaline. On forcing a piquette down through the crassamentum, I obtained a particle of blood for the microscope. The corpuscles were found flat and dark in their centre to half their radii, as if from collapse of the centre. Although one or two seemed ruptured, the rest of the discs were perfect in outline. On examining the phial three hours afterwards, at 8, P. M., I found that the clot and serum had still further separated, each occupying about half of the space of the phial. The serum was a little turbid, and the crassamentum apparently softer. On examining microscopically now by the light of an Argand lamp, transmitted through a 'bull's eye,' scarcely any of the discs had central darkness—neither were any ruptured cells visible. But about half the number of corpuscles had lost their perfection of outline, and were jagged by nucleoli on their edges, and the number of these seemed to increase during the act of observation. At the side of the thin glass, at which a little salted water had been applied, the corpuscles arranged themselves in rouleaus; in another specimen, however, they were numerous, without the use of salted water. In this specimen some imperfect or burst cells were seen. The serum, when heated, coagulated firmly and gave off no ammonia, with or without liquor potassæ. I saw nothing in this blood decidedly abnormal either in color or physical qualities, or minute organism: at any rate, nothing to countenance the idea that the blood was 'dissolved' or even seriously injured by the progress of the disease; and in this aspect of the case—the result of these observations—I have been considerably disappointed; for the passive (?) hæmorrhages before death led me to expect a serious change in the circulating fluid; 14th December, 1852. . . . . *Death in Yellow Fever.—Blood normal* in color and consistency, in the case of John Knowles, Seaman's Hospital. I examined his blood to-day, immediately after death. The heart was gorged. The clot was firm and in due proportion in the test-tube when it cooled. The corpuscles were not to be distinguished from those in perfect health; 13th November, 1852. . . . . *Blood of George Ball.*—Two hours after death still warm. A clot of yellow fibrine, about half-an-ounce in weight, found in heart. The serum of a deep yellow bile-color. The blood clotted firmly. There was an abundance of albumen. The corpuscles normal, except a little bossed or convex in centre, as if the cells were distended; blood slightly alkaline—did not yield ammonia by heat alone, and not much on the addition of liquor potassæ; although comatose before death, his breath was strongly alkaline.'" Gibney (Seaman's Hospital, 21st September, 1852) may be cited as another instance of healthy fibrine in fatal yellow fever.'

"There is, therefore, no doubt of the fact that the blood in the first stage of yellow fever has no appearance of being unhealthy, except as to its occasional intermixture with bile; and also that in many cases which terminate fatally, and with previous black vomit, the blood is found norma in all its appreciable qualities, except as to the before-mentioned intermixture. But in order to arrive at this conclusion, the blood specimen must be procured direct from the proper containing vessels. If instead of examining the blood of epistaxis in yellow fever, we take as our specimen that which has been discharged from the rectum, or by hæmatemesis, when the acid elimination has been scant and the blood little acted on, a very different condition will be found, as was observed in the cases of Jackson (Seaman's Hospital, 21st of April, 1852), Macnamara (Seaman's Hospital, 4th of November, 1852), and Racy (Seaman's Hospital, 7th of November, 1852). The following memoranda will serve to describe the condition of the blood in all these cases:

"'*Examination of Blood of Hæmatemesis of Yellow Fever.*—To-day Macnamara, in Seaman's Hospital, vomited about four ounces of mere blood. It was a thin, dark fluid, without fibrine—was strongly alkaline—in appearance like port wine. He had had creosote and soda prescribed for irritability of stomach, but none had been given before the blood was ejected. He had passed urine moderately (three ounces) about the same time that he vomited. It was acid, coagulable, with a few tube casts. His breath was alkaline. The blood was not brightened by treating it with nitrate of potash and muriate of soda. On gently heating a specimen in a glass tube, without any addition, it gave out ammonia freely. The heated blood showed scarcely as large a proportion of albumen as the urine had done. Three hours after the blood was vomited, I examined it microscopically. There was not a single perfect corpuscle found, and very little débris. The fluid was nearly colorless under the microscope. There were numerous vibriones of various sizes; some one-third the diameter of a blood corpuscle in breadth, and twice as long as the diameter of a blood corpuscle; others not half this size. They were equally broad throughout their entire length, and their motions were vermicular. There were also other little moving translucent bodies, circular or globular, about a quarter the size of a blood corpuscle. For the sake of comparison, I at the same time examined some blood of epistaxis which I had brought home from the Seaman's Hospital, and put aside in a phial a week before. When the cork was removed a slight explosion ensued, and the smell was offensive. It was so thick, however, as to paint the side of the phial when revolved, and of a deep bright red. When a drop of this was examined under the microscope, no perfect corpuscle was seen, but a large quantity evidently of cell-walls, and the hæmatosine was of a deep tint. There were no animalcules in it. 4th of November, 1852.'

"Blood passed off by stool, though unmixed with, and of good crassitude and color to the naked eye, is always found under the microscope with all its corpuscles ruptured. In this intestinal blood, I have never detected vibriones. It seems clear that the alteration observable in the blood from the stomach and intestines, is due in great measure to chemical changes which occur after its extravasation. But though the blood within its proper vessels is often found healthy, through the whole course of the disease up to the last moment of life, it is not always so; and in the last stages is frequently found injured in its obvious constituents of fibrine and cells. As illustrative cases of the deterioration of the fibrine element, reference may be made to those of Morgan and Laman (Seaman's Hospital, 21st of September, 1852), James Walker (Seaman's Hospital, 28th of November, 1852), and Antonio Fernandez (Colonial Hospital, March, 1853). In Morgan's case the fibrine was so diminished in quantity as almost to be lost. In Laman it seemed to have, in a great measure, lost its power of fibrillation. In Walker both fibrine and cells suffered. The right side of the heart was full of dark thin blood, without clots or fibrinous coagula. The color of the blood was of a dirty brown, and entirely fluid; the corpuscles were all altered and misshapen. In all cases, however, the albuminous element seemed, by the rough test of its becoming solid by heat to coagulation point, to be sufficient. A kind of medium deterioration occurred in the case of John Savage (Seaman's Hospital, 19th of November, 1852). My first observation on his case was as follows:

"'*Blood in Yellow Fever.*—To-day John Savage (second day of illness) was cupped on nape of neck. The blood was of a bright vermilion color, with good clot. The half of the number of corpuscles, however, were rough and jagged, apparently from the adhesion of nucleoli, or the splitting of cells. A few were evidently ruptured and torn. About one-half were normal. Perhaps the heat and spirit vapor in the operation had something to do with these appearances—14th of November.'

"The next note is as follows:

"'*Blood After Death in Yellow Fever.*—I had an ounce of blood from Savage's heart about four hours after death. There was a fair proportion of clot, but it was soft, though well separated. The serum looked very thin, and on revolving the blood in the phial, the sides were scarcely stained. But as to the clot, the whole looked like port wine and water. A drop taken from the bottom of the phial, on being examined by the microscope, was found to contain corpuscles, but not numerous. A few were injured, but the vast majority were normal. There were none having the appearance before imputed to the heat and spirit vapor. When the serum was heated, it all set into a firm coagulum, almost dry. It was only slightly alkaline, and gave off no ammonia when heated. Although the kidneys, as usual, were much gorged, their function had been but little impaired; thirty ounces of urine having been found in his bladder, though none had been passed for twelve hours before death'

"But during life, also, the blood is sometimes found altered. Thus, in Jackson, whose case has been already referred to, after black vomit was established, on the 20th April, though the blood from epistaxis was florid and the corpuscles were numerous, they were misshapen, and showed no tendency to form rouleaus. But a few were still normal. Next day, when hæmatemesis succeeded, black vomit and slight epistaxis returned, the corpuscles were still more altered,—they became angular, and elongated, with scarcely one normal cell. In the case of John Bridges, admitted to the Seamen's Hospital 9th November, 1852, a drop of blood taken by the artificial leech showed the oorpuscles spread over the field of view like a pavement. They all seemed flat, and jammed against each other, so that there was scarcely any current or movement amongst them. There was not a single corpuscle of normal appearance; there were no rouleaus, but it was evident that the cells were entire. They speedily became rough by escaped nucleoli. On applying a little salted water to the edge of the glass, currents were immediately induced, and the corpuscles became normal and plump in appearance; but in about a minute they all burst, and the field showed nothing but cell-walls. He had had black vomit before admission. His tongue was denuded of epithelium. He had had no treatment, and described his illness as only of two days duration. In Peter M'Quin's case (who died early epileptic, 11th November, 1852), the corpuscles were flat, indistinct, and irregular in shape, with many nucleoli adhering; but on adding salted water, they bristled with nucleoli like mulberries. Manuel Fernandez was admitted to the Colonial Hospital on the 6th of March with yellow fever. He had been perfectly *blanched* by previous attacks of intermittent fever. His tongue showed no capillary irritation, his face was pale, and his case at first was erroneously diagnosed, judgment having been biased by the previous history of the patient. He was treated, therefore, in the beginning, for an intermittent attack. After death, there was scarcely any yellowness of the skin. There was no *bloodiness* of the integuments in making the sectio, nor of the tissues of any viscus but the kidneys and stomach. Every other part was anæmic. The *liver*, though recorded as 'blood-congested,' from its deep purplish-red color, was not bloody when cut into. The urine found in the bladder was highly coagulable, though that passed during life was not so. The blood was *highly* ammoniacal, though not described emphatically so in the report, and *was totally dissolved.* In those specimens which I took away, and examined both by natural and artificial light, I failed to detect a single normal corpuscle. When the blood was examined, no decomposition in the body had taken place (seventeen hours after death); the *rigor mortis* was beginning to yield. The blood was like port wine in color and consistency. In one specimen under the object glass, two or three almost invisible attenuated corpuscles crossed the field of view, but none of any description in the other specimens, and not even the trace of a cell-wall was to be found. Could this total dissolution of the blood have been possible at any instant before death? Was it the joint effect of the intermittent fever, malaria, and yellow fever poison? Was it the solvent power of ammonia? *The healthy condition of the blood in yellow fever seems associated with free action of the kidneys, or copious black vomit, and alkaline exhalations of the breath. And the deterioration of the fibrine has an obvious relation to the amount of* free *ammonia remaining in the circulation.* The changes in the *shape* of the corpuscles are probably due to alterations in the density and saline constituents of the serum. The blood of the cadaver in this epidemic, was in the vast majority of cases more or less ammoniacal. In the case of Antonio Fernandez, the water used was *soapy* to the feel till the fibrine was washed out. This soapiness was noticed by Dr. Shier, who called my attention to the fact, before he was aware of our former observations on the ammoniacal alkalinity of the blood in these cases. In a few cases, however, the blood was acid, as in that of Roberts (Seamen's Hospital, 2nd of November, 1852); but it was rarely that ammonia was not extricated in any case by the addition of lime. *Bíle constantly*, ammonia almost constantly, and some undetermined acid occasionally, were the only foreign substances which we were able to detect in the yellow fever blood.* Of course, others may have been, and were, likely, present. In order to ascertain some of the proportions of the normal constituents of the blood, I requested Dr. Shier to undertake a chemical examination of some specimens in the Colonial laboratory. He readily consented, and devoted every week-day from the 21st of March to the 19th of April, to the subject. With the exception of one day, I was present the whole time. The following were the results of the laboratory operations in the post-mortem blood, after following, as far as was practicable, the modes of procedure recommended in Bowman's 'Medical Chemistry,' fourth section, on Blood. [We must omit these analyses.]

" The first important fact which was incidentally observed in conducting the experiments, was the rapidity with which the blood decomposed after being taken from the vessels of the cadaver. It set rapidly, and within half an hour all the healthy physiological changes occurred which were to be expected, and soon after decomposition commences. Frequently no separation at all occurred in the coagulum. The blood was taken from Antonio Fernandez and Maria de Jesus in the afternoon, put into beakers, the ground edges of which were greased, and a plate of glass closely applied over each. These beakers were then put into an ice-box till morning. Next day, the blood of the female was apparently uncoagulated and somewhat fetid, and in the evening was so offensive that it had to be thrown away. Immediately on the blood being removed from the body on the previous day, it became firm, as well in the beaker as in the specific gravity bottle. Decomposition, afterwards, no doubt, caused its fluidity and attenuation. In the case of Antonio Fernandez, the clot was large, without buffy coat, with slightly cupped surface, and imperfectly separated from the serum, which was of a dark reddish yellow color. The odor of the blood was then slightly fetid. The serum was removed by a pipette from two specimens, one to determine the proportion of *albumen*, and the other the proportion of water in the serum. To verify former observa-

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[* This observation would appear to substantiate an opinion we have long entertained, viz: that Yellow Fever is, perhaps, the *most bilious* of all the so-called "Bilious Fevers"; and that the arrest of the functions of the liver, causing the retention of the biliary elements in the blood, is the chief cause of the deterioration of this fluid. The suppression of urine soon follows suppression of bile, and we thus have direct sources of blood-poisoning as fatal in their effects as the prime originating cause of the disease.—E. D. F.]

tions, 31.605 grains were taken for the application of the bile test. But the serum was not perfectly clear, and our inability to obtain the requisite quantity necessarily caused a deviation from the plan prescribed by Bowman for the analysis of coagulated blood. The following note occurs in our journal of the 9th of April:

" 'It may be necessary to record, that in estimating the *albumen*, *salts*, and *extractions* of Antonio Fernandez's blood, we could not follow the plan of Bowman in his section on the quantitative analysis of coagulated blood, from the smallness of the quantity of serum obtainable. The albumen was therefore obtained by the method recommended by the same author for obtaining it in diseased blood. The washings are being treated with ether for the fat. But as hydrochloric acid was added to neutralize the alkalinity of the serum, of course no true estimate could be made of the *salts* from that sample. The salts of the serum were therefore estimated from the dry matter obtained in estimating the *water* of the serum. In addition to an estimate of the fat of the serum, we have now under treatment a sample of *dry blood* of Fernandez, to ascertain its proportion of *fat*. We shall proceed afterwards to estimate also the saline constituents of the clot. In reference to the estimate of the *water* in the clot of Fernandez, it may be remarked, that after being weighed, it was dried during several days on a chloride of calcium bath, an uniform density of the fluid being maintained by a tube returning the condensed vapor. While drying, the clot was carefully and repeatedly broken up into small fragments, to facilitate the escape of vapor, and the drying continued till it ceased to lose weight. It was not minutely pulverized, however, and again dried, as it probably should have been before being finally weighed. No instruction to that effect being given by Bowman, and having ourselves made no analyses of healthy blood, whereby a standard might be obtained, we deemed it prudent in this instance not to exceed our instructions, lest comparative results might be affected by a want of parallelism in the methods of procedure.'

" The present mode of defibrinating the blood is coarse and unsatisfactory, and some new or more chemical method is desirable. While, however, the present methods are pursued, and may be continued for comparative results, the degree of fineness of the muslin bag in which the blood is washed should be stated. In that employed by us in defibrinating the blood of Fernandez, the tissue contained two hundred threads in the superficial square inch. Richardson's blood was attempted to be defibrinated by agitation with pieces of lead; but finding this mode inoperative, and that very little fibrine had attached itself to the lead, we completed the operation by subjecting the blood to a gentle stream of pure rain water in a *calico* bag, which, when wet, was almost air-tight. I subjected the washings in both instances to microscopic examination, after they had remained about sixteen hours to settle. They were then fetid. In Fernandez's, I detected numerous fibrillæ and granules among the wreck of the blood-cells, and numerous oval and bottle-shaped polygastric animalcules, which moved with great velocity, and were each about four times the size of a blood corpuscle. There were also bodies in the sediment like quartz-shaped uric acid crystals, but on treating them with nitric acid and ammonia, they did not prove to be such. Some of these bodies seemed black to the naked eye, and bluish-black by transmitted light, but by reflected light, they showed the same quartz-like lustre and appearance. These, likely, had been stained by the dye of a black or dark-blue string, with which the mouth of the muslin bag had been tied. Richardson's blood had coagulated like currant jelly, and the serum had never separated. It was so ammoniacal, that fumes were detected by the muriatic acid rod at the temperature of the atmosphere, without being heated. The washings of his blood yielded considerable sediment. This sediment had a yellowish, curdy, flaky appearance to the naked eye, exactly like the "curdy" urinary sediment which I had observed, and is described before. The sediment showed no fibrillæ, but had a granular appearance, as if the fibrillæ had been disintegrated, or their granular structure disconnected, which may be the mode of the destruction of fibrine by yellow fever. If so, the amount of fibrine estimated in yellow fever blood by the usual methods, must be considered as not the whole fibrinous matter contained in the blood, but only the whole which has escaped disintegration, or which retains its power of fibrillating, and thereby capable of being collected by the present methods. There were some singularities in the case of Antonio Fernandez. He had suffered from an attack of pleuritus, extending over about three inches of the left side, coming on during the progress of the yellow fever, and occasioned probably by the situation of his bed, which was in nearly a thorough draught. He also had the scarlatinoid rash more markedly and more extensively than I ever saw before, and it was accompanied by a turgescence of the skin, which subsided on the advent of black vomit. As the black vomit was not very copious, and lasted only one day, and although he lost a considerable quantity of blood by urine, as he had suffered from an inflammatory complication, I naturally expected to find his blood rich in fibrine, and but little deficient in its saline constituents. It may be here noticed, that in obtaining the specimens for examination from the large vessels of the chest, care was taken that no fluid from the pleura, mediastinum, or pericardium, should be mixed with the blood. Some effusion had taken place as a consequence of pleuritis in the case of Fernandez. In washing out the fat from the albumen by boiling ether, the filters on the second day refused to act, probably from an obstruction of their pores, and steam-washing was had recourse to during two successive days, until no trace of organic or saline matter passed through, the criterion of which was the stainless evaporation of a drop of the filtered fluid on a piece of glass heated over the spirit lamp. It may be remarked, that this criterion would not be applicable to filter-washing with *commercial* ether, which leaves a stain of itself, from probably an impure spirit (containing fusil oil) being frequently used in its manufacture. Without attention to this fact, the ether filtration may go on to an indefinite period—for the stain on the glass will be well marked, and will *volatilize before being charred*, leading to the inference that the fat is not yet completely separated from the albumen, which may or may not be correct. The alcohol used for the *extraction* was of the strength of 42° of Baumé, and for washings were applied.

"On the 13th of April, an estimate of the *iron* in the inorganic matter (ash) of the *dry blood* of Fernandez, which had been incinerated the previous day, was commenced. As this analysis was independent of the instructions contained in Bowman's 'Medical Chemistry,' it will be right to describe the method pursued: To the ash was added an excess of muriatic acid, and then it was digested in a bath till solution was effected, and a few drops of nitric acid were added to peroxidize the iron. It was afterwards filtered, to free it from some particles of impurity, and treated with ammonia for precipitation, and allowed to remain till next morning. The ammoniacal precipitate of iron was then steam-washed till only a trace of stain could be detected on evaporating a drop of the filtered liquid on a piece of glass. The filter (No. 5) and its contents were then dried in another filter paper, so as to be removed into an agate mortar, when it was mixed with an excess of *pure* carbonate of soda. The filter paper was then ignited, and placed in a counterpoised platina crucible, and further incinerated over a spirit lamp, and then mixed with an excess of *pure* carbonate of soda, and added to the other portion so treated. The whole was then fused, and after fusion, lixivated with distilled water, and left till next day, On the following day, after maceration, filtration, and steam-washing on filter paper of known weight, the precipitate and paper were dried, and weighed in a counterpoised tube. The net weight of the iron was found to be .23 of a grain. The great care in removing the phosphates may show a weight of iron comparatively light. It may be remarked, that throughout the analysis of Fernandez's blood, the phosphates caused much trouble and delay. A crucible could never be used a second time after simple washing, or the use of an acid. Fusion by microcosmic salt, or other flux, was required after each incineration. Notwithstanding this strong crusting of the crucibles, the ash of the several incinerations was very deliquescent, no doubt from the presence of potash. After, deliquescence spangles were seen over the surface. On examining one sample of residuary ash, on the last days of operations, with a one-inch object-glass, I found that these spangles were long flat prisms, associated with amorphous opaque crystalline matter, tinted brown, probably from the presence of iron (it was the sample from the albumen washing). The whole of the prisms rapidly disappeared on the addition of nitric acid, and a great part of the amorphous matter. An accident happened to the specimen, when ammonia was added and the glass put in the sunshine. So I cannot say that the crystals were restored. It is probable, however, that the prisms were triple posphates, and the amorphous matter phosphate of lime. The estimate of *water* in Richardson's blood appears high. This may be in part owing to the extreme care with which the drying process was conducted. A water bath was used in the first instance, and the mass was then pulverized, and the drying finished off with a heat of from 230° to 240° in a chloride of calcium bath.

"In extended investigation on the specific gravity of yellow fever blood, a law of age and sex might be evolved. It may be noticed, in the instances already given, that the lowest gravities are in the boy and woman. Other circumstances being equal, the copious ejection of black-vomit should increase the density of the blood, by diminishing its proportion of water, and this view would seem to be borne out by the converse fact, that Francesco Mark, who died without black vomit, has blood of the lowest density of all the adult males; and Juan Paul, who had but little black vomit before death, is the next lowest in the same category. In Francesco Mark's case, the specific gravity bottle was filled seven and a half hours after death, and the blood set firmly in it. Robert Laurence was fifty years of age: the specific gravity bottle was filled twelve hours after death, and was weighed two hours afterwards. The blood had then set firmly, without separation of serum. He had had copious black vomit before death. In George Crispy's case, the bottle was filled eight hours after death, and the blood set well in it. A quantity was also placed in a small beaker, and twenty-four hours afterwards, a soft coagulum filled the whole space occupied by the blood, without separation at the sides. But there was on the upper surface about half-an-ounce of very dark serum, with floating pellicles. This serum may have constituted about one-tenth of the whole. The blood smelled decidedly *urinous.* On examining it with Pritchard's one-fourth or one-third inch object glass (for the power is not stamped on the case), the pellicle seemed to consist of granules, such as are seen in the curdy sediment of albuminous urine. There also appeared what seemed to be oil globules, and other bodies which were not round, but somewhat prismatic in shape, and luminous in centre, such as are delineated in drawings of hippuric acid. As regards P. Burke, on the 12th of April, 1853, I tried to take the specific gravity of his blood two hours after death, but it coagulated so rapidly, that it could not be got into the bottle, in a homogeneous state, and there was an excess of serum introduced. It was weighed, notwithstanding, and its specific gravity, in the condition stated, was 1.04508, at a temperature of 87°. In the bottle, the serum separated clearly, and in large quantity, and was, as usual, alkaline. He had had copious black vomit before death. Some of his blood was also set up in a beaker, and after twenty-four hours, inspected. It then had the clot and serum separate; but the clot was very weak, and the serum of a bloody orange color, and turbid, from bile and the hæmatosine of ruptured cells. No fetor was observable. On the 9th of April, I wished to take the specific gravity of the blood of Charles Bush, who died in the Seamen's Hospital on the previous day, in order to compare it with that of Francesco Mark, the former having had copious black vomit before death. But he had already been dead twenty-four hours; and very soon after the blood was put into the bottle, it began to bubble with gas, and actually frothed soon after. When weighed while the bubbles of gas were rising, its specific gravity was still 1.050, and in this state of decomposition it was very fluid, and readily flowed out of the bottle when inverted.

"Numerous quantitative analyses of the blood of yellow fever would obviously be of the greatest importance to the elucidation of the pathology of the disease. But such an undertaking is impracticable; though a working chemist, associated with an intelligent physician, might well occupy five or six years in such pursuits, to the great advantage of the whole tropical world. In the meantime, much valuable information might be added by the zealous practitioner to the general stock, by multiplying accurate observations of the blood's density, and its alkalimetry, according to Griffin's method. These, with a table of some of the most important symptoms, and the age and sex, etc., of each patient attached, would yield excellent results."

EDITORIAL AND MISCELLANEOUS.

Lecture Season—*New Orleans School of Medicine.*—The lectures in this Institution will commence on the 15th of November. Preliminary lectures will be delivered to students who may come into the city before that time. Elsewhere the reader will see our report on the yellow fever for the past month. The disease is on the decline, and as the season advances we may anticipate that this decline will be more rapid. We will detain our number in order to give the latest intelligence. Our object is to furnish a true statement—one that may be confidently relied on.

Criminal Abortions.—In a late number of the *Buffalo Medical Journal*, there appeared an able editorial on this most interesting subject. The Editor handled not only the subject, but the authors of abortion pills, *and* the editors of newspapers who advertise them, "without gloves"—if we may be pardoned for using a common phrase. We have long looked on with wonder and disgust at the utter licentiousness of the day in the way of advertisement of patent medicines, and more than once we have been on the point of animadverting on the same; but we have always been checked by the reflection—*cui bono?* Our journal circulates almost wholly amongst medical men, who think already as we do, and the evil can only find a remedy through the medium of the daily and secular press—the levers which have set it in motion.

In our last number we copied from a daily paper some very opportune and legitimate remarks on the subject of popular treatment of yellow fever, hailing the same as a noble as well as *rara avis:* we now extract from the Buffalo Journal an editorial from the *Brooklyn Daily Times*, and ask a careful perusal at the hands of those non-professional persons who may receive the number:

"*Criminal Abortions.*—We have been requested by a medical friend, to call attention to an article which appears in the current number of the *Buffalo Medical Journal*, under the above caption. The subject is a delicate one for a family paper to allude to, much less discuss; yet if the allegations of the Journal are true—and our friend assures us that his practice and that of every member of the profession furnishes evidence of their truth—then assuredly no mock delicacy or prudery should be suffered to stand between error and crime, and their needful expósure. No one doubts the propriety of medical journals treating on such a subject; but if the evil is so wide spread as is stated, the medical press, unaided by the newspaper world at large, cannot remove it. It is the family newspaper alone, which reaches the class whose moral sense needs to be awakened on the subject—and therefore it is the general newspaper which must grapple with the evil and strive to overthrow it.

"The Journal asserts that the daily, and even the religious press, insert advertisements of professed abortionists; of pills which 'are sure to procure abortion,' from which 'miscarriage will certainly ensue,' etc. That in consequence of the continual and unchecked publicity afforded to this class of announcements, females have 'come to regard abortion as one of the most innocent and natural things in the world'—so that it is no uncommon thing for medical attendants to be as coolly and unconcernedly asked to produce abortion as a dentist would be requested to draw a tooth. Nay, it is asserted, and advertisements are extracted from popular journals to prove the fact, that instruments are openly announced, and largely used, which are warranted to 'regulate or limit offspring, without injuring the constitution.' Still further, the Journal does not hesitate to assert that neither the poor, the abandoned, or the ignorant, patronize these infernal inventions so much as the 'educated and refined' ladies of fashion and social position.

"We cannot but regard the extensive publicity given to advertisements of the class we have referred to, as a conclusive proof that the crime which they facilitate is of frequent commission. No article is largely advertised, for which there is not a large demand—for it is the extent of the demand which warrants the seller in producing his goods and placing them before the public. If, then, abortion is thus practised—extensively and with hardly the thinnest veil of disguise—it would ill become the conductors of the press, who claim to be the guardians of public morals, to conceal the fact, as to squeamishly hesitate in denouncing this class of advertisers as atrocious scoundrels, and any publisher who inserts their advertisements as *particeps criminis*.

"Both in law and common sense, infanticide is regarded as murder, and it needs little knowledge of physiology to be aware that abortion is but a form of infanticide. Unless our boasted civilization is to sink to the level of Chinese barbarism, our women, of every class and degree, must be convinced that to deprive their inchoate offspring of life, is to commit murder, and that under circumstances of aggravation which make the foulest of crimes more atrocious."

At the same time, we beg leave to insert here copies of advertisements of abortion pills clipped this day from our own home press. Comment is unnecessary, after what the *Brooklyn Daily Times* has said above. Surely our papers cannot imagine that we have any other than the best motive for calling attention to the subject. It is one in which we are only equally interested with every editor of the daily and secular press, and it is as much their interest and duty to use every legitimate means to preserve the moral and physical welfare of the community as it can possibly be ours. Surely it is more reasonable to suppose that real success in life depends more on the good than on the harm we do in society, and he who is powerful to do harm is equally powerful to do good. Through the medium of the press alone can abortionists and their kindred flourish, and by the press alone can they be annihilated. Shall good or evil prevail?

The Great English Remedy.—*Sir James Clarke's Celebrated Female Pills.*—Prepared from a prescription of Sir J. Clarke, M. D., Physician Extraordinary to the Queen.—This invaluable medicine is unfailing in the cure of all those painful and dangerous diseases to which the female constitution is subject. It moderates all excess, and removes all obstructions, and a speedy cure may be relied on.

To Married Ladies it is peculiarly suited. It will, in a short time, bring on the monthly period with regularity.

Each bottle, price one dollar, bears the Government Stamp of Great Britain, to prevent counterfeits.

These pills should not be taken by females during the first three months of pregnancy, as they are sure to bring on miscarriage; but at any other time they are safe.

In all cases of nervous and spinal affections, pain in the back and limbs, fatigue on slight exertion, palpitation of the heart, hysterics and whites, these pills will effect a cure when all other means have failed; and, although a powerful remedy, do not contain iron, calomel, antimony, or anything hurtful to the constitution.

Full directions accompany each package. Sole Agent for the United States and Canada, JOB MOSES, (late I. C. Baldwin & Co.) Rochester, New York.

N. B.—One dollar and six postage stamps inclosed to any authorized agent, will insure a bottle of the pills by return mail.

For sale by J. WRIGHT & CO.,
dec12—eod sat W&-S W 1y Wholesale Agent for the South.

Important to Females.—*Dr. Cheesman's Pills, prepared by Cornelius L. Cheesman, M. D., New York City.*—The combination of ingredients in these pills are the result of a long and extensive practice. They are mild in their operation, and certain in correcting all irregularities, painful menstruations, removing all obstructions, whether from cold or otherwise, headache, pain in the side, palpitation of the heart, disturbed sleep, which arise from the interruption of nature.

To Married Ladies.—Dr. Cheesman's Pills are invaluable, as they will bring on

the monthly period with regularity. Ladies who have been disappointed in the use of other pills, can place the utmost confidence in Dr. Cheesman's Pills doing all that they represent to do.

Notice.—They should not be used during pregnancy, as a miscarriage would certainly result therefrom.

Warranted purely vegetable, and free from anything injurious to life or health. Explicit directions, which should be read, accompany each box. Price $1. Sent by mail on inclosing $1 to the agents.

JNO. WRIGHT & CO.,
oc3 2ptf 21 and 151 Chartres street, New Orleans.

TILDEN & CO.'S EXTRACTS—*Inspection of Medicines—Our Tariff on Imported Drugs and Medicines.*—In our October number, we spoke of the medicines of Messrs. Tilden & Co. in terms anything but complimentary. We revert to the subject, because we think it one of vital interest to our professional brethren, and those of the community who rely on them in their time of greatest need. In condemning Tilden & Co.'s wares, we do not pretend to say that they stand alone in the unholy business of selling worthless drugs; doubtless there are others equally guilty, and if we knew their names, we would not hesitate one moment to publish them. If a tailor makes us a coat which does not fit, or which is of material inferior to that which he represents it to be, we do not hesitate to advise our friends to pass his shop; yet, how trivial a matter is this, when compared to the one under consideration! Ages of toil, both mental and physical, have taught medical men that certain substances have certain medicinal properties; the sick man, whose value to his family, or, it may be, to an entire community, is incalculable, places himself in the hands of the physician, who prescribes these substances on the basis of their known power in a state of purity; that prescription is filled by an apothecary who has procured his supply from the manufacturer or wholesale dealer. The medicine is taken, but it produces no effect, it is spurious—a man dies who would otherwise have recovered. Who is in fault? We say that death lies at the doors of the manufacturer or wholesale dealer, *and* the apothecary. The first is guilty of manslaughter indirectly, (though knowingly,) the latter is accessory to the deed. The first knew the article was worthless when he put it into the market; the latter either knew that it was spurious when he purchased it for retail, or (what is almost as bad) he failed to recognize its worthlessness when dispensing. "But (the reader will say) is such worthless medicine ever dispensed?" In terms far short of exaggeration, we say, *every day!* What were we told by a very intelligent druggist's clerk the other day? We asked him what he thought of Tilden's Extracts. "*They are worthless!*" Do you ever dispense them? "No, but we fill country orders with them, because *they are ordered!*" Another druggist told us that he had Tilden's Ext. of Taraxacum, which he considered very good—"but as for the others ——————"; here he shrugged his shoulders. Some time ago, we prescribed ten grains of calomel and two grains of pulverized opium for a patient whom we were treating for cholera morbus. The prescription was filled by an apothecary hard by; we remained to administer it; *perhaps* the calomel was pretty good, but there was not the slightest taste or smell of opium about the dose. If the powder had been colored with a little dust from the street, it would have been as powerful to narcotize. We had to go and look for good opium, while our patient suffered for the want of it. Is that not a strange condition of society which permits such wrongs to go unwhipped of justice?

But, hold! here comes a bundle of exchanges from the office. We open the *Cincinnati Lancet and Observer*, and we feel relieved. Ever unwilling to do any man or set of men the slightest injustice, we have often asked ourself the question—Are you sure Tilden's medicines are not what they purport to be? and, notwithstandtng we have always been forced to condemn them, the fear of being fallible in our judgment has kept us silent until a month ago. But now we find that others besides ourselves are having their eyes opened. Dr. Jas. F. Hibbard, of Richmond, Indiana, writes to the editors of the above journal that "the family physician of the Rev. Mr. Wakefield prescribed Tilden & Co.'s sugar-coated pills, labeled "Compound calomel—Plummer's." One pill, containing three grains, was given to a child eight years old, and produced such emesis and hypercatharsis, that Mr. W. returned with the others, declaring his belief there was some mistake, as he had often before used Plummer's pills, with none but the pleasantest effect."

One of the returned pills was cut open and examined, and presented a dull gray, instead of a brick-dust red appearance. Some of the pills were forwarded to Tilden & Co. In due season, they replied that they had *analyzed* them, and found them the "correct comp. calomel pill, according to the U. S. P." (our readers, of course know that U. S. P. stand for *United States Pharmacopeia*, while Plummer's pill is a *British preparation!*) Meantime, the pills were also put into the hands of two different competent analytical chemists, (one of them not knowing whose pills they were,) and both declared the presence of *Tartar Emetic*, instead of the Ant. Sulph. Precip. Now, the certificate of Mr. Locke, chemist, together with bill of expenses for analysis, were sent to Tilden & Co., who paid the bill and returned the following answer, which will be highly satisfactory (?) to all honest medical men:

"They answered that they had made some pills from a private formula, corresponding in ingredients with the comp. cal. pill, except in the substitution of antimonii et potassæ tartras, for antimonii sulphuretum precepitatum, in appropriately smaller quantity, and that in some mysterious way the wrong label must have been placed upon them—that they had made but few of them, and these few were yet nearly all on hand. They paid the bill presented, and authorized the apothecary here to draw on Tilden & Co., New York, for any amount they might think Mr. Wakefield entitled to on account of extra expenses, etc.; thus making the amend financial."

We call on physicians, now, to read for themselves, and having read, we ask if any intelligent and conscientious man will henceforth waive the right, and his solemn duty, to make his *own* prescriptions, plainly putting down for the apothecary the ingredients and their quantities? Who are Messrs. Tilden & Co., that they shall undertake to manufacture pills by the cart-load for us to prescribe—changing the British Plummer's pill at their discretion, and withdrawing the calomel from our own Compound Cathartic pill, to suit the tastes of "those who are prejudiced against its use?" Are men to be relied on who make a *British* compound according to the *United States Pharmacopeia?* and who put into a single pill Tartar Emetic enough to produce violent emesis and hypercathaesis in a child of eight years? Medical men talk a great deal about "the people" being gulled and humbugged by quacks and charlatans, and yet we are ourselves the dupes of the veriest humbugs of the day.

What do we now behold on our desk? Nothing less than the simultaneous appearance of the foregoing exposure of (to use a tender term) the shortcomings of Tilden & Co., and the advertisement of their wares, in pamphlet form, in three medical journals received—the first page of these pamphlets containing a flaming announcement of their "*Sugar-coated Improved Compound Cathartic Pill*, (WITHOUT CALOMEL)!" Notwithstanding Tilden & Co. tell us in plain English that they have had the temerity to change the formula of the Compound Cathartic Pill, in order to pander to the ignorant prejudices of the million; notwithstanding they have thus openly violated the formula officially established and recognized by medical men, and on no other authority recognizable by medical men, we ———— shall we say it? No, not for the miserable pittance which falls into our money-drawer; but from motives as inexplicable as fallacious, indorse their trash, and tell even those who do look to us with confidence in our art and science, come, eat, and be cured!

We cannot believe that we are morbidly jealous of the purity of our profession, and that therefore we see iniquity where other men see only what is good; but be it as it may, we only say what we honestly feel—that we would as soon advertise Townsend's Sarsaparilla as this pamphlet of Tilden & Co. If it is intended solely for medical men, then it is an insult to their intelligence; if for the use of families, then it is one of the best devised quack sheets we have ever seen. In our libraries, we have books on books treating fully and reliably of all the articles of the materia; when we desire to drink in knowledge of this sort, we can go to fountains which pour forth only the purest of waters; but Tilden & Co. would have us ignore these, and turn to their valuable synopsis; indeed, we are fast drifting into a sort of Homœopathic practice: all the doctor has to do, is to fill his saddlebags with Tilden's pills, dragées, etc., and put one of these pamphlets in his pocket, and he is ready to encounter disease in every shape and form. There is not a disease to which flesh is heir, for which there is not one or more remedies, and Tilden's preparations are the very best. What have families now to do, but to get a pamphlet, a "monthly journal," and a set of pills, dragées, etc., and they are as well armed as are those who possess themselves of the Homœopathic book and box.

This pamphlet of Tilden & Co. is, to our mind, as transparent a piece of quackery as is their "Monthly Journal of Materia, etc."—several numbers of which we have been honored with. This precious sheet is sent (so they say) to "30,000 physicians and druggists in the United States and Canadas, thus affording the most available medium of advertising to the profession." Also—"No notice of Patent Medicines will be received unless accompanied with their formula of preparation"; yet, "*Tarrant's Effervescent Seltzer Aperient*" is set forth in flaming colors, modestly claiming to "effectually remove" *only* "Dyspepsia or Indigestion, Bilious Affections, Headche, Heartburn, Acidity of Stomach, Costiveness, Gout, Rheumatism, Loss of Appetite, Gravel, Nervous Debility, Nausea or Vomiting, Affections of the Liver, etc, etc. Also, "*Tarrant's Compound Extract of Cubebs and Copaiva,*" almost as efficient; and last, though not least, Tilden & Co.'s own "*Wine of Wild Cherry.*"

But we are disgusted with the consideration of the subject. Is it possible that we are the only medical man in the country who thinks that pure medicine is as great a desideratum as pure air and pure food, and that, come from whatever quarter of the globe it may, pure medicine should be admitted into our country *free of all duty?* If we could, would we, as a nation, impose a duty on fresh and invigorating breezes from across the broad Atlantic? No. And why? Because in them comes health and happiness for our people. But would we not lay an embargo, if we could, on every puff of air that came from abroad to breathe pestilence over the land? Yes. Then why throw the slightest obstacle in the way of the *free* admission of all pure foreign drugs?—of course, condemning, under rigid inspection, every particle that is spurious. We are not statesman enough to know whether or not it is best to make our people pay more for a home-produced plow, coat, or shoe, in order to encourage home manufactures, but the plainest common sense tells us that the people *must* have the best medicines, at whatever cost to the manufacturers of our own country. We are in favor of rigid inspection, and the summary destruction of all spurious drugs, but we earnestly call for the removal of all duties from *pure* foreign drugs.

THE ETHER QUESTION.—The Boston Medical men are having a nice little quarrel over the *would-be* discoverer of the anæsthetic properties of Ether—Wm. T. G. Morton. It seems that this fellow Morton has the impudence to adhere to the position that he is the discoverer, and having obtained a patent from Government (a patent for what?), he is suing Dr. Davis, of the Marine Hospital, Boston, for damages for using Ether as an anæsthetic. We thought that Drs. Morton and Jackson had been long ago satisfied with the medals they had received from abroad (though we believe Jackson got all these) for discovering that which they too well know they never discovered at all; and we began to hope that Dr. Wells, of Hartford, (the *real discoverer*) might live to see the world giving him credit for having conferred one of the most precious of boons on mankind. But alas! for modest merit in these progressive times. This generation must pass away without acknowledging the real discoverer, because of the clamor kept up by the impudent impostor. In this day Wells is dead, and Morton lives. Soon Morton will pass away from the sight and minds of men, or if he be remembered at all, it will only be to be execrated;

then will the name of Horace Wells occupy a place beside that of Jenner, and afflicted men, women and children, as they lie down to sleep under the ruthless knife, will bless that name above all others earthly.

DEATH OF DR. B. F. HARNEY.—We regret to have to announce the death of Dr. B. F. Harney, U. S. A. Dr. Harney died on the 29th of August, ult., at Baton Rouge, where he has long been stationed, and where he has as long been held in the highest estimation by the community. From the Baton Rouge Advocate we take the following:

Dr. Harney was born in the State of Delaware, from thence he removed to Mississippi, soon after the completion of his medical education. When the late war with Great Britain was declared, he became connected with the army as volunteer surgeon, in which capacity he joined a volunteer company raised in Mississippi, in 1814, for the protection of the frontier. He was subsequently received into the military service of the United States as surgeon, and shared the hardships and glories that immortalized the campaign of 1815. He also served in the Black Hawk and Florida Wars, was appointed to the important position of Medical Director of the United States Army during the Mexican war, and accompanied Gen. Scott's command during its victorious march to the City of Mexico. His services were equally as available on the field of battle as by the couch of the wounded. During a guerrilla attack he was slightly wounded in the ankle. Since his return from Mexico, and when not engaged in active service, he has resided here as surgeon of the United States Garrison at this place. His age is supposed to be about 80 years.

The many noble traits which adorned the character of Dr. Harney, as a man and a citizen, secured him the esteem and admiration of his friends and fellow-citizens. Charity, "which covers a multitude of sins" was practised by him in its truest sense—he visited the widow and the orphan in their affliction, ministering to their wants, in his official capacity as a distinguished Physician, and through his purse, which was ever open to supply the wants of the needy. Dr. H. was the oldest Surgeon in the U. S. Army, and was, by seniority entitled to the rank of Surgeon General, which we understand he twice declined accepting. The remains of the lamented departed will be interred at 5 o'clock this evening. As we write, places of business are being shut up, and cannon fired every hour during the day, indicating the high respect in which he was held in this community.

FARTHER REMARKS ON THE VERATRUM VIRIDE AND CHLORINE IN YELLOW FEVER.

By E. D. Fenner, M. D.

It will be recollected, that in the last number of this journal, I called attention to the virtues of these new remedies for yellow fever. I have now to say, that farther experience has confirmed the favorable impressions then expressed. I have the pleasure to add, that these remedies have been pretty extensively tried by at least three of our most respectable physicians, who have expressed to me their entire satisfaction with the results. To what extent they have been tried by others, I have not ascertained, but hope the results will be reported in due time. For myself, I must state, they have been my chief reliance in the treatment of yellow fever since I first commenced their use. I have treated with one or both of them fifty cases in private practice—of which, four have been lost. Two of these fatal cases occurred before I commenced using the veratrum—one a delicate young man, whose system was so thoroughly poisoned, that he succumbed with black vomit on the second day; the other was a stout German, to whom I was not called till the end of the second day of a very severe attack, when he had been excessively purged by another physician. Of course, I had but little chance to save him.

Of the two other fatal cases, one was a very delicate single lady, about thirty years of age, who seemed to have no recuperative power whatever, and sank on the fourth day, without black vomit or other hemorrhage, the intellect clear and urine flowing to the last; the other was a married lady, about the same age, who was between the seventh and eighth month of pregnancy. She was attacked after suffering intense anxiety on account of the illness of her husband, and was delivered of a living child on the third or critical day of the fever. But for this unfortunate complication, from which so few women ever recover in this disease, I have but little doubt she would have done well. She lingered till the sixth day, and sank from nervous exhaustion; no hemorrhage, and the urine free to the last. This case, and that of her sister—a young lady living with her—were the only ones I have met with in which the veratrum, in five and six drop doses, repeated every fourth hour, failed to reduce the frequency of the pulse as much as was desired. I did not push this remedy to the vomiting point, which perhaps would have been better. In both cases, as indeed in all others, the chlorine acted well on the kidneys and liver.

Under the use of the latter remedy, I have had no case of suppression of urine, but several of retention.

I have only prescribed the veratrum for the purpose of *reducing excessive febrile excitement,* as indicated by the rapid pulse and extreme heat of the body, an indication for which I have heretofore resorted to local depletion. This is more effectually and safely done by the veratrum than by the lancet, cups or leeches, and without despoiling the system of its vital fluid, the blood. This medicine appears to act directly upon that portion of the nervous system which controls the blood-vessels, and thus prevents the evil effects of a too rapid circulation of the blood. But this is not all that is required in the treatment of a case of fever. Something is still needed that will either neutralize the poison that has caused the disease, or promote its elimination from the system. For want of a known antidote to fulfill the *first* of these indications, physicians have hitherto directed their chief attention to the *latter,*—and hence the remedies in common use, such as emetics, cathartics, diaphoretics, diuretics, etc. Now, if in chlorine we should find a remedy that fulfills both of these indications, it would certainly be an important achievement. This can alone be determined by extensive experience. So far, the testimony is sufficiently favorable to call for farther experiment. The chlorine mixture here alluded to, certainly does act most admirably on the kidneys and liver, and by so doing, it must tend to preserve the integrity or sound condition of the blood; for there is now but little doubt that in the course of severe fevers, the blood becomes as much corrupted by the retention of excrementitious elements, bile, urea, etc., resulting from the arrested or disordered functions of the great eliminating organs, as by the original poison that first deranged the system. In short, the chief object of nearly all our remedies in the treatment of fever, is *to aid Nature in keeping up the continued action of the secreting organs.*

The hope to find a *certain specific* for yellow fever—an antidote that is capable of at once neutralizing and disarming the poison that has been slowly and stealthily introduced into the system, deranging all its vital functions—would appear to be altogether chimerical; yet, why may we not hope to find a remedy capable of exerting some special, happy influence over the poison producing this disease, like that universally admitted to be displayed by the Peruvian barks over intermittent and remittent fevers? The virtues of cinchona were unknown to the ancients, and in searching their records of fever, we find details of numerous fatal cases which no respectable practitioner would suffer to die at this day; yet, we are still as ignorant of the precise nature of the *fever cause* as they were; and although we have discovered what may justly be called a *sovereign remedy* for intermittent fever, no better explanation has yet been given of its *modus operandi* than that ascribing to it some special or antidotal power.

Yellow fever belongs to the class of *malignant* fevers, which are supposed to be caused by some subtle poison, malaria, or infection that enters the system, and produces all the phenomena of the disease, either *directly*, by its immediate and continued presence, or *indirectly*, by deranging the important processes of nutrition and excretion. Now, when we contemplate the powers of chlorine as a chemical agent, outside the body—its capacity to destroy colors and offensive odors, to arrest the decomposition of animal and vegetable matters, and to purify and disinfect foul and unhealthy places—we do not think it at all improbable that it might possibly act as a disinfectant within the body; provided it be not itself prejudicial to the system. But here we are reminded of the fact, that chlorine is one of the constituent elements of the body, and is an essential ingredient of some of the most important secretions, especially that of the stomach; and besides, we know that Chlorine and its compounds furnish us some of the most valuable articles of our Materia Medica, which act powerfully on the secreting organs and the blood.

In view of these considerations, we are brought to the conclusion, that whether chlorine acts directly as a disinfectant or not, in yellow fever, it may certainly prove highly beneficial, by keeping up the vigorous and continued action of the chief eliminating organs, which it certainly does.

I have no desire to extoll these remedies unduly, nor prematurely to claim for them powers which they may not be found to possess. I have no idea that these, or any other will ever be discovered, that shall enable us to save all cases of yellow fever; nor should the delusive hope be for a moment entertained, that any remedy, how great soever its intrinsic merit, will invariably produce beneficial effects, without being applie with judgement and skill. But, at the same time, it must be admitted that we are in need of some more appropriate and efficient remedies than any we hitherto, possessed and it is with the hope of supplying this want, tha I am nduced to urge upon the profession the trial of the veratrum and chlorine.

I am fully aware of the repugnance universally felt to all sorts of innovations in medical practice; yet improvements are still needed, and generally find advocates sufficiently courageous to assert and establish their claims at the proper time.

As before stated, I have only prescribed the veratrum when the febrile excitement was excessive. It was not given in half of my cases, the fever in most of them declining fast enough under the use of chlorine alone.

I have pursued this practice in my wards at the Charity Hospital, and with very satisfactory success, considering the bad nursing.

I now give the formula for making the *chlorine mixture*, which should have been inserted in the last number.

℞. Acid. Hydrochloric.
Aquæ Distillatæ, *aa*ʒii.
Mix slowly and add Potassæ Chloratis, ℥ii.

Let this be kept in a dark or covered bottle, with a glass stopper. Of this, I order two drachms to be mixed with a pint of clear water, and direct two tablespoonsful every two hours for an adult. The dose should be graduated to the age of the patient. To small children, I give a teaspoonful, still farther diluted. If the patient be taking the veratrum at the same time, I give the chlorine mixture every alternate second hour.

I have notes of all my cases, but have neither time nor space to report any of them at present.

ALBUMINOUS URINE IN YELLOW FEVER.—In our last number, I mentioned the occurrence of albumen in the urine as a diagnostic symptom of yellow fever. Since then, I have tested the urine in a great many cases, and found it invariably acid, but not always albuminous. Some cases, in which the kidneys acted very freely under the use of the chlorine, displayed no albumen, so long, at least, as they were under my observation. Some are of the opinion that albumen only occurs in *fatal* cases; but this is certainly a mistake. I met with it in most of my severe cases that recovered. I am disposed to believe that the observations of Dr. Blair on this subject are, for the most part, correct. The occurrence of albuminuria in yellow fever and scarletina points to a pathological resemblance between these two diseases, which would tend to explain the like beneficial effects of the same remedy in each. Dr. J. Hughes Bennett, in his late work on Clinical Medicine, says: "The coagulability of the urine, as well as various deposits which appear in it on critical days, must, when they occur, be considered as an evidence of the excretion of morbid products which have circulated in the blood; hence, they are common, not only in scarletina, but in all inflammatory affections, as well as in fevers."—E. D. F.

OUR ORIGINAL MATTER.—For reasons which have been heretofore laid before our readers, we have been obliged to defer the completion of Robin's monograph on the mucous membrane of the uterus until the present number. It is now complete, and we venture to say that no more valuable paper has appeared in any journal in the country. The progressive medical man *must* study microscopic phenomena True, every one cannot have a microscope, nor spare the time to make actual observations, but all can study the results of the labors of men so reliable as Robin.

An accident prevents our completing Dr. Harris' valuable paper in this number. It will be completed in the December number.

NEW MEDICAL JOURNALS.—We have received the second number of the Medical Journal of North Carolina, and find it replete with interesting matter. If the profession of North Carolina do not fully sustain such a journal, they are certainly blind to both interest and duty.

"The Belmont Medical Journal," is another new journal, of which we have before us the June, July, August and September numbers. It represents the Belmont Medical Society (Bridgeport, Ohio). It is a small monthly journal of sixteen pages, but is well edited by Drs. J. G. Affleck and J. M. McConahey, and will, no doubt, be well patronized.

"The Medical and Surgical Reporter" has not only changed its domicile to Philadelphia, but has become a "Weekly." We have received two numbers, the first of which is almost wholly devoted to setting forth the advantages of Philadelphia as a place of resort for medical students, and is called the "Students' Number."

HEALTH OF THE CITY—*October* 29*th*, 1858.—We have delayed the issue of our journal until this late date, in order to furnish our readers with the latest intelligence in relation to yellow fever.

Since our last issue, the weekly mortality by yellow fever has been as follows:

For the week ending 26th September, 444; week ending 3d October, 380; week ending 10th October, 390; week ending 17th October, 308; week ending 24th October, 265.

Since the 24th instant, the daily mortality has diminished materially—

averaging about 30; and for the past two days being respectively 21 and 25. So that we may now safely predict the speedy disappearance of the disease. A few more days, and absentees will be pouring in.

Report of Deaths in the City of New Orleans, for five weeks ending October 24th, 1858.—*From 19th to 26th September*, 1858.—Men, 319; women, 112; boys, 111; girls, 77; adults, 431; children, 188; males, 430; females, 189. Total, 619.

From 26th September to 3rd October.—Men, 283; women, 96; boys, 90; girls, 71; adults, 379; children, 161; males, 373; females, 167. Total, 540.

From October 3rd to October 10th.—Men, 271; women, 93; boys, 95; girls, 78; adults, 363; children, 173; males, 361; females, 171. Total, 537.

From October 10th to October 17th.—Men, 257; women, 63; boys, 108; girls, 62; adults, 320; children, 170; males, 365; females, 125. Total, 490.

From October 17th to October 24th.—Men, 191; women, 66; boys, 89; girls, 64; adults, 257; children, 153; males, 280; females, 130. Total, 410.

Total mortality for five weeks, 2596. Of these, 1787 yellow fever.

PAMPHLETS RECEIVED.

"LECTURES ON THE PRINCIPLES AND PRACTICE OF PHYSIC. Delivered at King's College, London, by THOMAS WATSON, M. D., Fellow of the Royal College of Physicians, etc., etc. A new American, from the last revised and enlarged English edition, with additions. By D. FRANCIS CONDIE, M. D., etc., etc. With one hundred and eighty illustrations on wood. Philadelphia: Blanchard & Lea. 1858."

We are at last in receipt of the long-wished-for new edition of Watson's Lectures. All we can do at this late hour is to note its receipt, thank the publishers for the copy, and promise to say more about it hereafter. One thing is certain; no practitioner should be without the new edition.

"THE DUDLEY OBSERVATORY AND THE SCIENTIFIC COUNCIL. STATEMENT OF THE TRUSTEES. ALBANY: 1858."

We have been very much interested in the perusal of this pamphlet. We have been waiting to hear this side of the question, and we think the

Trustees have not only vindicated themselves, but have completely convicted their opponents. A journal like this cannot undertake an extended notice of this controversy, but all enlightened men must feel an interest in the welfare of such an institution as the Dudley Observatory. We think it a great misfortune for Astronomy and the Observatory that Dr. Gould ever had anything to do with it.

"THE TRANSACTIONS OF THE NEW HAMPSHIRE MEDICAL SOCIETY, SIXTY-EIGHTH ANNIVERSARY, HELD AT CONCORD, JUNE 1ST AND 2ND, 1858."

"PHYSIOLOGY, PATHOLOGY AND THERAPEUTICS OF MUSCULAR EXERCISE. A paper read before the Cook County Medical Society, and published at their request. By W. H. BYFORD, M. D., Professor of Obstetrics, etc., in Rush Medical College." From the author. A very interesting and instructive essay.

"THE AMERICAN HOMŒPATHIC REVIEW. EDITED BY ROGER G. PERKINS AND HENRY M. SMITH. OCTOBER, 1858."

We acknowledge the receipt of this precious organ of quackery, the avowed purpose of which is to annihilate truth and science, and to exalt humbuggery by preying on the credulity of man. We must respectfully decline the honor (!) of exchanging, as we should deem ourself in some measure aiding and abetting fraud. We desire to hold no converse with Homœopaths, either personally or otherwise, for while we have too much respect for their common sense to believe that they are fools, we must conclude that they are unmitigated knaves. We claim to be a *Physician*, and this a Homœopath cannot be.

EXCERPTA.

A CASE OF PUERPERAL APOPLECTIC CONVULSIONS, WITH SPONTANEOUS EXPULSION OF THE FŒTUS AFTER DEATH.—*By Benjamin F. Fessenden, M. D., Plymouth, N. C.*—June 2, 1848. I was called to Mrs. H——, a woman of medium stature, rather robust, aged thirty-one years. This was her third pregnancy; in the two first labors she did well, they were natural and terminated within the usual period. She had been in good health, but complained for the last month of headache, or a *bad feeling* about that organ. The day before I saw her, she complained of severe headache, and had a spell of blindness of short duration. At 8 o'clock, P. M., she had symptoms of labor, being about the completion of her term. Her husband informed me that she was seized with violent convulsions about 10 o'clock, and that they had continued, with but slight intermissions, up to the moment of my arrival.

Owing to the distance from me, I did not see the patient until about 5 o'clock, A. M., June 3; I then found her in a supine position, continually convulsed, every feature and gesture distorted, respiration attended with a hissing sound, frothing at the mouth, and with each paroxysm lasting about ten minutes; after which she sank into a stertorous sleep. She was insensible, exhibting no signs of consciousness or feeling; her pulse was full, hard and slow; the pupils of her eyes were dilated and insensible to light; her bowels were confined, and there was no uterine action during the time I was with her—if there had been any they were entirely suspended.

Treatment of the Case:—I immediately bled her, allowing the blood to flow until it made a decided impression on the pulse. Gave her twelve grains of calomel, to be followed by a wineglassful of black draft every hour, until it operated on the bowels; and also recommended enementa of infusion of senna, sulphate of magnesia and ol. terebinth, twelve ounces at a time, to be repeated until the bowels were thoroughly moved; with cold applications to the head. At 10, A. M., I sent for Dr. W. W. Ward, in consultation. When he arrived about 2 o'clock, P. M., there was no change in the condition of the patient; there had been no action from her bowels, and the enemas (three in number) were all retained. Upon consultation we applied cups to the temples and back of neck, blisters to back of neck and thighs, and gave two drops of ol. tiglii. On examination we found the os uteri still undilated and hard, and with no perceptible progress of the labor. She died about 6 o'lcock, P. M. The corpse was dressed as usual for the grave; and the day after, when about to put her in the coffin, the child was found delivered. At what time this spontaneous action of the womb took place, I had no accurate means of knowing. On inquiry, I learned from those who were sitting up with the corpse, that about six or eight hours after it was dressed, there was an agitation or movement of the body, accompanied by a strange sound, which somewhat startled them at the time, but that attributing it to their imaginations, they thought no more of the matter until the discovery of the child. I supposed the expulsion must have taken place at that time.

VERATRUM VIRIDE.—*By James B. Colegrove, M. D., Sardinia, N. Y.*—[Communicated to the Boston Medical and Surgical Journal.]—This article, which has been so recently, and yet quite extensively introduced amongst the profession, and which, as an arterial sedative, seems likely to become a desideratum, judging from the high encomiums bestowed upon it, is nevertheless a *poison*, and whatever virtues it may possess when administered to children, it should, I think, be given with the greatest caution. It is in inflammatory affections of the respiratory organs that it exhibits its qualities in so effectual and happy a manner; and it is equally injurious and dangerous in all diseases where there is much tendency to emesis. I have never succeeded with this remedy in any degree in enteritis or peritonitis, nor would I attempt its employment in gastritis, or inflammation of the brain, or any of its appendages. Its indiscriminate use in all inflammations would be likely to lead to the most disastrous results. Again, I conceive that there are certain nervous idiosyncrasies which forbid its use, requiring the nicest discrimination on the part of the physician.

Case.—A. B., a girl, 9 years old, on February 18, 1858, was seized, at 9, P. M., with severe rigors, followed by fever, thirst, headache and pain in the left lung. I was summoned on the morning of the 20th, and found the condition of the little patient as follows: Pulse 140; tongue covered with a white coat; respiration painful; intense fever, which appeared in occasional flushes, followed by "sinking spells"; a racking cough, and bloody expectoration. The above symptoms of pneumonia were confirmed by auscultation.

Treatment —Veratrum viride (Norwood's tincture) in doses of two drops, in a teaspoonful of water, once in three hours, with directions to increase the quantity *one drop* at every subsequent dose, unless vomiting occurred, in which event, the quantity to be given should be lessened one half. I also directed the application of tepid water, containing a solution of supercarbonate of soda, to the surface of the body every third hour, and fifteen drops of sweet spirits of nitre every third hour.

Evening.—Patient exceedingly restless. Pulse 144. Three doses of veratrum viride have been given (nine drops). Expectoration unchanged. Other symptoms are the same as in the morning. Treatment to be continued, and three grains of Dover's powder to be given at 10 o'clock.

Feb. 21st, 9 o'clock, A. M.—The mother of the child has been considerably alarmed by the appearance of the patient. At intervals of two hours, she said, the child would become frantic and wholly unmanageable; her language was incoherent, and she finally became nearly or quite insensible. Thrice this has occurred during the night. The symptoms this morning are unchanged, except that the face is pallid, the lips compressed, and the eyes wide open and glaring. Four doses of veratrum viride have been given (sixteen drops). I directed the nurse not to increase the dose above this quantity. Now, the patient has taken seven doses of the veratrum viride, in all twenty-five drops, in twenty-four hours, with no amelioration of her condition. The haggard expression of her countenance, and the unusual symptoms which have sufficed to create alarm in the mind of the mother, are undoubtedly due to the medicine. No vomiting has yet taken place. Shall I continue the veratrum? or shall I discontinue its use and resort to the lancet? After considerable consultation, I concluded to increase the dose one drop, and try it twelve hours longer. Treatment otherwise unchanged.

12 o'clock, noon. The unusual interest which I have felt in this case induced me to make another visit after the expiration of three hours. Two doses, of five drops each, of veratrum viride have been given. I should have mentioned that I noticed a slight dilatation of the pupils his morning. Now, this dilatation is increased. The whole expression of the eyes and countenance is wild and unnatural; breathing hurried and difficult; lips white; mouth firmly closed; subsultus tendinum; patient starts up wildly, and screams as if deranged; pulse 160; skin dry, but without much heat; she refuses the caresses of her mother, whom she does not recognize. I resolved to continue the veratrum viride six hours longer, and, in case there should be no improvement, to discontinue it altogether. I have frequently given the medicine, but have never witnessed like results before. Dose, five drops once in two hours. A Dover's powder of three grains to be given during the first interim.

9 o'clock, P. M.—Immediately after the Dover's powder had been swallowed, copious vomiting ensued, since when the patient has slept. I found her now sleeping quietly; pulse 96; skin moist and cool; breathing natural and easy. From this period the patient was convalescent.

Had I not commenced the use of this powerful drug, I think after witnessing the effect produced upon the child, I should have withheld it and employed the lancet, which, with calomel and antimony, is generally sufficient to restore the worst cases of pneumonia. As it was, the result was favorable, yet the ordeal was truly fearful. I will not detain the reader with further remarks upon this case.

I have used the veratrum since the first of January last, in thirty cases of inflammation, and, with some exceptions, I regard it as a safe remedy in the hands of a careful physician. But I do not think it safe at all when given *ad libitum*, or in the hands of nurses. On the contrary, its effects should be constantly and carefully watched, and by the physician himself. Its powers as an arterial sedative have not been over-rated. It possesses alterative qualities, also, which are by no means inconsiderable.

CHLOROFORM IN NATURAL LABOR.—*Letter from Dr. Rigby.*—Having received some queries on the action of chloroform in midwifery, I venture to offer a few remarks on the subject, which I have made in the course of my practice.

I may premise that the quantity administered at a time in different patients has been as nearly the same as possible, and I have not seen any reason to alter the mode of giving it, which I adopted shortly after its wonderful powers were made known by Professor Simpson of Edinburgh. A piece of sponge, of about the size of one's thumb, slightly moistened with chloroform by holding it against the open mouth of a bottle and shaking it once or twice, is the only inhaler, if it can be called such, which I use. I always direct the patient to apply it herself at first, in order to give confidence, and avoid the disposition to gasp and struggle, if the sponge is held by another person. In a few moments the hand begins to drop, and it may then be placed on the upper lip, which has been previously well greased with cold cream, and a pocket-handkerchief laid lightly over the face. The effects are produced quicker and more decidedly when the face is covered with a handkerchief, since she now breathes a chloroform atmosphere by the mouth, while she inhales the vapor, nearly pure, by her nose.

In natural labor, where it is not desirable to produce a powerful effect, the rule, which I long ago pointed out in this journal, of endeavoring, as far as possible, to draw the line between allaying pain and destroying consciousness, ought I think always to be adhered to. A patient may thus be kept quite sufficiently under the influence of chloroform, for any length of time, and yet be able to hear and answer questions; and thus we frequently hear a patient remark, "I know I have a pain, and yet I do not feel it!"

That the action of chloroform is considerably modified by certain circumstances and conditions of the system, is well known; witness the restlessness, talking, and even screaming, so frequently seen in hysterical females; the vomiting which follows where food has been taken shortly before; the severe throbbing of the head and tinnitus aurium during the early inhalations, and the intense sick-headache and even vomiting, which follows the recovery from the exhibition of chloroform where the bowels are loaded or deranged; but independently of these and other modifying causes, we see it affect the uterine contractions very differently in different people, even where no unpleasant effects have been produced; where, on the contrary, the patient has repeatedly begged for more, when she feels its effects wearing off, and where none of the causes to which I have just alluded existed.

In natural labor, where the pains are strong and quick (especially if it be not her first labor), the chloroform appears to have no other effect than that of deadening sensation; the os uteri yields so easily, the head descends quickly, and with so little effort, that one is almost tempted to conclude that the soft parts have been rendered more dilatable by the chloroform; and this is especially the case as the head approaches the os externum, through which it frequently passes so quickly and imperceptibly, that a person unaware of the fact would probably find the head born before he had thought it necessary to support the perineum. This is the usual action of chloroform in natural labor, when given to a moderate extent; but we meet with cases every now and then where it undoubtedly retards labor, either during the first or dilating stage, or during the second or expelling stage. It is easy to understand why chloroform should affect the manifestation of that partly voluntary, partly involuntary power which is destined to assist uterine contraction, when the presenting part has passed through the os uteri and entered the vagina. "At this period," as has been well expressed by Dr. A. Farre ("Cyclopæd. of Anat. and Phys., p. 673), "the abdominal and pelvic muscles are brought powerfully into play. Their coöperative action is occasioned by the parts of the child occupying the pelvis irritating structures which are abundantly supplied by spinal nerves. And now the chief use of spinal reflex action, in relation to labor, becomes manifest, not so much in regard to the uterus itself, whose contractions are probably still mainly dependent upon its own sympathetic nerves, as in that correlation with other parts, between which and the uterus it is essential that consentaneous action should be occasionally established." But during the first or dilating stage, it must be remembered that the pains are uterine contractions, unmixed with any auxiliary activity; that the uterus is thrown into these contractions by the agency of its sympathetic nerves; and that they follow (as Dr. A. Farre has shown) a rhythmic action similar to that of the heart and respiratory muscles, and as little under the control of the will as these organs are. It is therefore difficult to understand how it is that we occasionally see the pains of the first stage (involuntary rhythmic uterine action) so checked by chloroform as to require its discontinuance, in order that labor may proceed. For a long time I resolutely doubted this fact, and endeavored to attribute it to accidental circumstances; but it has recently occurred to me with such distinctness as to set the question at rest in my own mind. The patient, a young primipara, was in excellent health; she had no apprehensions about the chloroform; on the contrary, she repeatedly asked for it before I would give it her; the pains were perfectly regular, and fairly active, and the chloroform was given in a few drops at a time on a sponge, in the manner already described. According to her own sensations, it made her feel tossy and restless. The pains immediately became feeble and inactive, and after a short time were nearly suspended. I stopped the chloroform; the pains soon returned, and becoming brisk, she begged she might inhale some more. The pains again ceased; returned again, when the chloroform was stopped; and again ceased on her making a fresh trial of it, until being herself convinced that it retarded labor, she determined to go on without, and a large child was born after a perfectly natural labor.

The fact of chloroform being capable of suspending uterine action, even when given in small quantities, is one of sufficient rarity to justify its being looked upon as almost the exception to the rule; but its power to suspend the partly voluntary, partly involuntary efforts, which are destined to assist the uterine contractions during the expelling stage, is one of such frequent occurrence as must have excited the notice of all who have devoted any attention to the subject. The unaided uterine contractions are insufficient of themselves to force the presenting part over the perineum and through the os externum; as long as the patient is under the influence of chloroform, she makes no effort, or assists so feebly as to be of no use, and at this rate, the case would soon require the assistance of the forceps. But if the chloroform be stopped, the bearing down efforts again show themselves, they soon become vigorous and active, and the labor is speedily terminated.

I have no reason to suppose that chloroform, given under proper precautions, can exert any prejudicial influence either on the separation and expulsion of the placenta, or on the recovery of the patient afterwards. With regard to the first, it may almost be stated that chloroform is never given during this last stage of labor, the patient ceasing to inhale it the moment the head is born, so that she has usually recovered from it before the placenta was expelled; on the other hand, I have frequently seen the placental stage terminated while the patient was still unconscious, and without the smallest interruption to its healthy course.

In severe, and particularly instrumental labors, where the bowels have been carefully regulated, and food not taken shortly before inhalation, chloroform is undoubtedly as valuable in preserving the patient from the effects of the shock on the nervous system as it is known to be after surgical operations, and so far from producing any unpleasant or mischievous after effects, I feel convinced by ample observation, that when judiciously administered, the prognosis, as regards the patient's recovery after a severe labor, is more favorable where chloroform has been used than where it has not been. I am, etc., E. RIGBY, M. D.

—*London Medical Times and Gazette.*

INTUSSUSCEPTION TREATED IN A NOVEL MANNER.—In the *Southern Medical and Surgical Journal* we find the following, which is part of a report of a case by Dr. Tate, of West Point, Ga.:

"March 17th, 8 o'clock, A. M. Dr. D. arrives. Upon being informed of what had been done, proposed putting the patient under the influence of chloroform, and again attempting the reduction of the hernia. The suggestion was adopted. Dr. D. failed in his attempts at reduction. Whereupon I proceeded to operate for strangulated inguinal hernia, and accomplished in this way the reduction of the strangulated portion of intestine.

"At noon, two hours after the operation, patient still continues to vomit and hiccough—says he is relieved of pain at the point of strangulation, but suffers intensely near the umbilicus (to the right of umbilicus). Prescribed ℥iss of castor oil, and renewal of the injections of warm water, using no salt. This treatment was continued until 10 o'clock at night, at which time I procured a pump syringe, and with it threw into the bowels six pounds of warm water, which was soon ejected without either smell or color; I then proceeded, after the lapse of an hour, to inject water slowly into the bowels, until they retained the enormous amount of one gallon.

"Croton oil had been given since 6 o'clock, A. M, in four drops at a dose, repeated every hour until sixteen drops were given, without the least effect upon the bowels being manifested.

18th, 2 o'clock, A. M. Being well satisfied that an intussusception, or other mechanical obstruction, existed above the strangulated point, and having, as I conceived, used every remedy worthy of trial in such a case, I determined to proceed upon my own responsibility, let consequences be as they might; therefore, I began again the use of warm water enemas, throwing them into the bowels slowly and cautiously; and after having introduced, by a pump syringe, one gallon of water, I next dissolved 40 grs. of *tartaric acid* in ℥iv of water, introduced that into the intestine; had a large compress prepared and placed in the hands of a strong negro fellow, with instructions to apply it to the *anus*, and hold it there, so as effectually to prevent the escape of either gas or water after I should introduce 40 grs. of *bi-carb. of soda*, dissolved also in ℥iv of water. The soda was introduced, the compress used admirably, and poor Will rolling on the floor, crying at the top of his voice, 'I shall burst, I shall burst—take that thing away, my bowels are tearing in two.' The compress was removed; gas, water and fecal matter escape freely, to the astonishment of all bystanders. In half an hour the same amount of warm water, tartaric acid and soda were used again, and with the same happy effect.

"The only medicine given after this was calcined charcoal, which passed through his bowels without difficulty. All being well satisfied that the obstruction was fully overcome, and Will declaring himself cured, he was discharged on the 20th."—*Med. Jour., of North Carolina.*

GLYCERINA CUM FERRI IODIDO.—As an interest appears to exist at present respecting the preparation glycerina cum ferri iodido, perhaps the following formula will be acceptable for its simplicity, quickness, and perfectness: Put into a three oz. phial 2¾ fluid ounces of pure (colorless and anhydrous—sp. gr. 1267) glycerine, and then insert a small glass funnel, so that the point shall be immersed in the glycerine; place a 2 drachm filter into the funnel. Into another (1 oz.) phial put one-eighth of an ounce of clean iron wire, cut into small lengths, one-fourth of an ounce distilled water, and 100 grains of iodine; shake the whole until the froth is white, and then at once decant the liquid into the filter afore-mentioned. When it is all through the filter, put ten drops of water into the 1 oz. phial, and shake it about to wash the iron wire, and then drop it round the upper part of the filter to wash it also. The contents of the phial require now only to be shaken together, and the process is completed. The whole may be done in less than half an hour; and if the glycerine be of the character above mentioned, and the operator expert, the preparation will be colorless and quite thick, and, what appears to us of some moment, of similar strength to the syrupus ferri iodido. To make the preparation similar in strength to the Ph. L. and Ph. D. 96 grains and 112 grains of iodine respectively will be required.—*Mr. Smith in Pharmaceutical Journal.*

COÖPERATION WITH HOMŒOPATHS.—The following is the conclusion of a speech by Dr. Ranking at the Annual Meeting of the East Anglian Branch of the British Medical Association: I will apprise you of the fact that curiosity tempted me to discover the antecedents of several of those who have seceded to homœopathy. You have doubtless generally been told, as I have, when some new accession to the ranks of the homœopaths has occurred, that the neophyte has sacrificed a previously good position, and has been constrained by conviction to adopt the new views; with, perhaps, the addition of lamentations that the inspiration of the "divine Hahnemann" had not sooner illuminated his benighted mind. In fact, if we are to believe all we hear on this head, and which the public take for granted, there never was a diplomatised homœopath who had not resigned a splendid practice on the old system, and who, disinterested man that he was! had not preferred to begin life anew, rather than continue longer in the errors taught by a Cullen, a Gregory, a Prout, a Bright, and a Watson. Unfortunately for the disinterestedness of many at least of these gentleman, the inquiries I have made entirely bear out the assertion of our associate Dr. Barker, "that no instance has occurred of a man with a good practice becoming a convert to homœopathy." In those instances I have inquired into, including one which has recently made some noise, I have ascertained that the individuals did not in any case enjoy the confidence of the community—in fact, that, as regards practice, they were disappointed men. Here, I think, we have a ready solution of their apostasy. I will now propose the following resolutions: 1. That the system of treating disease called homœopathy has been subjected to close scrutiny and unbiassed investigation, not only by individual Physicians, but by Medical commissioners of undoubted credit, and has been pronounced to be destitute of probability as a theory, and when applied to real and serious disease, of success in practice. 2. That, in the land in which it originated, it has nearly become extinct; while it is expressly forbidden in the public services of this and other countries, as chimerical and unsafe. 3. That this meeting regards homœopathy as one of the many modes of playing upon public credulity, and considers that any Physician or Surgeon who meets in consultation, or otherwise encourages a practiser of this system, is unfit to remain a member of the British Medical Association. 4. That the members of the East Anglian Branch of the Association pledge themselves to refuse consultation or other professional communication with homœopathic practitioners. These resolutions received the signature of the President, and twenty other of the members present.—*London Medical Times and Gazette.*

CASE OF A PREGNANT WOMAN, BOTH OF WHOSE MAMMÆ HAD BEEN EXTIRPATED.—*By Robert Lee, M. D., F. R. S.*—9th September, 1828. A woman, about 40 years of age, in the last month of pregnancy, applied at the Westminster General Dispensary this morning for a midwife to attend her during her labor. She stated that both mammæ had been removed for a cancerous affection several years ago, and that since their removal she had borne five children, which she had brought up by hand, and that they were all now in good health. Previous to the extirpation of the mammæ, which was performed in Ireland, she had borne two children. She states that her mother and, I think, one of her sisters, had been destroyed by cancer of the breast, and that she, becoming alarmed for herself in consequence of the appearance of hard painful tumors in both her mammæ, consented to have them removed, which was accordingly done by a Surgeon in Ireland. The glands had been completely removed, and the cicatrices were distinct. Since the operation she has enjoyed excellent health. During gestation nothing unusual is experienced in the situation of the breasts; but the second day after the confinement there is an unusual fullness in the parts, and in the glands of the axillæ, similar to what occurs after delivery, when the secretion of milk is about to take place.

I had nearly forgotten this case, when I received the following note from Sir Benjamin Brodie, which made me refer to my journal, where it had been recorded:

"Broome-park, near Dorking, Surrey,
September 29, 1843.

"*My Dear Sir*—Will you be so kind as to inform me whether you have any experience as to what will happen to a woman who, having lost both her breasts, was delivered of a child? Would the constitution suffer from the entire loss of the apparatus for secreting milk under such circumstances?

"Believe me to be, dear sir,
"Yours always truly,
"B. C. Brodie."

In reply to this note, I inclosed a copy of the history of the preceding case from my journal. I have not since 1828 met with any other case like this. 16th July, 1858.—*London Medical Times and Gazette.*

What is "Change of Type in Disease?"—*By W. O. Markham, F.R.C.P.L., Physician to St. Mary's Hospital.*—What is the meaning of the term "Change of Type in Disease?" It has struck me, in thinking over the answer that should be given to this question, that different persons attach a very different sense to it, and that many who make use of it, not unfrequently do so without holding any very distinct or definite idea of the term. It would therefore be well, perhaps, to dwell for a moment on this point, and to endeavor to discover what is the real and proper value which this phrase, "Change of Type in Disease," ought to bring with it. Such a discovery might, I believe, preserve disputants from a great deal of erroneous and superfluous reasonings. I am the more inclined to refer to this subject, because, as far as I know, those who use these now familiar words have not themselves explained the exact meaning which they would infer from them.

The first particular which strikes one on considering this matter is, that the hypothetical change may affix to different objects, viz: to the disease itself or to the person who is the recipient of the disease, or to both the one and the other at the same time. Now it cannot be doubted for a moment that some writers who use the phrase, mean thereby to convey to their readers the idea, that an actual change has taken place in the nature of the disease itself—in its intensity, for instance, or some other of its attributes; that there is some particular quality in the morbific element which causes it, when it falls upon or enters into the body, to manifest its presence in a different manner at one period from what it does at another. That such was the idea that Sydenham attached to the epidemics he describes, seems tolerably clear. He found that certain of these diseases, even during the course of a single year, so varied in their characters, that a remedy which, as he says, would cure a man in February, would probably kill him in the following October. It is quite clear that he never meant to infer from this, that the constitution—the bodily condition—of the man had undergone, during the interval between those two months, such a change as would account for the difference in the symptoms of the epidemic at those different periods.

And surely when we now-a-days speak of epidemics and their varities, we hold very much the same notions as Sydenham did about them, in this respect; and we do so, because we find that, during certain years and at certain periods of the year, epidemics act with greater force and violence, and that their influence reaches more extensively through the people, than they do during other years and at other seasons. An epidemic typhoid fever, or scarlatina, for example, may be at one time of the year what is called "mild" in its character, and the patient attacked by them pass gently and quietly through the exacerbation of the fever, and arrive safely at their cure; at another period, and not a distant one, the same fevers may assume what we call a "malignant" form, and then they fall with peculiar violence on their victims, and with very fatal effects; those who recover from them have often a hard struggle for life, and many succumb during the struggle. Now, it is out of all question to suppose, that any changes which the human constitution can have undergone during the short period of time which has elapsed between the visitations of these so different forms of epidemics, can explain the difference in their forms.

We trace the change, and in the present state of our knowledge, we are logically compelled to do so, to some peculiarity in the nature of the epidemic, to its intensity or malignity, as we say metaphorically. These diseases, then, *change their type* quite independently of any change in the constitution of the individual attacked by them; but, of course, they may be likewise influenced by any such change in the constitution.

But it is very certain, again, on the other hand, that writers, when they speak of change of type in disease, and especially in reference to the dispute about bleeding in inflammations, very generally mean to infer thereby the fact of a change in the human constitution itself: they, as I understand them, mean simply this, that the human body is not now constituted exactly as it once was at some previous period, and that it therefore resents diseases, and the remedies applied for their cure, differently from what it did at that previous period. They do not argue, for instance, that pneumonia is in itself a different disease from what pneumonia was fifty years ago; all they maintain is, that the body in which that inflammation manifests itself has undergone some great change. And the proof of the change is founded on the fact, that whereas, in former days, those who suffered under this inflammation bore bleeding well, but that now men no longer do so.

Thus, then, it appears that there are, as I have started with saying, two very different ideas attached to this term, "change of type;" one idea, supposing that the disease itself is altered in some of its qualities; and the other, that the constitution of the individual in whom it is manifested has undergone a change. And, consequently, any arguments in favor of the theory of a change in type in inflammations, which is derived from a consideration of the different methods of treatment applied at different times to epidemic fevers, cannot have the same value and the same meaning, as those which are founded upon the changes in treatment which pneumonia has undergone in recent times. But authors, and in particular Dr. Christison, argue from the comparison of the effects of treatment of epidemics in past and in present times, that diseases have changed their type, and applies the conclusion to the inflammations of the past and the present times. Epidemic fevers vary in their very nature—this is admitted; but no such distinction has ever been taken in the case of pneumonia or other inflammations. What is asserted in the term "change of type," as applied to pneumonia, is, that the constitution of those now subjected to it differs from what it formerly did. It has never been said, that pneumonia, like fevers, possesses in itself at different times a different character. There is clearly then a very marked distinction between the two cases, and this has not been observed by writers on this subject.

Inasmuch, therefore, as epidemic fevers admittedly differ essentially in their nature at different times, having at one time more, and at another less, of an asthenic character, it necessarily follows, that great suspicion must always attach to any conclusions concerning alterations in the constitution of the human body which are derived from a consideration of these fevers, as observed at different and distant epochs. And even in the case of inflammations, is it not also certain, that they also at different times differ in their essential nature, though not in so distinct and well marked a manner as epidemic fevers? We often observe that pleurisy and pneumonia of one time of the year, or of one year, are much more severe in their character, more asthenic than those of another year, or of another period of it; and surely, to explain this fact, we invoke no change in the human constitution. We do rather, as Sydenham did, seek the change in something *without* the body, in the variable constitution of the epidemic influence. These facts prove the necessity for our having some clearly understood definition of the term—"change of type;" and also shows the great difficulty of our drawing any just conclusions from the comparison of the results of treatment of disease at different periods.—*London Medical Times and Gazette.*

CÆSAREAN SECTION PERFORMED AFTER DEATH OF THE MOTHER—RECOVERY OF THE CHILD.—*F. R. Owens, M. D.*—August 28th, about 1 o'clock, P. M., I was called to visit Mrs. Q.; 32 years of age; has been married seven years, and has borne three children. Her first pregnancy terminated in a miscarriage at four months. Has had phthisis for the last twelve months, and been gradually sinking.

When I arrived, the friends stated that she had been dead some minutes, and they were anxious about the child. She had been attacked with a severe fit of vomiting, which terminated in large gushes of blood in quick succession, till it had filled a large chamberpot, and she sunk exhausted in a shorter time than it takes to relate it. I felt her wrist and heart, and held a glass to her mouth, but no evidence of vitality existed. She was in the last month of pregnancy, and some women present had felt the child moving. I hurried to my office for instruments, and when I returned and found no change, I immediately performed the Cæsarean section through the linea alba, and extracted from the uterus a male child of eight months, and apparently lifeless. I put it immediately into warm water, inflated the lungs, applied stimulants to the nose, etc., etc. About fifteen minutes after the operation, pulsation commenced, which extended along the cord about eight inches from the abdomen, very feeble at first, but gradually gaining force, and inspiring hope and encouragement to continue our efforts to increase the little life it had. After an hour's persevering effort, it gasped once, then again, and a few minutes after the heaving of the little chest denoted the perfect establishment of the respiratory function. I had it wrapped up warm in cotton, wool and flannel, and kept near the fire. About three hours after the operation, it began to cry pretty strongly. It is now seven weeks old; it nurses and feeds well, and is thriving.

CASUALTIES IN THE RUSSIAN ARMY IN THE CRIMEA.—A St. Petersburgh letter of July 5th, says:—" Fresh and harrowing details have just been published of the casualties suffered by the Russian army during the war in the Crimea. It appears that in the affair of the Tchernaia alone, on the 16th of August, 1855, there were 5048 wounded, amongst whom were 246 officers and seven generals. At Fort Nicholas, where the first hospital for the wounded was organized, as many as 200 amputations were performed in a single day, and one surgeon had often 500 patients to attend to. Most of the men who evacuated the Simpheropol hospital died on the march homewards. These revelations, proving how defective the Russian military administration is, have made a great sensation in St. Petersburgh.

FLUSHING OF THE CHEEK IN PNEUMONIA.—Is it a fact or not, as long ago stated by observers, and lately reässerted by M. Gubler, that the face of a person suffering from pneumonia, flushes on the side corresponding to the lung inflamed? The explanation of the circumstances given by Andral, viz: that the cheek is red because the patient lies on the affected side, and presses the face against the pillow, is rejected by MM. Gubler and Bouillaud, for it may be observed, they say, when the patient lies on his back, and not only in pneumonia, but in tubercular disease of the lungs, and in capillary bronchitis. We ask if the fact is true, for it is one not confirmed by our own clinical experience. We think M. Brown-Séquard is rather hasty in seeking to give an explanation of it, by pointing out the reflex action which he supposes excites the flushing, before it is definitely settled by general observation. On such a point as this, there surely ought to be no difference of opinion.

WIND OF A SHOT.—The following extract from an Indian letter confirms the doubts entertained as to deaths attributed to the "wind of a shot:"—"Brigadier Russel is also about to leave the army, under the advice of a medical board. Never, perhaps, in all the chances of war has there been such an escape as his. A cannon ball cut the gold watch chain at the back of his neck as cleanly as if it had been a pair of nippers, and did him no further injury, except inflicting a shock to his nervous system.—*Med. Times and Gaz.*

TWINS BORN AT AN INTERVAL OF FORTY DAYS.—A case is related in *Ann. Méd.* in which a female, aged 24, healthy, of regular menstruation, and primipara, gave birth, after a short and regular labor, to a fully developed but feeble infant, which died eight days after. The placenta was delivered by natural efforts. The abdomen did not, however, completely subside; active movements were still experienced by the mother, and neither lochial or lacteal secretion was observed. No other particular symptom was manifested until the fortieth day after the birth of the infant, when she gave birth to another, feeble as the first, but evidently having arrived at full maturity. The lochial flow and secretion of milk now appeared.

EFFECTS OF HEATED FURNACES ON THE HEALTH.—The Fire Marshal of New York observes, that "it has been stated to me on reliable grounds, that the health of the children in the public schools is found to suffer from the mode of heating. In many instances the teachers are obliged to allow them to leave school before the regulated hours, in consequence of the violent headaches produced by the closeness and impurity of the atmosphere. It is found from experience that the heated air thrown off from furnaces, and iron surfaces generally, is divested of most of its vital properties, and is apt to produce congestion. In the case of persons of delicate constitutions, this tendency is immediately developed by furnace-heat. This effect has also long since attracted the attention of physicians, and in some families the use of the stove-furnace is positively prohibited." He recommends the hot-water-pipe system.

A FRIEND of ours tells us that a patient of his has communicated to him the following amusing incident respecting the globulistic buffoonery. This patient is now a respectable tradesman. He once was manufacturer of globules to a renowned London Homœoquack, who made enormous profits by the sale of the chests of these minute entities, retailing them to his patients at the sums of from two up to twelve guineas, according to the size of the pockets of the *globe-mouches* he had to deal with. In these chests the infinitesimal *armamentaria* were all duly portioned out and labelled, and specific directions given for their use. The manufacturer aforesaid now positively asserts that there was not one particle of drugs in any of the globules; but that they all, however variously labelled, consisted wholly and entirely of sugar of milk! He adds, that he cut the business because he really felt ashamed of the transactions he found himself engaged in.—*London Medical Times and Gazette.*

DISEASE OF THE MANUFACTURERS OF QUININE.—It appears, from a communication from M. A. Chevalier to the French Academy of Sciences, that the workmen employed in the manufactory of the sulphate of quinine are liable to a peculiar cutaneous affection, which is severe enough to cause them to suspend work for a fortnight, a month, or even altogether. It attacks not only the workmen, but those about the place. No remedy has yet been discovered.

PUNCH ON VACCINATION.—What Jenner said on reading, in Elysium, that complaints had been made of his having a statue in Trafalgar square:

"England, ingratitude still blots
The 'sutcheon of the brave and free,
I saved you many a million spots,
And now you grudge one spot to me."—*Punch.*

ARTIFICIAL RESPIRATION IN CHLOROFORM POISONING.—A case has occurred in Paris in which artificial respiration was resorted to with success, to obviate the effects of an over-inhalation of chloroform.

NEW ORLEANS

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AN ESSAY ON THE CLIMATE AND FEVERS OF THE SOUTH-WESTERN SOUTHERN ATLANTIC, AND GULF STATES.

[Revised and enlarged from the Memphis Medical Recorder.]

By JAMES C. HARRIS, M. D., of Wetumpka, Ala.

[*Concluded from October Number*]

New Orleans.—This city stands in a large bend on the east bank of the Mississippi river, about ninety miles in a direct line from its mouth, in latitude 29° 57′ north, longitude 13° 9′ west. Its boundary in front extends along the river about five miles. In the rear, the corporation extends to Lake Ponchartrain, though the habitations at present only reach in this direction about two miles. The southern front of the low, alluvial surface upon which the city stands is considerably below the annual elevation of the river, with its northern portion below the occasional rising of the lake. On account of this geographical peculiarity the city has to be protected from inundation (and which, we believe, has been effectually accomplished) by the erection of strong and extensive levees.

"The intermediate space between Lake Ponchartrain and the city is a cypress swamp, presenting about midway a considerable elevation, called the *Metairie Ridge.* This vast swamp has undergone a wonderful amelioration within the last twenty-five or thirty years; the part within two miles of the city has been thoroughly drained, by which means a large extent of valuable land has been reclaimed, and which, in a few years, will be covered with buildings and gardens. The surface is gradually becoming more elevated; the streets are annually extended in this direction; and thus the spot which a few years ago was a *pestiferous fen*, will, probably, soon become the abode of a dense and active population."[*]

On account of a counter-current which exists in the Mississippi from some point in the Third to a point in the Second Municipality, there is continually depositing in front of the city an alluvial formation which has received the name of *Batture.* On the subsidence of the river from July to November, this margin, extending along the river for more than three miles, filled with all kinds of organic recrements, and exposed to the action of the sun, must, and does, as a matter of course, emit gases more or less deleterious to health.

Besides these supposed sources of fever, we are informed by Dr. Fenner that there are also within the corporation a considerable number of vacant lots, the surface of many of which are lower than the level of the streets, and contain, during wet weather, stagnant water; stagnant water is also found under many of the houses.[†]

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From the foregoing medico-topographical description, together with its decidely southern latitude, the inhabitants of this city are, as a matter of course, subject to every grade and variety of malarial fever; those residing adjacent to or within the swamp being less liable to yellow fever, and more to intermittents and remittents, than those residing on the opposite or river side.

Fort Livingston.—The island of *Grand Terre*, upon which this fort stands lies at the junction of Barataria Bay with the Gulf of Mexico. The surface of the island rises about two feet above the highest tides of the Gulf, and consists of a dark sand, covered with grass, and overshadowed with small live oaks. From the middle of May, to the middle of August the *land breeze* not unfrequently entirely fails; whilst that from the *sea continues all night.* Captain Barnard, who had been stationed four years on this island, informed Dr. Drake[‡] that although the mean population during this period had been about fifty, that he was unable to recollect of the occurrence amongst them of a single case of either *inter-*

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[*] Fenner's Southern Medical Reports, Vol. I, p. 17.

[†] *Ibid.*, page 63.

[‡] Principal Diseases of the Valley of N. America, p. 86.

mittent or remittent fever; and that the same was true as regarded yellow fever, notwithstanding the usual intercouse had been kept up with New Orleans, when the fever was epidemic there.

Fort Pike.—The island of Petites Coquilles, upon the northern margin of which stands this fort, seems to have been originally formed of a congeries of small shells, with an admixture of argillaceous deposits brought down by Pearl river. It lies thirty-five miles north-east of New Orleans, and between Lakes Ponchartrain and Borgne, exhibiting an area of seven by twelve miles, with an elevation over the Gulf of not more than two feet. Although it is intersected by numerous bayous of salt water, their bottoms never become a source of malaria, as they are under the influence of the tides, and, consequently, are never exposed to the action of the sun. The prevailing winds during the summer are from the Gulf of Mexico, and its soil is said to be fertile.

According to the Army Medical Returns, this post has remained constantly exempt from yellow fever, the average ratio of intermittents being nineteen and that of remittents seven per cent.

The fact that Key West, lying at as great a distance from the main land as Fort Pike, and surrounded by salt water and marshes, has been scourged with yellow fever, inclines us to the opinion that the cause of the remarkable salubrity of this post, compared with Fort Wood, about twelve miles to the north-east, surrounded by marshy low-lands, and under the influence of the immense swamps that skirt the Mississippi, is more the result of the want of a sufficient quantity of organic material undergoing decomposition than that it is encompassed with salt water.

Texas.—This state, like the Carolinas and Georgia, is naturally divided into three regions: the level, the undulating, and the hilly and mountainous districts.

The level region occupies the entire coast, extending from thirty to sixty miles into the interior; the undulating succeeds this, and embraces the whole of the interior and north, and reaches westward to the mountain tract. The mountains and table-lands are generally some two hundred miles distant from the level regions, and cover the greater portion of north-western Texas, on the confines of New Mexico. Hills presenting a low, broken appearance are first found between the Brazos and Colorado, westward of the latter river, and north of the sources of the Nueces and San Antonio; the country is traversed in various directions by outliers of the Rocky Mountains, none of which, however, rise to any very considerable height.

In the level region, the alluvial lands of the several rivers that empty into the gulf are from three to twenty miles in width, and heavily timbered. On many of these rivers are also canebrakes of immense extent. These bottoms, and the alluvial lands generally forming this lower section, are the proper regions for the cultivation of rice and sugar.

In the second division, characterized by high, rolling, verdant prairies, narrow wooded bottoms, beautiful islands of timber, and quick-running streams, the soil is but little inferior to that of the alluvial region just described, but with more varied products. In this region the planter may raise cotton and tobacco, every kind of grain and stock to any extent, and with very little labor.

Along the entire coast, including all the river bottoms, from the Sabine to the Nueces, is almost one entire belt of timber. The eastern section, as far as Red river, although occasionally variegated with beautiful prairies of greater or less extent, is also well timbered with pine, oak, hickory, pecan, cedar, cypress and other forest trees. The orange, lemon and lime, the pineapple and olive, all ripen together, and berries and nuts are abundant.

Florida.—This State, south of 28° north latitude, consists almost entirely of an extensive swamp, known as the *Everglades.* To the north and north-west of this low alluvial tract to the Georgia and Alabama lines, although the surface of the country is rather rugged, it nowhere rises more than two hundred feet above the level of the Atlantic ocean. This north-western portion consists chiefly of an extensive pine forest, interspersed with hummocks, prairies and marshes. The hummocks, with a clay soil mixed with sand, are scattered throughout the country, and vary in extent from a few acres to thousands, and are covered with a fine growth of red, live and water-oaks, dogwood, and pine, and are, when cleared, most excellent land. The prairies are sometimes pretty extensive, stretching miles in length and breadth, and forming most excellent natural pastures. The swamps along the rivers are covered with a heavy growth of timber, and those of the pine barrens, with pine, cypress and cypress knees.

South of the main land a chain of small, rocky islands, known as the Florida Keys, extend to the northward, ending in a cluster of rocks and sand-banks, called the Tortugas. Of these there is but one, in a medical point of view, that we conceive worth notice, and that is Key West, or Thompson's Island. This island, the most southern settlement of the United States, lies about sixty miles south-west of Cap Sable, and between eighty and ninety miles north of the city of Havana, and contained, in 1854, about three thousand inhabitants. It is from seven to eight miles long, with an average breadth of about two miles. As regards its general surface, it is low and level, the south-eastern shore presenting the most elevated point. This ridge, consisting chiefly of sand and shells thrown up by the sea, rises about five feet above high-water mark. In the interior of the island are found many marshes and lagoons, some of which are lower than the surface of the surrounding ocean. These marshy low-lands, with a layer of soil sufficient to support a vegetable growth, and shaded by small trees and shrubs, are covered, in some parts, during the rainy season, with fresh water.

The Military Post of Key West, and which was at one period the principal naval station of the United States for the gulf of Mexico, stands on the north.west end of this island, in latitude 24° 33′ north, longitude 4° 52′ west. Notwithstanding the garrison, in April, 1833, in consequence of sickness, had to evacuate this post and occupy temporarily that of Fort Clinch, we are informed by Dr. Forry, that he was unable, on account of the defectiveness of the Army Medical returns, to determine the precise character of the prevailing diseases, but concludes very *erroneously*, as subsequent experience has shown, that *fevers* of *malarial* origin, were not very prevalent.* Yellow fever prevailed here as an epidemic in 1824, and also again in 1854. During the latter year, we are informed by Assistant Surgeon Simpson, that the fever first made its appearance near the centre of the town, in the immediate vicinity of a large pond, which had been the receptacle for quantities of filth and decayed vegetable matter; and that radiating from this point, it gradually spread over the whole island, attacking indiscriminately *both whites* and *blacks*.†

In here bringing to a close our imperfect Medico-Topographical account of the great region under consideration, we discover if we attempt to reproduce some of its leading physical features, that with a general Southern inclination throughout, its middle and southern portions, but more particularly the latter, present to the action of the sun a great inclined plain, furrowed with sluggish streams, and interspersed with extensive swamps, filled with all kinds of organic materials undergoing decomposition; thus creating and presenting to the mind of the believer in the *malarial* origin of *fever*, not only numerous and extensive laboratories for the manufacture of his favorite agent; but actually offering, as will hereafter be shown, a few facts in elucidation of some of the most obscure and long-disputed points connected with the subject.

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* Forry—Climate of the United States, p. 217.

† Medical Statistics United States, Aug. 1854, p. 323.

The term *climate,* we believe, in an ætiological sense, is generally used to express states of the atmosphere, and these states as consisting in varying quantities of certain of the elements of the air itself;—such as heat, light, electricity, mists, and clouds; dews, rain, hail, frost, snow, and winds; together with the different ascending gaseous exhalations from decomposing matters lodged on the surface of the earth. With this brief definition of the term climate, we are now prepared to examine some of its elements in detail; but before proceeding to do so, will merely observe, that the actual presence of some, and relative proportion of others, are frequently no more the result of a high temperature than of topographical peculiarity.

Temperature.—If we assume the Tropic of Cancer as a base line, and calculate a decrease of 1° 16′ of mean temperature for every degree of latitude North from Key West to the thirtieth parallel, it gives us about 70° as the mean temperature of this parallel; and from this line, allowing a decrease of 1° 30′ for each remaining degree, with a reduction of one degree for every two hundred feet, where the elevations are abrupt, and four hundred on the great inclined plain to the west of the Mississippi, we obtain for the thirty-sixth parallel the temperature of about 60°; and for the entire region under consideration, an annual mean temperature of not far from 66°. The foregoing calculated mean temperatures, it must, however, be borne in mind, are only offered in the absence of more reliable data, as mere doubtful approximations to the truth.

Again, if we take fourteen and fifty-two degrees, the mean annual temperatures of the summits of the Rocky and Appalachian mountain ranges, and compare them with seventy-one and seventy-eight degrees, the mean minimum and maximum temperatures of the Gulf of Mexico and Gulf stream, we at once, through the influence of our winds, discover a sufficient cause for the sudden changes we so frequently experience.

From an examination of the data contained in the accompanying tables, (Abstracts A and B) carefully compiled from every reliable source within our reach, the only *variations* we discover, worth notice, from the general law of a regular decrease of temperature as we ascend on nearly the same lines of longitude from the equator to the pole, and from the level of the sea upward, are those of the summer and monthly temperatures of New Orleans and Fort Gibson; the first the result, perhaps, of the increased temperature of New Orleans over its environs;* and the last, either of inaccurate observations, or a dry, hot wind from its south-west. We also further learn, that as we ascend from the modified atmosphere of the coast, where the seasons almost imperceptibly glide into each other, into the interior, the difference between them regularly increases, and the climate becomes colder and more variable.

From the few thermometrical tables we have had an opportunity of examining upon the subject, it appears that the daily range of the mercury is lower upon the coast, the bays, the interior lakes, and for some distance up the larger streams, than it is at other points within the same parallels, and remote from these; and that consequently the climate *is more or less excessive*, even within the same parallels of latitude, in proportion to elevation and removal beyond their influence.

Winds.—When a portion of the atmosphere is heated, it becomes lighter, and rises, and the colder and denser air around flows toward the base of the rarified columns; in this way, both *local* and *general* winds are created. When they blow from a warmer to a colder region, the temperature of the latter is raised, and the reverse; they are, therefore, no less the cause of a change of temperature, than the effect. For the want of sufficient data, we are unable to determine the relative prevalence or proportion of any of our winds; but conclude, that below the thirty-sixth parallel, the *South-west*, *South-east*, *North-west*, and *North-east* are the most prevalent Of the local, the dry *South-west* is the only one that we will at present notice.

The South-west aud South-east Winds.—The commencement of these moist, warm winds, from the bosom of the Gulf of Mexico and Atlantic

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* The mean annual temperature of London is shown to be 1° 30′ higher than its environs. (See Clark on Climate, page 65.)

Ocean, is frequently attended with storms of thunder and lightning, but generally not of a very violent kind; if the former of these continue for thirty-six or forty-eight hours, clouds high in the atmosphere, moving in the direction of the surface winds, make their appearance, from which, either sooner or later, copious showers of rain descend. The South-west wind, on reaching the Ozark Mountains, in the State of Missouri, from topographical peculiarities, is believed by Dr. Drake† to be deflected from its regular course, and turned on Fort Gibson, mingled with the true South-east.

The North-west and North-east Winds.—These winds, originating around the *Arctic Circle*, are, when compared with our other winds, always *cold.* The first of these, in crossing the Rocky Mountains, from the low temperature of their summits, deposits in the form of *snow* a large portion of its moisture, and then descends and sweeps across the Southern States, as an exceedingly *dry* and *cold wind.* In September and October, although the difference between the temperature of the air of these mountain ranges and that rising from the surface of the Caribbean Sea, the Gulf of Mexico, and Atlantic Ocean, is *considerable,* it does not appear to reach its maximum before February or March, and is then probably more than 100°; during this period, and as the result of these extremes of temperature, the North-west wind around the shores of the Gulf, and as far north as Cape Hatterass, is sometimes converted into a *tempest,* under the name of "*Norther.*" The *North-east,* although a *cold* wind, is always a more *moist* and *warmer* one than the preceding.

The Dry South-west Wind.—This wind, the result of the action of the sea on the *inequalities* of the earth's surface, where it originates, may appear in any region, or at any time, and is always, comparatively, a wind of high temperature; although strictly *local,* it is not without more or less progression, and prevails most in summer and autumn, when the mercury ranges from 90° to 100°. During the month of August, under the influence of this wind, the mercury at Fort Gibson once reached 117°; and the high temperature of June, July and August, at Fort King, in the interior of Florida, and the military posts along the Southwestern border of Texas, is probably to some extent the result of a similar cause.

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†Principal Diseases of North America, page 575.

MEAN TEMPERATURE OF THE YEAR AND OF THE SEASONS.—[Abstract A.]

Places of Observation.	Latitude.	Longitude.	Feet above the Ocean.	Mean Annual Temperature.	Temperature of the Seasons.				Difference between Winter and Summer.
					Winter.	Spring.	Summer.	Autumn.	
	°	°		°	°	°	°	°	°
Havana, Cuba	23.10	4.50	Gulf.	77.34	72.68	76.28	82.32	78.08	9.64
Key West, Florida	24.23	4.52	12 feet.	76.48	70.05	76.04	81.39	76.96	11.34
New Orleans, Louisiana	29.57	13.09	14 feet.	70.06	55.23	79.90	83.04	70.78	27.81
Natchez, Mississippi	31.34		264 feet.	66.86	52.18	67.80	80.96	67.06	28.78
Memphis, Tennessee	35.08	18.00	400 feet.	64.10	44.00	60.20	80.20	62.00	36.20
Fort Gibson, Indian Territory	35.47		600 feet.	62.90	42.50	61.26	79.17	61.53	36.67
Mobile, Alabama	30.42		12 feet.	70.29	57.26	70.13	82.75	70.01	25.49
Wetumpka, Alabama	32.30			65.50	51.57	68.12	80.44	67.13	28.87
Huntsville, Alabama	34.45		600 feet.	59.38	42.15	59.96	75.62	59.79	33.47
Oglethorpe Bar, Georgia	32.05		40 feet.	67.44	54.06	67.08	80.70	67.94	26.64
Augusta, Georgia	33.27	4.32	400 feet.	64.00					
Athens, Georgia	33.58	5.34	782 feet.						
Fort Moultrie, South Carolina	32.45		25 feet.	67.00	51.52	67.02	81.56	67.89	30.04

MEAN TEMPERATURE OF THE MONTHS.—[ABSTRACT B.]

Places of Observation.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Names of hottest and coldest months.	Difference between hottest and coldest months.
	°	°	°	°	°	°	°	°	°	°	°	°		°
Havana……	69.98	71.96	75.74	78.98	83.58	84.12	84.30	85.84	83.04	79.52	75.56	71.78	Aug. and January…	…15.86
Key West……	69.46	70.26	73.83	75.35	78.98	81.03	82.53	81.98	81.19	77.21	74.22	70.52	July and January…	…13.09
New Orleans……	56.42	59.17	68.60	73.71	78.96	83.09	83.90	83.27	80.23	71.69	60.81	54.38	July and December.	…29.52
Natchez……	52.27	54.51	59.66	69.85	74.52	80.71	81.32	80.85	77.18	67.01	56.98	49 73	July and December.	…31.59
Memphis……	42.00	46.00	51.00	62.00	68.00	82 00	82.00	77.00	71.00	57.00	58.00	44.00	July and January…	…40.00
Fort Gibson……	45.47	41.25	53.51	61.28	72.69	78.65	81.49	83.28	74.61	65.95	54.12	46.20	Aug. and February.	…42.03
Mobile……	57.59	57.90	62.40	70.64	77.45	81.64	83.68	82.91	79.99	69.55	62.59	56.30	July and December.	…27.38
Wetumpka……	50.58	56.18	60.78	70.49	73.28	80.81	81.30	80.81	75.52	69.46	61.02	50.36	July and December.	…30.94
Huntsville……	42.06	42.59	51.34	61.30	67.25	74.23	76.39	76.24	70.15	59.50	49.74	41.81	July and December.	…34.58
Oglethorpe Bar……	57.29	53.17	59.90	66.90	73.47	78.96	82.20	81.13	81.83	63.35	59.90	53.48	July and February.	…29.03
Fort Moultrie……	50.08	51.71	58.12	66.95	75.98	79.85	83.29	81.53	75.15	66.60	61.92	52.76	July and January…	…33.21

Dew Point.—By this phrase we understand that degree of temperature at which moisture begins to be deposited from the air—for example: if we submit two volumes of atmosphere at the temperature of 100°, to a cooling process, and one of them commences depositing dew at a reduction of 20°, and the other at 30°, the one from which dew is first deposited contains the most moisture, and is said to have the *higher dew point.* The complement of the dew point is the range through which the mercury falls before dew appears; when this point is reduced to nothing, and coïncides with the temperature of the air, *saturation* exists and *evaporation* ceases.

In the limits assigned to this essay, the three great regions believed to differ most in their absolute atmospheric vapor and *dew point*, are, first, its Sea and Gulf coast, and the trough of the Mississippi; and secondly and thirdly, the regions to the north of these, and to the east and west of the *Mississippi.*

Regions of the Coast and of the Mississippi.—The atmosphere of this region, from the Rio Grande to the Cape Fear river, and for some distance up the Mississippi, with a mean annual temperature of nearly 70°, has constantly, from the great extent of watery surface over which it rests, a high *dew point*, with a small complement. In support of this opinion, at least for a portion of the summer and fall months, the following meteorological table is submitted:

Places of Observation.	Year.	Months.	Therm.	Hygrom.	Diff. bet. Hy. & Therm.	Saturations.	Authority.
			°	°	°	°	
New Orleans	1849	July,	78.74	75.53	3.21	16	Fenner's Reports page 101.
		Aug.	80.96	76.75	4.21	13	
		Sept.	78.44	73.78	4.66	11	
Fort Moultrie	1849	July,	78.89	76.77	2.12	...	Med. Stat. U. S. A. pp. 237 & 525.
		Aug.	81.25	77.69	3.56	..	
		Sept.	75.29	71.70	3.59	...	

Regions to the East and West of the Mississippi.—The atmosphere of these regions, with a mean summer temperature of nearly 80°, like that of the coast and the Mississppi, would be, under favorable circumstances, capable of sustaining a high dew point; but on account of their elevation, and remoteness from the Ocean and Gulf, no less than the arid and heated regions lying to the west and south-west of the western region, we conclude their *dew point* is frequently *low*, particularly around the northern and western borders.

If we admit, and which under the circumstances we are bound to do, that the entire exemption of Fort Livingston from intermittent and remittent fever, clearly proves that neither a high temperature, a high dew point, nor the near approximation of the latter to the temperature, are sufficient either singly or combined to produce any of the *varieties of malarial fever*, we are forced either to remain in ignorance of their *efficient* cause, or seek it outside of these, *or any other mere atmospheric change* of the locality where they occur; but that there does exist, as the remote cause of fever, *a gaseous poison*, *an intangible element*, the result of decomposing organic remains, we think clear, from the fact that wherever we find a large amount of vegetable matter in connection with certain degrees of heat and proportions of moisture, there we meet with febrile diseases; to this effect we have the concurrent testimony of the profession everywhere, and we would as soon think of denying its existence on account of the experimental failures of Broschi and Moschati, made in the most pestilential regions of the Campagna di Roma, and on the air of some insalubrious rice fields in Tuscany, for the purpose of discovering its presence, as we would to conclude there are no *roses*, because chemistry has thus for failed to furnish us with means of detecting *odours* in the air.

Believing there is no intelligent Southern physician who has taken the trouble to examine the subject, that pretends to deny the *malarial* origin of intermittent fever, we conclude if anything more were necessary to show a similar origin of the other varieties than what is already before the profession, that the history given us by Doctor Heustis of the bilious-remittent, or yellow fever, that prevailed in Cahawba in the summer and fall of 1821, before steamboat navigation had been introduced on the Alabama river; and that of Doctor Gantt, of its occurrence in the Pleasant Valley during the fall of 1824, and when there was no yellow fever in Mobile, are conclusive, and ought forever to set the subject at rest.

Having in the preceding pages during the summer and fall months, shown in some of the cities and towns around the coast, and for some distance up the valleys of some of the principal streams, the occasional prevalence of all the *varieties of malarial fever*, and in others, the more elevated and dryer regions of the interior, the continued absence of at least one, if not more of the higher grades; we will now proceed to develope some of the laws upon which this relative difference in prevalence appears to depend, and then dismiss this part of the subject.

From as careful an examination of the history of *malarial fever* as we have been able to give the subject, it appears that the elements necessary for the production of the milder grades, are a mean monthly temperature of about 60°, in connection with certain proportions of moisture and vegetable matter; and for the higher, an increase of the monthly temperature to 80°, with a high dew point and small complement; and althouh the former is said to cease prevailing on a reduction of the temperature to 50°, and the latter at 70°, we know that where the heat of the preceding nine months has been greatly over these points, their respective *types* continue to show themselves sporadically throughout the winter months.

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Continued Typhoid Fever.—As the records of medicine are full of proof in support of the fact that *continued fever*, originating in southern latitudes not unfrequently runs into, and assumes a remittent or even *intermittent type*; and as remittent fever sometimes during its progress, either from increased *congestion*, or the supervention of inflammation in some one or more of the principal organs of the body, looses its *paroxysmal features*, and degenerates into a low grade of febrile action; together with the great similarity of the symptoms, tendencies, terminations, and post mortem lesions of the former with those of the *continued stage of remittent fever*, we infer that they are essentially and to all intents and purposes the same. Nay, more, that the high temperature, high dew point, and sudden changes of our climate, predispose strongly to *viceral engorgements;* and that these obscure congestions thus created, sometimes from the commencement in those of feeble constitution, or long resident among us, entirely *prevent* or *mask* the remissional features of our fevers, is a fact, we presume no one at all familiar with the endemic influences of our climate will pretend to deny; but that either of these varieties of fever, are ever, within the limits assigned to this essay, so far changed as to present us with anything more than a *continued fever*, the result of these causes, and with affinities and resemblances much stronger marked in favor of our *autumnal remittents*, than their supposed primary type as met with in the cities of Glasgow and Dublin, we think the conditions under which they originate and prevail *utterly forbid.*

Beleiving that the *typhoid* fever described by Louis, and the *typhus* fever, not only of the British isles, but also of the rest of Continental Europe, *are the same disease;* and that the theatre of their origin and greatest prevelance, is in countries and localities where the malarial element has never, within the memory of man, been present in sufficient quantity to produce even the most mild and simple form of *periodical fever;* together with the fact, that under our genial sun and democratic institutions, when imported in the persons of emigrants and landed on our shores, they fail to propagate themselves and soon die out; we conclude that the term *typhoid* (which means something belonging to typhus) cannot with any degree of propriety be applied to any other period of either our pure or modified forms of *fever* than their *latter stages.*

In here dismissing for the present this part of our subject, we embrace the opportunity of saying to Professor Dr. J. Cain, of the Charleston Summer Medical School, that in the notice as editor of the Charleston Medical Journal and Review,* he has done us the honor to make of our paper on the non-existence of typhoid fever in the south,† that he entirely misunderstood our position. In that paper we were not denying the *sporadic or even sub epidemic* occurrence throughout the Southern States, of *tolerably well marked continued fever;* but merely when it did occur, particularly during the summer and fall months, that we had ever met with in any one case enough, or in violence sufficient of *typhoid symptoms*, to make us so far forget ourselves as to pronounce the disease *typhus fever*, and treat it accordingly

Symptoms:—An attack of this variety of fever most generally makes its appearance in a very general manner; the patient after complaining for sometime of more or less languor, loss of appetite, headache, and pains in the limbs and back, is seized with slight coldness of the extremities, or frequently recurring chilly sensations, followed by a *mild grade of fever*, in which the remissions and exacerbations, if noticeable at all, are irregular or exceedingly obscure, attended with thirst, quick tense pulse, sallow or darkly flushed cheeks, and high colored urine. At an early period of the attack there is more or less dullness of the mind, amounting in some cases to mild delirium, attended frequently with slight subsultus tendinum, or occasional *epistaxis.* The tongue, at first moist, is frequently red around the edges, and covered with a white or brown fur; as the disease progresses it gradually becomes dry, cracked, and finally not unfrequently presents a clean appearance. The bowels, although frequently regular or costive, sometimes become loose; the stools being watery, of a dark yellow, or muddy appearance, and very offensive. After the persistence of these symptoms for eight or ten days, if the disease does not clearly assume the *remissional type*, or terminate in death within the same period, from violent engorgement or inflammation, it gradually looses its acuteness, and begins to present the symptoms already detailed as characteristic of the *typhoid stage of remittent fever*.

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* See November No., 1850, page 823.

† New Orleans Medical and Surgical Journal, May No., 1850, page 712.

Treatment.—If we grant that the long and continued operation of the remote cause is such as to produce in the system the pathological condition that gives to this variety of fever, from the commencement, the *continued type*, as a matter of course the first and most clear indication to be fulfilled, in its proper treatment, is to restore the secretions, and break up and remove the local hyperæmias, that probably, under the circumstances, are the proximate cause of the *particular type it presents.* For this purpose we give eight or ten grains of calomel in combination with a quarter of a grain of the acetate of morphine; this quantity of calomel and morphine we repeat about every four hours, according to the amount of hepatic derangement present, until two or three portions are given. At the end of twelve or fifteen hours after the administration of the last one of these powders, if the bowels are not sufficiently moved, we give a dose of castor oil. After the operation of the oil we then commence and give five or six grains of quinine at mid-night, at day light, and at ten o'clock the next morning; this we repeat the next night and succeeding morning as above. If we now find the tongue improving, and the fever abating, which we frequently do, we complete the cure by the continuance of the quinine a few days longer; but, on the contrary, should the tongue remain furred, and the febrile symptoms but little, or not at all abated, we repeat the calomel and morphine powders again, at perhaps a little longer interval between each dose, taking care not to carry them off after the administration of the last one, with oil, or any other laxative, sooner than eighteen or twenty hours. If the calomel thus given brings off free, consistent, bilious [tany] discharges, we can then generally within the next twenty-four hours bring the system under the influence of the quinine, and *abate* the fever.

For the management of those cases in which the bowels are rather loose from the commencement, or become so during the progress of the disease, we will have to refer to our plan of treating the typhoid stage of remittent fever, contained in the May No., 1852, page 742, of the New Orleans Medical and Surgical Journal.

The fact that our distinguished friend Dr. Boling entirely failed to cure the mildest case of *continued fever*, by the successive daily administration of from thirty-six to forty grains of quinine; no less than that our patients have generally and within a very reasonable length of time thoroughly recovered; has caused us, as yet, to feel no disposition whatever to change our plan of treatment; but knowing in some cases, and under certain circumstances, how difficult it is, from idiosyncrasy, or the former abuse of mercury, to administer calomel and opium in this variety of fever, so as to procure its proper curative effect, without occasionally ptyalizing our patient, we recommend to those of our professional brethren, who believe that in the early stages of *continued fever*, if febrile excitement be *quenched* and *kept under*, by the liberal administration of quinine, that the *unaided powers* of the *system* are competent to a *cure*, a trial of the thirty grain doses of quinine in combination with one or two grains of opium, as recommended and practiced by Dr. Fenner, of New Orleans.*

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MEDICAL NOTES AND REFLECTIONS.

[*Continued from page* 746, *Vol.* 4.]

By Dr. J. E. Thompson, of Arkansas.

Abdominal Abscess. Operation—Five Gallons of Pus Discharged—Recovery.

On the 21st of September, 1857, I was called to see Jack, a negro servant, aged about forty, of strumous constitution, who was laboring under a large tumor in the left iliac region above the pubis. By manipulation, the tumor appeared hard, prominent, and elongated from above downwards—except a spot in the centre about the size of the bottom of a tea-cup, which was soft and fluctuated on pressure. Externally, the tumor measured eight inches in its longitudinal diameter, and 5½ inches in its transverse diameter. The general condition of the patient was anæmic; pulse 100 and weak; skin hot and dry; tongue normal; bowels constipated; urine scanty and highly colored; no appetite; thirst urgent; countenance anxious; cannot sleep from pain in the region of the tumor. I determined on opening the abscess, and accordingly made a deep longitudinal inscision through the fascia transversalis to the left of the epigastric artery, two inches in length. I took care not to wound the peritoneum, for the fascia that covered the peritoneum was only involved in the abscess. The patient was then turned on his left side on the edge of the bed to facilitate the discharge of pus. In two hours *three measured gallons* of pus came away, of a dark-green color, and exceedingly fœtid, similar to that of rotten eggs. The patient expressed himself greatly relieved after the evacuation of this amount of pus. The tumor now became very soft, and sunk considerably. I put him on nutritious diet, muriated tincture of iron and quinine as a tonic; his bowels were regulated by enemata of castor oil, spts. turpentine and slippery elm water, with fomentations to his abdomen. During the course of two weeks *two gallons* more of pus came away, of a lighter color and not so fœtid. The wound was not allowed to heal for four weeks after the operation, when it readily healed; after which, the abdomen presented nothing abnormal by manipulation, nor did it cause him any pain whatever. His bowels became regular and appetite good; and at the end of six weeks he was able to ride a distance of three miles to my office, and do little jobs on the plantation.

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* See Fenner on Quin., New Orleans Med. Jour., 1852, page 318.

Reflections.—This negro had had a "sore leg" for about thirty years, but was otherwise healthy, and an able "cotton hand." About the first of April, '57, the *sore* on his leg (the left one) healed up, which the negro regarded as a great triumph, having accomplished it himself, as he thought. About the first of May thereafter, he began to complain of occasional severe pains in his abdomen. It, however, did not trouble him much, till sometime in August; when his complaints grew so clamorous that the overseer examined him to see whether he was "sick, or possuming," and discovered the tumor for the first time. The swelling was kept painted for three or four weeks with tincture of iodine and mercury, given almost daily, prior to my seeing him. The pathological cause of this abscess, was, no doubt, due to a scrofulous diathesis, which is quite common in our negroes in the South—originating from that "sore leg" which had been suffered to heal, thus checking an accustomed discharge of pus, which, from long standing, had become constitutional. As the channel through which this *waste* passed off, was now closed, it collected in the abdominal fascia, and there formed pus cells, from which the enormous quantity of *five gallons of pus was discharged.* So soon as I learned the fact of his having had an old ulcer on his leg, measures were taken to renew the discharge of pus; and as soon as it began to run freely, the pus ceased to flow from the wound in the abdomen. It is a fact worthy of note, that the healing of these indolent ulcers of long standing, which have been discharging quantities of pus for years, often produces a metastasis of the disease to some vital organ, such as the lungs, liver, etc., which would finally prove destructive to life unless the disease was speedily transferred to its original seat, and the discharge of pus, which seems to be a *waste,* which the system has become accustomed to throw out, kept up as long as life lasts. I have treated several cases where metastasis took place, and the disease seated itself in the pulmonary parenchyma, with all the characteristic phenomena of chronic pneumonia or primary phthisis, when a total subsidence of all these characteristics took place, as soon as a discharge of pus was established from the old ulcer on the leg.

Placenta Prævia, with Cervical Attachment of the Placenta.

Mrs. D., æt. 46, sanguineous constitution; the mother of twelve healthy children, and seven months advanced in her thirteenth pregnancy, was attacked on the 1st of October, 1857, with profuse uterine hæmorrhage, which was only temporarily arrested by the attending physician. More or less blood, sometimes florid, and at others, grumous, escaped from the uterus up to the 20th of October, when the hæmorrhage recurred, and I was sent for, because the other Physician "could not stop the bleeding" as they supposed. The *os* was rigid—in fact, nothing unusual was noticed except the placental *souffle* being distinctly audible in the *cervical uterine region;* the fœtal heart could be heard, though feeble; the abdomen appeared flatter than usual. I endeavored to explain to the lady the cause of the hæmorrhage, and the probable result of her case, *i. e.* that the hæmorrhage could not be arrested until labor came on, or was brought on prematurely. I gave her sugar of lead, and opium, plugged the vagina, etc., but only succeeded in arresting it partially, as was the case before. After this attack, she did tolerably well, except an occasional flow of blood, until the morning of the 3d of November, when the hæmorrhage recurred with redoubled fearfulness. I now found my patient with a small weak pulse and faint from loss of blood. After checking the hæmorrhage a little, I searched for the fœtal heart and placental *souffle,* but could find neither. Confident that the fœtus was dead, and from a sense of the extreme danger of my patient's condition, I determined to rupture the membranes and bring on labor; which being done, the uterus contracted, and arrested the flooding; but when it relaxed, the hæmorrhage became so profuse that I became apprehensive that she would die before I could effect the expulsion of the fœtus. Fortunately for myself, but infinitely more so for my patient, the hæmorrhage began to subside.

The uterus now refused to act, and after my patient had rallied a little, I ordered twenty grains of powdered ergot every half hour until the uterus should act. After she had taken *one hundred* grains of ergot, pains came on; after each pain, the hæmorrhage was very profuse. On examination per vaginam, I found the *placenta lying over the mouth of the uterus*; and when I pushed it to one side, the right foot and left arm presented. The hæmorrhage now ceased. I pushed up the arm and drew down the left leg, and both fœtus and placenta were expelled. The uterus soon contracted to the size of one's fist. The fœtus apparently had been dead a week or more. The placenta was considerably larger than usual.

November 4th. Patient quite pert, yet weak; as her bowels had not moved for about twelve hours I ordered two ounces of castor oil, with gruel and Hoffman's anodyne.

5th. No action from the bowels yet; patient looks well; pulse 80; but no fever nor unnatural soreness of the abdomen. Took some tea and toast for breakfast. To have two doses of oil to-day, and two injections per rectum if the bowels do not act.

6th. Same as on yesterday. Gave twenty grains of calomel, to be followed in five or six hours (if it does not act) by oil and glysters.

7th. Bowels not moved; patient says she feels quite comfortable and and has appetite enough. Gave croton oil—two drops every two hours, till she took eight drops, followed by oil and glysters.

8th. No action yet; says she felt some griping in her bowels last night; but otherwise she is improving; ordered an enema of castor oil, common salt, and starch water every three hours until her bowels should act.

14th. 8 o'clock, P. M. Bowels did not act till 7 o'clock, P. M. to-day. Improving rapidly; can sit up in the bed.

Reflections.—This lady went from the evening of the 3d to the evening of the 14th, without an action from her bowels, notwithstanding she took over a pint of castor oil, twenty grains of calomel, eight drops of croton oil, besides about thirty glysters!—all, without causing even nausea or but little griping in the bowels. It is remarkable indeed that she should have *no fever, nor sickness* of the stomach, and not even any *unnatural tenderness of the abdomen,* and yet her bowels so *obstinately constipated for eleven days.* Neither did the mercury produce any constitutional effects upon her system. This case presents several points of interest, the aggregate of which are indeed peculiarly interesting, if not almost with few parallels. The points to which I allude are, first, cervical attachment of the placenta; second, imflammation of the placenta over the mouth of the uterus; third, cessation of the hæmorrhage as soon as the placenta became detached; fourth, the quantity of ergot taken, i. e. *one hundred grains;** fifth, obstinate constipation for eleven days—want of fever and tenderness of the bowels—quantity of purgatives taken, without producing a single bad symptom. These constitute the peculiarities of the case, and, I repeat, as a combination, as far as my knowledge extends, they stand alone. In the language of Dr. Robert Barnes, of London, "It would indeed be a triumph exceeding, because superseding all therapeutical improvements, if we could arrive at such a knowledge of the etiology of placenta prævia as would enable us to obviate the occurrence of this abnormity. That prophylaxis will ever supersede therapeutics in this matter can, however, scarcely be anticipated. Still it is eminently desirable never to lose sight of the fundamental question of etiology in our inquiries." (London Lancet for 1857, p. 283.) The reasons are obvious enough why the placenta is commonly developed at the fundus of the uterus; but why it is that the placental mass is so frequently developed at the cervix uteri, is a question that physiologists of late have puzzled their brains about considerably, and yet are scarcely able to give a satisfactory answer. We know that when the ovum emerges from the orifice of the Fallopian tube into the uterine cavity, it is caught in a fold of the vascular, thickened, and coagulated lining mucous membrane of the uterus in the immediate vicinity of its entrance. There it is fixed, and is developed; and that part of the chorion which lies in opposition to the decidua vera is most disposed to become developed into placenta, while the villi of the remaining part sometimes disappear altogether, or at least, do not assume the placental character; though, at other times, even at the end of gestation, it is not uncommon to find the villi of the chorion not entirely disappeared from any part, but there yet remains numerous small vessels all over its surface immediately in juxtaposition with the vessels of the decidua, which seem analagous to the already developed placenta. In this case the placenta was thinner than natural; consequently, this superficial development was insufficient for the nutrition of the fœtus; more and more vessels were necessarily called for, until they had formed a placenta of sufficient extent for the proper nutrition of the fœtus; hence the placenta was of unnatural largeness, yet wanting in thickness. Owing to the narrowing of the uterus towards its neck, it is highly probable that sufficient placenta cannot easily be developed in a case of prævial attachment without spreading beyond the usual limits, or acquiring very unusual thickness. Thus Levret drew especial attention to the shape of the placenta in prævial attachments. Cases of placenta prævia are of sufficient proof to show that the uterine mucous membrane can at any part furnish the proper uterine element of placenta. Besides the example I have just described, further proof may be drawn from the observation of cases of extra-uterine pregnancy; where the ovum is lodged in an abnormal situation, not adapted to supply sufficient nutriment for the embryo, it is not uncommon to find the greater part of the surface of the chorion developed into placenta. There are also the *placenta spuria* and the *placenta succenturiata*, which also prove that any part of the chorion may be developed into placenta, according to the requirements of the embryo. But, are there any signs by which we may discern the existence of placenta prævia prior to the occurrence of labor? Levret enumerates the following signs, to-wit: "The belly of the patient is not pointed or rounded as a ball, but somewhat flattened. It appears as if it were divided into two parts, as in twin-pregnancy; but that which essentially distinguishes the first case from the second is, that the division is not found exactly in the middle, nor according to the vertical line of the body, but more on one side than the other, and a little obliquely. Moreover, if we question the woman, she will admit that in the first months of pregnancy she felt a swelling with hardness, in one or the other side of her belly. This hardness had gone on increasing, and had never changed sides; and that this side is the most painful spot of the whole belly, and that in which she feels the movements of her child the least." Gendrin gives

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* The ergot was of good quality, as I have used some of the same before, when one or two doses produced the desired effect.

two signs: "first, a pulsation may be felt at the os uteri, not synchronous with the mother's pulse; and secondly, that owing to the interposition of the placenta *ballottement* cannot be performed." Dr. Robert Barnes says "the lower segment of the uterus is generally larger, softer, more fleshy than in ordinary gestation. The presenting part of the child either cannot be made out at all, or very indistinctly; and occasionally, when the os uteri will admit the finger, the quaggy placental mass may be felt. Somtimes a persistent dragging pain in a particular part has led to a stethoscopic examination, when the cervical attachment of the placenta has been accurately determined." (Lancet for October, 1857, p. 284.) Dr. Cohen, of Hamburg, says in a paper on this subject, that the "side of the uterus to which the placenta grows is more swollen. In by far the greater number of cases this side is *painful,* and that the smaller part of the placenta is attached either to the *right* or *left* side; so seldom to the fore or after side, that for diagnosis it is enough to examine the right and left sides." Moreau remarks: "only one phenomenon may cause a suspicion of the cervical insertion of the placenta: this is the epoch at which hæmorrhages occur. Generally, they take place early in pregnancy, but they never arise before the moment when the neck of the organ begins to unfold itself. Most frequently, it is from the sixth to the seventh month."

The indications in diagnosis may be summed up as follows:

1. *Previous History.*—Abortions before; disease of the placenta; still-born children, and placenta prævia in former pregnancies, should put us on our guard.

2. *General signs.*—Flattening of the abdomen; division of the abdominal tumor; swelling; pain in one side of the pelvis; hæmorrhages—when it occurs, etc.

3. *Physical signs.*—The placental *souffle* being audible in the *cervical uterine region* instead of in the region of the fundus; *ballottement* being wanting, or performed with difficulty; difficulty in determining the presentation of the fœtus, or feeling the quaggy placental mass through the os uteri with the finger. By a careful observation of the above phenomena we may pretty accurately determine whether the case be placenta prævia or not; and we should be very cautious in making up our diagnosis, for acting from an incorrect one might subject us to the mortification of sacrificing the life of our patient at the expense of our rashness and consummate ignorance. Without a correct diagnosis in the case of Mrs. D., all

efforts to have saved her would have proved abortive; as she had about one month to go yet, before the full term of utero-gestation would have been completed. By my not knowing the true cause of the hæmorrhage, I might have been led to let nature take her course—and good heavens! what would have been the result. Of course, my patient would have fallen a sacrifice to hæmorrhage, the result of my ignorance.

VERATRUM VIRIDE IN YELLOW FEVER.

By J. S. Duval, M. D.

Houston, Texas, Nov. 2d, 1858.

E. D. Fenner, M. D.—Dear Sir: I have to-day been shown (by Dr. McCraven) an article of yours on a stray leaf of the *New Orleans Medical News and Hospital Gazette,* entitled "New Remedies in Yellow Fever; Chlorine and Veratrum Viride." As you have requested physicians, residing at places where yellow fever is now prevailing, to let you know the result of their observations, I embrace this opportunity of furnishing my modicum of experience.

While residing in Nashville, Tenn., in 1853, my attention was directed to the treatment of typhoid fever, with veratrum viride, by Prof. W. K. Bowling. After noting the beneficial results from the use of this potent remedy in that and other fevers, I adopted it as a constant companion, and when I came to Texas, in 1854, supplied myself plentifully with Norwood's Tincture of veratrum viride. For the past five years I have used it in subduing every case of fever I have been called on to treat, by whatever name or grade, where the pulse indicated much febrile action. The result of my experience with the remedy in subduing febrile action in cases of wounds I reported in the *Nashville Medical and Surgical Journal*, in 1855. Previous to my locating in this place, in 1857, I had only two or three opportunities for testing its availability in yellow fever, and not until this Fall have I been able to decide the point, save by reasoning from analogy. Unfortunately my first case here, which occured the 4th of October, was in the person of a very delicate young man who had been taking patent liver pills for six days before he was seized by yellow fever. At no time during his illness would his pulse admit of its use. He died. On the 5th of October I was called to see Mr. T——, a very athletic man, whose weight is about fourteen stone. He was taken about 6 o'clock, P. M., and I saw him at 9 o'clock, three hours after his attack. He had taken a foot-bath and a dose of oil previous to my arrival, but his fever was high and still rising—pulse quick, full and strong; his skin was very active, and he was drinking orange-leaf tea. I gave him eight drops of veratrum, and left a solution: six drops to each tea-spoonfull of water, to be given every three hours in tea-spoonfull doses. I visited him at 4 o'clock, A. M., October 6th; found him without fever; his bowels had been thoroughly acted on by the oil, and he had perspired profusely all night; at 11 o'clock, A. M., his fever came up again, and I repeated the veratrum at intervals of three hours. By 4 o'clock, P. M., he was free of fever, and had no more from that time, during his illness. In six days he was up about his room, and on the ninth came on the street to attend to business.

I have treated eighteen cases up to date, all of them with the veratrum. I have lost but two cases, one of which did not take veratrum at all, and the other, a German woman (but three months in America, and but three weeks in this place), I am convinced had the fever a day or two before I was called to her.

I have not used the chlorine mixture. In conclusion, I will add I am satisfied that veratrum viride is worth all other remedies in yellow fever, but it must be used in the first stage or first day of the disease. Especially with children is it available. If it is any advantage to cut short the stage of febrile action, then veratrum is the remedy; it will do it with certainty and safety, if properly administered and cautiously guarded.

I generally give the following impromptu prescription *which I prepare myself:*

℞ - Water, six tea-spoonsfull.
Veratrum viride, drops 36.
Camph. Tinct. Opii ½ tea-spoonfull, mix.

Of this I order a tea-spoonfull every three, four or six hours, *pro re nata.* When I can do it I visit the patient and give the medicine myself; at any rate I visit him before the remedy has had time to do any damage by its depressing power. I have never known of any ill effects produced by it, and I have used it in an extensive country practice, when it was impossible for me to remain and watch its effect. The paregoric obviates all tendency to emesis.

[N. B.—It becomes our painful duty to announce, that, since the above interesting letter was put into the hands of the printer, we have received the sad intelligence that the writer is no more. My correspondent, Dr. McCraven, of Houston, in a letter to me, of the 16th November, says: "This season, my last case, (and unfortunately fatal) was my valued friend, and confrere, Dr. J. S. Duval—a most promising young physician, of fine attainments and fine prospects. He was about the last, if not the very last victim of the season. I have no heart to write more at present."

We had no personal acquaintance with Dr. Duval, yet we cannot but feel assured that if he had lived, he would have done honor to the profession. Like a true medical hero, he has fallen at the post of duty, fighting against the invisible destroyer, who gets imperishable fame for his conquests, but sheds no honor upon his conquerors. Had Duval fallen on the bloody battle-field, perhaps a monument would be erected to his memory; but as he has sacrificed his life to save his fellow-beings from destruction, he must be doomed to oblivion. Alas! for the justice of mankind, which embalms its *destroyers* and crucifies its *benefactors*!!

E. D. F.]

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INTRODUCTORY LECTURE,

Delivered before the Class of the New Orleans School of Medicine, Session 1858–'59.

By I. L. Crawcour, M. D., Professor of Chemistry, New Orleans School of Medicine.

Gentlemen:—We have met to-day, to inaugurate for the third time our Medical session. Many of you have come from distant States to take your seats for the first time upon the benches of a medical theatre, others present the familiar faces which had almost daguerreoptyped themselves upon our mental vision during our previous intercourse; all of you do we greet as friends, as fellow co-laborers in the paths of science, as comrades in the field of common usefulness.

To you, however, who for the first time have adventured into the precincts of Physic, I would fain propound a simple question: Have you thoroughly and well considered what you have undertaken? Have you well weighed the responsibilities you are about to assume? Have you seriously asked yourselves what the profession of Medicine requires of its votaries? Think for an instant what you are about to become; you are called Physicians, Doctors. As Physicians you are the interpreters of nature, the expounders of all that surrounds you; as Doctors you are the *Doctores et Docti*, the teachers and the taught, the Apostle and the Neophyte, for as Plato justly observes, "The Physician must learn something from, and as far as he can, must teach something to the sick whom he attends;" and more than all this you are the healers of men. Reflect for a moment upon the Atlantean weight you have to bear, all the multitudinous ills which afflict mankind will be displayed before you, all the intricate machinery of the human frame will be bare before your eyes, and you will have to repair, to mend, to regulate the machinery of its wheels. Humanity in its most hideous phases will be your daily companion; moral and physical ills will beset you at every step; vice and misery will stare you constantly in the face, and ingratitude will be your most frequent reward. A self-denial and a disregard of the comforts of life, worthy an ancient stoic, must become a habit of daily life, and an equanimity of mind, a most unruffled composure in the most adverse and trying circumstances, must become an integral part of your mental constitution.

Pause while you may upon the threshold; and if any among you are daunted by what you have to undergo, depart at once and seek some easier path to wealth and honor. The forum and the camp, and the thousand avenues of mercantile life are open to you, each offering a surer and an easier road to wealth, if such be your object. Saddest of all trades, but noblest of all professions, medicine promises you no riches, can provide you with but few rewards, but it can render you useful to your fellow-men, can teach you to allay the anguish of your suffering fellow-creature, and more than all, will aid you, in what should be the search of every man—the knowledge of truth. "Whoso lifts my veil shall see the truth," was the inscription on the veiled statue at Sàis. The art of medicine, the science of nature, will lift that veil, and doubt not that the truth will shine upon the brow of its sincere votary. If, therefore, in despite of the obstacles in your path, notwithstanding the ruggedness of the road you have to traverse, you still will pursue the noble career which lies before you, then do we cheerfully welcome you among us, and rest assured, that as far as we can, we will endeavor to lighten the labor you have to undergo, and assist you in climbing the lofty peak on which the temple of Æsculapius is reared.

First among the blessings given by the Deity to man, most prominent among the benefits which man can confer upon his fellow-man, the art of healing is the one which most assimilates man to his Maker, for next to creation is the preservation of the works of the Creator. Preëminent in its usefulness, in its antiquity, medicine knits us together with a strong fraternal bond, it reminds us of our common origin; in sickness and suffering all are equal, the master and the slave, noble and peasant, king and subject, all acknowledge the brotherhood of pain, the free-masonry of agony. Alexander, of Macedon, once said he was conscious of being human only by the necessity of sleep. How often did the mighty conqueror become sensible of his humanity when writhing in the consuming fever which quenched his light; victor and master of half the world, the meanest soldier in his army was but his equal, when the parched lip and the burning skin attested the mightier sovereignty of disease.

Almost divine in its relations to man, medicine truly deserves that the ancients should have held it a direct emanation from the Deity. Coëval with the world's first history is it made by the grand old Greek, who gives to the Titan, Prometheus, the honor of bearing to mortals that best and greatest gift of the Gods, from Heaven.

> "Hearken the rest," says the bound Prometheus,
> "And marvel farther—what more arts and means,
> I did invent, this greatest—if a man
> Fell sick, there was no cure, nor esculent,
> Nor chrism, nor liquid, but for lack of drugs,
> Men pined and wasted—'till I shewed them all
> Those mixtures of emollient remedies,
> Whereby they might be rescued from disease."

Thus held divine in its origin, can we wonder that divine honors were paid to the earliest practitioners of the art, and that amidst the tumult and bustle of the camp, in the rudest age of Grecian history, the wound of a physician should be the signal of distress and concern to an entire army.

> "A wise physician, skilled our wounds to heal,
> Is more than armies to the public weal,"

Says the venerable Nestor, when conveying the wounded Machaon from the field of battle, and so keenly did the entire army sympathize that we are further told, that

> "Trembling Greece for her Physician feared."

To attain a competent knowledge of any mechanical trade, a long and tedious apprenticeship is required; can the science of our own most intricate structure be acquired without similar toil? No! there is no royal road to medicine. Labor incessant and continuous labor, must be your watchword, patient industry, perseverance and unremitting observation your guides. Every sense must be sharpened, every faculty strung to its highest state of tension.

Differing from other sciences, medicine calls into requisition every one of our organs. The eye, the ear, and the hand, must each receive their share of training. The ear must be educated to distinguish the nicest gradations of respiratory murmur, the crepitation of pneumonia, the rhonchus and sibilus of bronchitis, must become such familiar sounds as to lead to an unmistakable diagnosis; for remember, an error in the true signification of a murmur may make the difference between the life and death of your patient. The voice of the ever restless and surging heart must convey such an impression to your ear as if the organ were naked in your hands, and you could see and unravel the whole of its delicate machinery of valves, and chambers, and muscles.

The eye must be taught to note the aspects of disease; for the physiognomy of most maladies is as well, nay, better marked than the physiognomy of character. Each set of symptoms stamps, as it were, its seal upon the visage; the pale, blanched face of malaria, the pinched expression of peritonitis, the ferretty eye of yellow fever, the œdema of Bright's disease, are all marked and palpable to sight, and it is by diligently exploring the great storehouse of disease opposite, by familiarizing yourselves with the varied features of the minor maladies, in the dispensary of this institution, that you will catch that rapidity of reasoning, that quickness of diagnosis, which looks like intuition, and would make one think the mind had leaped to a foregone conclusion, did we not feel sure that the result occured from a series of strict logical inferences, which long habit had caused to traverse the brain with the rapidity of the lightning flash. It must not be supposed that the rapid diagnosis of the practiced physician is a preconceived idea; it is in fact the highest stage of reasoning, and constitutes the perfection of medical experience.

In the words of one of the soundest of modern metaphysicians, "We may fancy that we see and feel what we in reality infer. Newton saw the truth of many propositions of geometry without reading the demonstrations, but not, we may be sure, without their flashing through his mind. A truth, or a supposed truth, which is really the result of a very rapid inference may seem to be apprehended intuitively. It has long been agreed by philosophers of the most opposite schools, that this mistake is absolutely made in so familiar an instance as that of the eye-sight. There is nothing which we appear to ourselves more distinctly conscious of, than the distance of an object from us, yet it has long been ascertained that what is perceived by the eye, is at most nothing more than a variously colored surface, that when we fancy we see distance, all we really see is certain variations of apparent size, and more or less faintness of color, and that our estimation of the object's distance from us is the result of a comparison, (made with so much rapidity, that we are unconscious of making it) between the size and color of the objects, as they appear at the time, and the size and color of the same or similar objects, as they appeared when close at hand, or when their degree of remoteness was known by other evidence. The perception of distance by the eye, which seems so like intuition, is thus in reality an inference grounded on experience; an inference, too, which we learn to make, and which we make, with more and more correctness as our experience increases, though in some familiar cases it takes place so rapidly as to appear exactly on a par with those perceptions of sight which are really intuitive, viz.:—our perceptions of color!!

It is this rapidity of reasoning which you must endeavor to gain, and, believe me, it is no easy task. Do not misunderstand me; I do not wish you to infer that the instant you see a case of disease, you must leap to the conclusion it must be of such or such a nature. Years must elapse, and experience be multiplied upon experience, ere you dare hope to equal the great masters of diagnosis, but diligence, perseverance, and constant familiarity with the various phases of disease will at length conduct you to the desired result. The eye must be schooled to see, the ear to hearken, and the hand to feel the shadows of difference, and do not be ashamed to acknowledge a mistake.

It was said to me by, perhaps, the greatest diagnostician now living (Dr. Addison) that he always learned more from his errors than from his successes. Ponder this well, and you will see the truth of the seeming paradox. In the words of a celebrated German poet, "They are wise who travel through error to truth—but they are fools who ever cling to error." Do not, like some, assert that your hand cannot deceive you; your eye never fail you; consider whether your senses may not be fallible; exert a becoming modesty; condescend to examine into the minutest particulars, and by so doing you may be spared the terrible ordeal of mistaking an aneurism for an abscess, or the provisional callus of a fractured limb for a recent dislocation. Bear ever in mind, that life and death are in your hands; the error of a moment may turn the scale against you, and the opportunity once lost may never be regained. The necessity of thoroughness cannot be too strenuously impressed upon you; at any period of your life you may be suddenly called upon to act in emergencies, where an instant's indecision may bring destruction to your fellow-man. You have no time to refer to books; a dim confused idea of what you onght to do may alone be present to your mind. How terrible must be the feeling of ignorance at an important crisis like this. How awful the idea, that had you bestowed the time lawfully required of you on your studies, the fellow-creature now supplicating you for aid, need not have besought in vain. Bring the question still more home to yourselves, your parent, your wife, your child may require your help, and rely with fond confidence on your skill. Dare you deceive them, dare you play the charlatan in your very household; can you stifle the still, small voice, which says you are sacrificing the health, destroying the lives of those who are to you as your very heartstrings? Your despicable ignorance, your cowardly idleness, now rise up with terrible distinctness before you; you would give years of future life for one half hour of the knowledge once so bounteously spread before you, and which should and might have been now your own. Remember that in our profession, "Ignorance is a crime," and although you may think its results may be glossed over, a time will surely come when it will be fearfully and signally avenged.

The branches of medical science to which you will have to give attention are numerous, but all bear a fixed coördination one to another. Some may be termed basement or fundamental, as forming the substratum from which others spring—these, though absolutely indispensable, may be termed subsidiary or derivative. The two fundamental sciences, those which form the corner stone of medicine, and which are in fact the alphabet of the art, are anatomy and chemistry. Anatomy teaches us the exact construction of our body, the relations of muscles and nerves, arteries, and veins, the position and structure of the various vicera within us. As our object is to cure disease, and as that disease must be situated in some one or other of the structures of the body, in what manner can we apply our remedies unless we have some ideas of the structures in which disease exists. What physician would venture to make a diagnosis; what surgeon would dare to operate without a knowledge, aye, and an accurate knowledge of those parts which, hidden from his eye, demand his aid? A physician without a knowledge of anatomy is a vessel without a rudder, driven hither and thither by varying winds, drifting on shoals and quicksands, and if reaching port, only by a chance, over which it has no control. Repugnant and disgusting as may first appear to you the dissecting room, habit will soon reconcile you to it, and there is no branch which will at length exercise such a fascination for you. To disentangle the delicate threads which carry sensation, to examine the exquisite mechanism by which motion is allowed, to observe the marvellous machinery by which the ebb and flow of the blood is maintained, can only raise emotions of wonder and delight in those who witness them.

"How vast and diversified a field of knowledge," says Professor Owen, "opens out before us as we emerge from the portals of anatomy. Our study is that of the highest and last created product which has been introduced on this planet. Every part we study has its like in our own bodies—it may be vibrating, contracting, pulsating; and if we extend our knowledge from human to comparative anatomy, we shall there find a close and general resemblance between the structures of lower animals and that of man, and that almost every part of the human frame has its homologue in some inferior animal, and that man's organization is a special modification of a more general type."

There is but one place where such knowledge can be obtained and that is the dissecting room. Books and plates may refresh your knowledge in after life; the didactic lectures in the theatre may serve to direct your studies, but a true and thorough knowledge of the structure of man can only be obtained by individual labor, and diligent exploration of the human organism; early and late let the scalpel be in your hand, search the innermost recesses of the frame, and remember that the incisions you practise in your anatomical examination of the dead subject will be the surest guides in those you may have to perform on the living body. Practical surgery may, indeed, be termed applied anatomy. Next in importance to anatomy, chemistry demands your special attention. This doctrine may be novel to some of you who have been in the habit of regarding chemistry as a mere amusement, as an unnecessary addition to the routine you have to undergo.

Your surprise will diminish when you find that every function of the body is the result of chemical action, that your respiration is a purely chemical operation, that you digest your food by chemical solution, and that every change occurring in your body is a strict combustion. The intense heat of fever is accompanied by the same destruction of tissue as if the parts were ignited in a furnace. Chemical composition and decomposition are occurring every instant within us, and we are in fact living laboratories. I know that the so-called mere practical man will sneer at the idea of chemistry being an integral part of medical education. He is right to sneer, for ignorance confers the privilege. The mere practical man professes to denounce theory, and to confine himself strictly to facts. He himself is one of the worst, because the narrowest of theorists. He denounces, indeed, great principles, but is a bigot in little ones, and unwilling or unable to grasp facts in the mass; he clutches them hard in detail. It was your practical man who denounced Harvey, who vilified Jenner, who tortured Galileo, and all but destroyed the glorious enterprise of Columbus.

Do not, therefore, be afraid of being theorists in medicine, for, from a well founded theory often springs the surest coördination of facts. No logical theory of disease can be framed without a knowledge of chemistry, for no disease exists which is not attended by chemical change. The branch of physiology had no being until chemistry paved the way for it, and no physiologist can advance a step without its aid. The names of Claude Bernard, Shröder Van der Kolk, Mulder and Donders, stand high on the roll of Fame, as benefactors to practical medicine; and it was only by bringing their knowledge of chemistry to bear upon physiology that these bright luminaries of our art have achieved their immortality.

It must never be forgotten that the great problem of nature is composed of known and constant, and of unknown and variable elements; and it is a great mistake to suppose that in order to resolve and estimate the unknown, and to fix the fleeting shadows of the variable, it is necessary to neglect those elements which are constant and calculable. Many of the so-called vital processes may be successfully imitated by purely chemical operations. Urea, one of the most complicated products of our own organism, may be made artificially. Digestion can be perfectly paralelled by artificial solvents of food, and it has recently been proved, by the patient researches of Dr. Richardson, of London, that the liquidity of the blood in the body and its coägulation out of it, is a simple chemical process, due to, in the one case, to the evolution of ammonia, in the other, to its retention in the body. This discovery, alone, which is a purely chemical one, throws out new and striking views with regard to the cause of disease and its appropriate treatment.

The more our knowledge of chemistry advances, the more intimately d we find it linked with the conditions producing and curing sickness. A single instance, the newly discovered allotropic form of oxygen, (ozone) establishes the proposition. This agent, which presides over the laws of atmospheric electricity, and which restores to the atmosphere its normal oxygen, exercises a most important influence over us, exciting life, provoking maladies, and, in many cases, producing death.

Spots infested by pestilential epidemics are deficient in this agent; during cholera, and, probably, yellow fever, and typhus epidemics, it is absent. Chemistry, by teaching us how to manufacture and set free this agent in quantity, may grant to medicine, the precious boon of destroying pestilence, and disarming disease of its most deadly weapons.

The branches of physiology, materia medica, and midwifery demand your most earnest attention; and remember that numerous as these several studies seem, practically they are mutually interdependent, and all point to the great object of all medical knowledge—the diagnosis and treatment of disease. I say, emphatically, diagnosis, for all rational treatment must be based upon our being aware, or not, of the disease we treat.

"Diagnosis," says Dr. Watson, "forms the indispensable basis of all advances in physic as a practical art. When we can once identify a given diseased condition, we obtain the privilege of watching the behavior of that diseased condition again and again, under the operation of therapeutic measures, and from that time the increase of our knowledge concerning the appropriate management of that disease becomes progressive and sure. The term experience is obviously mis-applied, and the results of our observations vitiated, when any doubt exists about the sameness of the objects contemplated. It is mainly to this imperfection in the diagnostic part of medicine that we must attribute the uncertainty and variation both of doctrine and practice, which have brought so much suspicion, and ridicule, and reproach upon the science we profess. False experience has greatly hindered the progress of the healing art, and false experience springs from false diagnosis. Accuracy of diagnosis cannot be too highly estimated, nor too diligently sought after." An old Greek writer has truly said, "he is the best physician who can distinguish what he can do, from what he cannot do;" and recollect, that without an exact knowledge of the conditions which constitute disease, it will be utterly impossible for us to draw the distinction. There is but one method by which this crowning portion of your studies can be perfected, and that is by the sedulous investigation of disease in its every aspect.

Familiarity with disease can be obtained only by watching carefully at the bedside of your patient, by following him through every varying phase of his malady, from its initial to its termination, either in recovery or death. And your quest must not stop even here, it must extend beyond death. You must in the silence of the dead-house interrogate every organ, probe every tissue, and force nature to reveal through what secret lesion she succumbed to her foe. This is what we understand by clinical medicine. And here I cannot but congratulate you in having for a teacher, and ourselves in possessing for a colleague, one who, in this department of our science, stands preëminent; one who, by his laborious researches, has done so much to exalt the profession he adorns, and who stands a living exemplar of the truth of Armstrong's aphorism, that "genius in a medical man is synonymous with the habit of patient observation and continuous reflection."

And while felicitating you and ourselves on possessing such a coädjutor as Austin Flint, let us pause for a moment, to deplore the loss we have sustained. Death, who with equal steps strikes at the door of rich and poor, of peasant and of sage, has bereft us of a wise and good man, one who would have taught you with all the vigor of his well stored mind, who would have welcomed you with all the warmth of his large heart. Little did we think a few short months ago, when we listened to the eloquent valedictory of our gifted friend, that it would, indeed, be his valedictory to the class, his farewell to the school which numbered him among its founders. The bodily form of Picton has, indeed, passed away, but that which constituted the man—his goodness, his virtues, his benign influence—remains, and imprinted on our hearts, lies the venerated image of a sincere friend, a good man, and an accomplished physician.

I have thus cursorily mapped out before you the several branches which form the tree of medical science. They appear numerous and difficult; practically you will find them all interwoven, and all may be acquired by a moderate amount of diligence. "*Nil tam difficile est, quin quœrendo investigari possiet,*" should be your motto, and let it ever be impressed upon your minds that your future success as physicians must inevitably depend upon the use you make of the opportunities held out to you during your studenthood. Instances have sometimes occurred where, by a happy occurrence of fortuitous circumstances, a man destitute of solid attainments, has been raised to an elevation above his merits, but his position is always insecure, and it invariably happens, that after a few years of partial success, he sinks into his deserved insignificance. It is only when a position is based upon real knowledge that it can be maintained, that is the sole foundation which can never be rased; and, rely upon it, no man who ever deserved success, failed in attaining it. But it is not alone your medical education which demands your most serious consideration; there is something more. You are now free from the thraldom of school; no parent's eye watches over and controls you; you are free, and what is termed your own masters. Ever bear in mind there are two things which make the true physician: the one is based on technical knowledge, the other is founded on morality and truth. In the few months you will be with us, many habits may be formed, many may be broken; endeavor to acquire those which will best stead you through life, and a conscientious devotion to your medical studies will be your best discipline, and your surest safe-guard against the temptations constantly assailing a young man on his first introduction to a large city.

Learn to say *No*, when your conscience forbids you to say *Yes!* And remember that many of the habits you now form may cling to you through life, and that you have it in your own power to determine what you will be. Your future history and character must be, to a great extent, of your own making. Pause, therefore, and dispassionately consider what you will make of yourselves. Medical practice is both of a moral and a physical nature; the more you approach the great standard of morality, the more you will be fitted for your vocation; for, as your search is for the truth, so should you be living exemplars of truth and uprightness yourselves. You will be admitted to the inmost recesses of families; you will be the depositories of momentous secrets; you will be the confidants, the ministers, of thousands. Ask yourselves whether, in your own families, you would admit into such close relationship, one on whom rested the slightest taint of dissoluteness or intemperance.

In the words of the poet, you have to do with that, which is—

"Above all price of wealth,
The body's jewel, not for mind's profane,
Or hands to tamper with in practice vain.
Like to a woman's virtue is man's health;
A heavenly gift within a holy shrine!
To be approached and touched with serious fear,
By hands made pure, and hearts of faith severe,
Ev'n as the priesthood of the One divine."

It hardly falls within my province to regulate the method of your studies, but I have so frequently noticed a usual custom of students that I cannot refrain mentioning and commenting on it here. The habit I allude to, is that which induces most of you to run after the extraordinary diseases and neglect those which are common, but which are precisely those you will have to treat every day of your lives. Let a student be told that a bloody operation is to be performed in the amphitheatre and every seat will be filled. Probably none of you may ever be called upon to perform a similar operation, while the minor surgical ailments, the ulcerations, dislocations, fractures, which may chance to be your very first, certainly, your most usual practice, will be comparatively neglected. In like manner, in medical cases, the incurable diseases will be regarded with something like awe. A student will stand for hours listening to, and commenting on, an aneurism of the aorta, while the coughs, the colds and fevers will hardly receive a passing thought. Men will go into ecstacies over what they term a "beautiful case of heart disease," and treat with a kind of supercilious contempt the catarrh or influenza, infinitely more important. I have never seen a disease yet, which deserved the name of beautiful, and I certainly should never think of applying the term to one which, in the the present state of art, is incurable. Remember we have to cure disease, and our object is to prevent men from dying before their time; and it is precisely these minor maladies which inflict the largest amount of misery upon mankind. Study these, and leave the curiosities of medicine to maturer years, to those who can afford to indulge in the luxuries of disease, if they deem it fit. It is only necessary for you to be aware of their existence; your time is too short to allow more than a passing glance at them. Let the solemn aphorism of the great father of medicine ever ring in your ears:

> "Ars longa, vita brevis, via difficilis,
> Experientia fallax!"

And you will be saved from wasting valuable time on what can be of no permanent use to you, or any human being.

During your student-life, and afterwards, you will meet with persons who will endeavor to detract from the science you profess: the world is full of men who have not yet learned to act in medicine, as they would in other subjects, and infer that in a science so complicated and difficult as our own, he who has most studied it is most likely best to understand it. Men who, if their watch is in the slightest degree deranged, would be horrified at the idea of any but a professed watchmaker attempting to regulate it, will either treat the complicated machinery of their own bodies themselves or entrust it to the most arrant quacks and rogues. They will tell you they do not believe in medicine, but will swallow with ludicrous credulity the unblushing lies and humbugs of Homœopathy, Hydropathy and the thousand other-pathys, daily springing into existence. They will endeavor to prove to you the nullity of the science you have spent years to master. Do not argue with them, for you must be worsted. You are not their equals; your knowledge must necessarily be limited—their ignorance is boundless. Again, you will be told that medicine has done nothing for mankind, that you do nought but experiment upon those submitted to you. Medicine done nothing for mankind? Look at the annals of past ages, see the diseases that have disappeared and are still disappearing before the advance of medicine. Diseases formerly of every day occurrence, are now known only by name. The scurvy, which formerly decimated our shipping, has well nigh disappeared; the black death, the fearful pestilence which ravaged Europe, is but a record of evil; who now dies of ague? yet, formerly, so common was it that probably a fifth of the annual mortuary was due to it. In 1658 the greatest sovereign that ever sat on the English throne, the Protector, Oliver, died of it. Had cinchona been discovered three years earlier he might have been saved, and the political aspect of the world would probably have been changed.

If we look over the registers of insurance offices, we shall find that life has of late become progressively lengthened, and the decrease in the mortality of infants is something prodigious. I contend this is due solely to the advances of medical science, whose province is now recognized as embracing not only the cure but the prevention of disease.

With regard to the second count, I plead guilty—we do experiment; and on the 14th of May, 1796, an experiment was tried in an obscure village in England, by an obscure country practitioner, which has had the singular effect of saving, in England alone, over 100,000 lives a year, and, probably, of a million throughout the world, to say nothing of the millions more preserved from blindness and loathsome disfigurement. Need I say that I allude to the discovery of vaccination by the immortal Jenner, the greatest benefactor the world has ever seen? This, after all, was an experiment, and as such should have been condemned. A few years ago another experiment was tried, the initiative, in this case, being taken in this country, and we now see the horrors of surgical operations destroyed, the agony of suffering soothed, and the pangs of humanity assuaged. Ether and chloroform were both experiments.

Yes! we do experiment, but our experiments are to destroy pain, to lengthen life, to exterminate disease.

There is another charge, yet, you will have to refute: It has constantly been asserted, by the multitude, that the study of medical science has a tendency to make men irreligious, to induce a belief in materialism; that the constant familiarity with death induces a disregard for life, and a contempt for the sublimest scene of humanity—the death-bed of a fellow-being. The absurdity of the charge is so palpable as hardly to require an answer. We alone know that life and death are synonymous, that without death there is no life, that life itself is but a constant succession of deaths, and that when death ceases, life ceases. We, who see such constant manifestations of the indestructibility of matter, who witness the protean forms which particles are each instant assuming, who observe that the minutest monad exercises a specific power over its surroundings, who know that even a force is never lost, we, alone, can justly appreciate the immortality of the universe, and conceive the vastness and sublimity of Him who has created it!

And if prayer may consist in the constant contemplation of the works of the Creator, in a ceaseless supervision over what he has created, in the exercise of self-denial, of patience and of unrequited charity, then, I say the life of the conscientious Physician is a perpetual prayer; and that his religion, the religion of works, outweighs all mouth-professions of faith, all fullsome adorations, and embraces the divinest attributes of all creeds.

Yes! our profession is great and noble, and if it be fallen somewhat from its high estate, the fault is not in it, but in us, and it is we that must again raise it to the dignity and honor it once enjoyed.

I have detained you longer than I had expected, been more diffuse than I had wished, but I desired to place prominently before you the duties you owe your profession, the public and yourselves. I wished to show you you required no extraordinary genius to attain the summit of your profession, but that all its details may be mastered by patient thought and perseverance.

Over the portal of Dante's Inferno was inscribed:

> "Lasciate ogni speranza, voi ch'entrate."
> Ye who enter here leave hope behind.

Over the door of this house I would write, YE WHO ENTER HERE LEAVE SLOTH BEHIND!

Labor and Industry must be your constant watchwords, and doubt not that a close and unremitting attention to your profession will insure you a career honorable to yourselves and useful to your fellow-men.

EDITORIAL AND MISCELLANEOUS.

DEATH OF PROFESSOR J. M. W. PICTON.

Died, on the 28th of October, 1858, after a long and painful illness, which he bore with all the fortitude of a man and a true Christian, Dr. J. M. W. Picton, for the past thirty-two years an eminent practioner of medicine in this city, and late Professor of Diseases of Women and Children in the New Orleans School of Medicine.

At a called meeting of the Faculty of the New Orleans School of Medicine, the following preamble and resolutions were unanimously passed:

"Whereas, it has pleased an Allwise Providence to take from our midst, in the prime of his manhood and usefulness, our friend and colleague, Dr. John Moore White Picton—

"*Resolved,* That this Faculty do truly and sincerely lament the loss of their friend and colleague.

"*Resolved,* That in the death of Dr. Picton the New Orleans School of Medicine has been sorely afflicted. He was one of its founders, exerted himself to the utmost for its advancement, and truly excelled in all that constitutes the able and valuable professor.

"*Resolved,* That in the death of Dr. Picton, Society has lost a most useful man, the Church a faithful christian, and the profession an ornament.

"*Resolved,* That the Faculty do most heartily sympathize with the bereaved family of the lamented deceased, and do most earnestly pray that He who has seen fit thus to afflict them, may give them strength to bear the blow and humility to accept it.

"*Resolved,* That the Dean of this Faculty be instructed to transmit a copy of the foregoing preamble and resolutions to the family of our deceased friend and colleague, and, also, to publish the same in the December No. of the New Orleans Medical News and Hospital Gazette.''

Our pen is totally inadequate to the task of expressing what we feel in here recording the death of one so highly valuable and valued in all the relations of life. But a few months ago he was amongst us, in all the vigor of manhood, a warm friend and an earnest and conscientious colaborer; now he is gone forever from this world of probation, and we have nought of him left save the bright and inextinguishable light of his noble example—an heritage to which no man in the community can lay exclusive claim, yet one which each should bless and cherish while life is left him.

For thirty-two long years did Dr. Picton labor for this community in the capacity of physician and surgeon, and no man before him, or cotemporary with him, ever more fully commanded the respect and confidence of society. He was thoroughly educated in the United States Military Academy at West Point, and commenced his career with a commission in our army, but circumstances induced him to turn his attention to medicine, and to locate in New Orleans. His taste for surgery was at one time indulged, and he has left behind him the record of some of the most important and highly successful operations, among which stands preëminent his case of elephantasis, wherein he successfully removed a scrotal tumor weighing 53 lbs.—the only successful case of the kind on record. The greater part of his time, however, was devoted to what is known as "family practice," and here it was that his virtues shown with a brilliancy rarely rivalled. He was honorable, gentle, affable and well skilled—he was "the good physician." But it was not only in this capacity that he appeared before the gaze of man. Dr. Picton was one of the founders of the New Orleans School of Medicine, and as a Professor of Diseases of Women and Children, he labored for the advancement of the Institution with all the energies of his mind and body. That he succeeded as a lecturer and teacher, it is needless to say, and he was beloved by his colleagues and pupils. His place will be hard, indeed, to fill.

But again we say our pen cannot give expression to what we would say of our lost friend and colleague. To praise him is needless: his virtuous and useful life praises itself. If we would ourself ask a special boon, it would be that we might live as good a life and die as sweet a death as his.

Health of the City and Medical Lectures.—At last has yellow fever ceased to be the sole topic of conversation, and New Orleans looks like herself. All is bustle and business, with a fair proportion of pleasure, and our city rolls onward in the car of prosperity. The medical lectures in our schools commenced the 15th of November, but to much smaller classes than usual, because students were afraid to encounter yellow fever. However, the classes have filled up rapidly within the past ten days (we write on the 25th of November) and there is now no doubt about our having a fair share of the patronage of our Southern young men; though of course many have been driven away to other schools through erroneous impressions in relation to the prevailing fever. A few years will set this right, however. As soon as the physicians in our country are made fully aware of the unrivalled advantages we possess, they will know it is the policy of their students to wait a little on yellow fever rather than run off to schools which cannot possibly afford what ours can. The epidemic has lingered with us this season later than it ever did before, and yet it has been safe for students to come in some time before the very first arrived. This matter, too, will be more fully investigated and understood as the country more fully appreciates the extraordinary advantages of our city for the medical student.

Prof. Austin Flint arrived in our city in due time, and at once entered on his duties as Professor of Clinical Medicine and Auscultation and Percussion in the New Orleans School of Medicine. To say that he has proved in the highest degree acceptable, both to the Faculty and Students of the Institution, is needless: he is all that we could desire as a colleague and a teacher, and we venture to predict for him the increased admiration of all with whom he is thrown in contact. The field in which he labors is fertile in the extreme, for he has free access to the great Charity Hospital, and he is widely known to be a systematic and untiring laborer. The Chair he occupies is new in this country, but its importance is recognized by all who have heard him and his worthy predecessor, and it is no exaggeration to say, that the student who assiduously attends his lectures at the bedside and in the amphitheatre will have a more useful fund of information on the subjects taught than can be elsewhere acquired in this country, and this because there is the time and the material, and then a teacher unsurpassed.

We beg leave to call the attention of our patrons to the card of Prof. Flint on our advertisement page. He offers his professional services to our community, and when we reflect that he has devoted his life thus far to the accurate study of disease, and more especially to the manifold and important affections of the Heart and Lungs, we are sure he will be regarded as not only a valuable acquisition to the ranks of the profession South, but his consultations will be sought by many who appreciate the value of the really scientific physician.

By the announcement below, it will also be seen that he proposes to give private instruction in his specialty to students and graduates. This is a rare opportunity for all interested. Professor F.'s style of teaching surpasses anything we have seen.

PRIVATE INSTRUCTION IN AUSCULTATION AND PERCUSSION, ETC.—Professor AUSTIN FLINT will give private instruction in Auscultation, Percussion, etc., to Medical Students and Physicians desirous of becoming practically conversant with physical exploration as applied to the diagnosis of diseases of the Heart and Lungs. A course of instruction will consist of Twenty-five Lessons, embracing explanations, examination and demonstration of physical signs at the bed-side. Fee for the Course, $25.

New Orleans School of Medicine, Nov. 1858.

THE BOSTON JOURNAL ON THE PURITY OF MEDICINES.—The Boston Medical and Surgical Journal, for October 28th, has an article on the subject of "The Purity of Medicines," which seems to have been elicited by late remarks in our medical journals on what it calls the "sundry short-comings" of Tilden & Co., manufacturing pharmaceutists. The Boston Journal evidently coïncides with the editors who have seen fit to condemn Tilden & Co., but it thinks that one journal has commented "with unnecessary harshness·" The cap fits us, because we receive all the journals in the country, and not one has so freely expressed itself as ours. The difference between the Boston Journal and ourself is plain, and to us, entirely satisfactory. If we drown a man in self-defence—that is, to keep him from drowning us,—we would plunge him into deep and hot water; while our neighbor would gently roll him into shallow and milk-warm *molasses* *and water.* If a man wrongs us individually, we may choose to overlook his act altogether, or to remonstrate in terms of mildness; but when a man, as we believe, wrongs the profession we have adopted, thereby not only injuring us, but every member of the profession and of our community falling under his influence, then do we not only think it our privilege, but our duty, to visit him with all the strength of condemnation that our command of genteel language will allow. When Tilden & Co. dare to counterfeit the "Compound Cathartic Pill" of the U. S. Pharmacopia, in order to pander to the presumed popular prejudice against calomel, we think it our privilege and our duty to denounce the act and to hold its authors up as fit only for the severest condemnation of all true medical men; while the Boston Journal chooses to say: "We protest, then, against this act of Messrs. Tilden & Co., which only serves to foster an absurd prejudice, and rather tends to cripple the practitioner who knows how to use medicine."

Does not every newspaper in our land teem with advertisements of "pills without calomel"—"pills purely vegetable"? And in what are Messrs. Tilden & Co. better than the advertisers? Are they not, indeed, *worse?* The regular quack advertises his pills, and claims no connection with the medical profession, while Tilden & Co. profess to be of us, and yet are not. We can see *something* to admire in an open unblushing quack; while we can only despise every characteristic of a Judas.

The Boston Journal says that Tilden & Co. are going to publish an explanation of their short-comings, and it "cannot but hope that it will prove abundantly satisfactory." We look anxiously for the explanation, and cordially join the Journal in its hope. But we say now, that no patient of ours will ever swallow one of Tilden & Co.'s sugar-coated pills, of any kind, sort, or description, by our advice. We prefer our own formulæ, or those of our own brethren, to any that a mere manufacturing pharmaceutist can make, and we are only astonished to find the least favor shown to such individuals.

We are sorry to find ourselves clashing with the Boston Journal on so important a subject as this, but that cannot be avoided, we suppose. We have done what we conceived to be our duty, and it has seen fit to criticise our conduct; it must not pout, then, if we say we are ashamed to see it saying even a half a word for Messrs. Tilden & Co. With all their wealth and advertising influence, Tilden & Co. should be able to fight their own battles.

BRADYCROTE TREATMENT OF YELLOW FEVER.—We are under obligations to the authors for a valuable pamphlet entitled, "Bradycrote Treatment of Yellow Fever, by Veratrum Viride and Gelseminum Sempervirens. Reported for the Charleston Medical Journal and Review, by O. A. White, M. D., and W. H. Ford, M. D., Charleston, 1858."

It seems that in Charleston, as well as in New Orleans, the veratrum viride has been tried in the treatment of yellow fever during the past summer. We do not stop here to inquire into the claims of priority, etc. If it ultimately turns out that the remedy is really the best, then the parties using it must fight the battle. We have but time now to note the results of the treatment pursued by Drs. White and Ford.

The total number treated by veratrum viride was 117; of which 15 died. Of these, "38 passed black vomit downwards; of which 12 died, and 26 recovered."

The total number treated with gelseminum sempervirens was 24, of which all recovered. Two of them vomited black vomit, and 5 passed it downwards. We say, without hesitation, that the number recovering after the appearance of black vomit has been greater in the hands of Drs. White and Ford than we have ever before heard of, and this deserves the fullest consideration; but we do not find that the total mortality is less in their hands than it is commonly found to be here. We have compared notes with reliable gentlemen in our profession here this fall, and we find that where their patients are nursed, and without the use of the v. v., or g. s., the mortality has not exceeded 10 per cent. Of course no man can be expected to cure cases that are almost dying when he is called to them. For ourself, we say that we are bettter satisfied with the result of our own treatment than with the treatment with the v. v. in Charleston. We have treated 45 cases of yellow fever, of which two were beyond all hope when we first saw them, and we have lost 5. Properly speaking, we have treated 43, of which we lost 3. We do not pretend to say a word against the new remedies in question, for our experience with them is limited. So far as we have used them, we are rather disappointed; but we shall try them again. We mention here, in passing, that the gelseminum is quite extensively used in Mississippi, on the plantations, as a febrifuge, and under the quack name of "Speed's Fever Tonic."

It is a pity that Drs. White and Ford did not confine themselves more fully to the new remedies, as the observations would then have been more valuable. Instead of this, we find them not only using mercury, but "invariably attempting" to produce ptyalism. We, also, find them giving, in the beginning, a saline cathartic, "and if the headache was intense, five or six evacuations were procured." Also, "a saline diuretic, and refrigerant mixture, specially directed towards the kidneys as soon as the intensity of the symptoms had been subdued."

Now, here in New Orleans, it is the rare exception to find a physician of experience in the treatment of yellow fever attempting mercurialization, inasmuch as it is regarded as a disagreeable complication, but many give a mild purgative dose of calomel in the beginning, and find benefit from it. Our own observation teaches us that the great majority of yellow fever patients progress thus: You are called to a man with intense headache, backache, high fever, etc., etc.; he is just taken. If he has very recently eaten a meal, we give him a little warm salt-and-water to drink, and unload his stomach. Otherwise we give a dose of oil, and put his feet into a hot bath. He soon falls into a profuse perspiration, in a few hours his bowels are well relaxed, his perspiration continues for two or three days, assisted by diaphoretics, and gradually declining, until on the third or fourth day we find all fever gone, some food indicated, and the patient on the road to recovery. Of course, complications—as congestion, etc.—have to be resisted *pro re nata.* The same thing we notice where the v. v. is given. Now it remains to be proved whether this is coïncidence or cause and effect.

BOOKS AND PAMPHLETS RECEIVED.

"BRIEF EXPOSITIONS OF RATIONAL MEDICINE, to which is prefixed THE PARADISE OF DOCTORS—A Fable. By JACOB BIGELOW, M. D., late President of the Massachusetts Medical Society, etc. Boston, 1858."

When we very recently noticed the receipt of Sir John Forbe's late book, entitled, "Nature and Art in the Cure of Disease," and, in the the mildest terms we could, put cur condemnation on it, we were in hopes that we should never again be called on to read a book, from a respectable source, so little calculated to do any good and so eminently calculated to do harm; so far from expecting to find a single respectable indorser of such a work, we have looked, with confidence, for nought but terms of condemnation from the pen of any and every respectable medical man who should deign to notice the book at all.

Who is Sir John Forbes? and what has he done? We will only detain the reader with answers almost as short as the questions themselves. Sir John Forbes is "Fellow of the Royal College of Physicians, London, and Physician of the Queen's household, etc., etc." Sir John Forbes has been a regular practitioner of medicine for fifty years, has amassed a princely fortune by practicing as other physicians do, and now issues a book to inform the world that art can effect nothing in the cure of disease, that nature does the whole work. Comment is scarcely necessary. To have maintained his position in the Queen's household he must have practiced medicine legitimately—he must have aided those who were really sick with the known useful appliances of our art: he must have given purgatives to overcome constipation, emetics to unload the stomach, quinine to prevent paroxysms of intermittent fever, etc., etc., etc., etc.; and he must have vaccinated to prevent smallpox. If he administered these and many other known useful remedies, without believing in their efficacy, then was he receiving princely pay for the grossest deception; if he administered bread pills in their stead, and clandestinely, then was he not only guilty of the same offence, but he should be despised for not having the moral courage to have come out long ago and proclaimed the views he now sets forth. We can only reconcile his conduct with his dotage; we are bound to presume that his proper position is beside the venerable Prof. Hare, who in his old age fell a victim to the miserable humbug, Spiritualism.

But in Dr. Jacob Bigelow, of Boston, we now find a champion of Sir John Forbes. He, too, writes a little book (a very small one) in which he indorses all that his oracle has said, and prefixes a fable, "The Paradise of Doctors." Shade of Æsop! come to the assistance of the modern fable writer. In the name of science and common sense, give him a theme, and then invigorate his pen.

To read these little books of Sir John Forbes and Dr. Bigelow, one would suppose that they were the only "rational" medical men in the civilized world. They tell us of "Artificial" doctors, "Expectant" doctors, "Homœopathic" doctors, "Exclusive" doctors, "Hydropathic" doctors, and, lastly, "Rational" doctors. All those who read their books must belong to one or another of the five first named; while *they* are, *par excellance*, the "rational" doctors. We look around us and find that the course pursued by respectable medical men is about as "rational" as the frailties of the human mind will admit—that no conscientious and well informed man is solely the one or the other—that every such man sifts from each all that is good, and eschews all that he deems evil, and yet all are irrational practitioners, and the bright day of "rational medicine" is just dawning in Boston and London. Sir John Forbes and Dr. Bigelow had better, old as they are, study human nature a little farther; they will surely find that, although the tendency of man is to run into extremes, there is a conservative balance ever existing, and the march of science and art is onward—the march of medicine is onward. Thus far they have lived long lives to little purpose, outside the little circle of self; and now they are establishing the unenviable reputation, at least with their professional brethren, of having long practiced an art, the utility of which they only ignore when they are looking into their graves—or, when they no longer need the bread and meat, and raiment which the practice brings.

"TRANSACTIONS OF THE THIRD SESSION OF THE MEDICAL SOCIETY OF THE STATE OF CALIFORNIA; Convened at San Francisco, February, 1858."

We are under great obligations to Dr. Thos. M. Logan, of Sacramento, Corresponding Secretary, for the foregoing valuable volume. Certainly, the California doctors know how to keep up their State Medical Society, and this volume of Transactions really does them honor. We wish that time and space would permit us to notice it in detail; but we shall certainly recur to it. When will the doctors of our State arouse from their lethargy and resuscitate their State Medical Society?

"AN INAUGURAL DISSERTATION ON STRYCHNIA; presented to the Faculty of McGill College, May 1st, 1858, prior to receiving the degree of Doctor of Medicine and Surgery. By ALEXANDER P. REID. Montreal, 1858."

Really a valuable and interesting pamphlet, and doing honor both to its author and to the institution from which he received his degree. It is a pity, however, that the author entirely neglected to mention the frog test for strychnia. It is certainly a most important and interesting matter, and we are surprised that it should have escaped his attention.

"The Annual Address delivered to the Graduates of the Atlanta Medical College, at the Commencement, September 2, 1858. By Curtis B. Nottingham, M. D., of Macon, Georgia."

A handsome and interesting address.

"The Treatment of the Paralysis of Motion. By Charles F. Taylor, M. D., from Amer. Med. Monthly for November, 1858.

A valuable paper.

Report of Deaths in the City of New Orleans, *for the four weeks ending November* 21*st*, 1858.—*For week ending* 31*st October*.—Men, 169; women, 49; boys, 68; girls, 52; adults, 218; children, 120; males, 237; females, 101. Total, 338.

Yellow fever, 174; congestive fever, 15; consumption, 13; infantile convulsions, 15; stillborn, 6.

For week ending November 7*th*.—Men, 134; women, 46; boys, 51; girls, 38; adults, 180; children, 89; males, 185; females, 84. Total, 269.

Yellow fever, 136; congestive fever, 13; consumption, 17; infantile convulsions, 6; stillborn, 4.

For week ending November 14*th*.—Men, 97; women, 33; boys, 62; girls, 37; adults, 130; children, 99; males, 159; females, 70. Total, 229.

Yellow fever, 66; congestive fever, 9; consumption, 20; infantile convulsions, 8; stillborn, 7.

For week ending November 21*st*.—Men, 69; women, 45; boys, 40; girls, 35; adults, 114; children, 75; males, 109; females, 80. Total, 189.

Yellow fever, 17; congestive fever, 5; consumption, 20; infantile convulsions, 8; stillborn, 5.

New Medical Journal.—We are in receipt of the October and November numbers of the "North American Medical Reporter," edited by W. Elmer, M. D., and published in New York City, by W. A Townsend & Co. It is a journal of 28 pages, got up in neat style, and published at $1 per annum.

List of Payments to November 26, 1858.—Drs. H. Peake, vols 4 and 5, $10; J. H. Richardson, 1 year to Sept., 1859, $5; J. T. Randolph, vols. 3, 4 and 5, $15; G. E. Elmer, vol. 5, $5; H. Bouldin, vol. 5, $5; Mr. L. P. Conner, vols. 5 and 6, $10; Drs. J. T. Murdoch, vol. 5, $5; J. A. Warren, vol. 5, $5; T. C. Brunson, vol. 5, $5; H. Ravenel, 1 year, to Aug., 1859, $5; T. J. Heard, vols. 4 and 5, $10; J. P. Barbot, 1 year, to Oct., 1859, $5; Henderson, (N. O.) vols. 4 and 5, $10; Mr. J. H. H. Hedges, vol. 5, $5; Drs. H. J. Means, vol. 5, $5; Eylandt, (N. O.) vol. 5, $5; T. J. Henderson, vol. 5, $5; White, (Berwick's Bay) 1 year, to Nov. 1859, $5; J. Kleinpeter, balance to end of vol. 5, $8; J. H. Castleton, vol. 4, $5; J. J. Burroughs, vol. 5, $15; and A. Martin, vol. 5, $5.

EXCERPTA.

Poison Oak—(*Rhus Toxicodendron*).—*By F. P. Wierzbicki, M. D.* There are several species of the "poisoned oak" known to naturalists. I have met in California with but on species. *Rhus toxicodendron* infests the whole country, from San Diego northward. Where nothing else would grow, it flourishes, and flourishes almost as well as it does in rich soil, where it frequently attains the height of ten feet and the dimensions of a large bush. We can safely say that a person cannot walk in the fields without treading upon, or brushing against its evergreen branches at every step; so widely spread is the noxious shrub. Some writers make a distinct species of what they call *Rhus radicans.* I agree with Dr. Bigelow that there is no such species; it is the same *Rhus toxicodendron* that becomes *radicans* when there is a support close at hand, around which it can entwine itself; otherwise its stem grows sufficiently stout to be erect and face the winds of the heavens.

There is, however, another species of the shrub in California, in the latitude of San Diego and southwards. My impression is, that this species has not yet been described. Certain it is, that it does not correspond in appearance, either with *Rhus vernix* or *Rhus pumilum*, which are both considered very poisonous. It grows in that region very luxuriously; is evergreen and attractive in looks, like its cousin of the north. It emits, however, an extremely offensive stench, as a warning, as it were, to the passer-by, not to approach, for it is the most virulent of its class in its influence upon the human system.

The poison oak at night, or when it grows in a shade, emits, according to some, hydrocarburetted gas with some acrid vapor; and it is this which is supposed to be noxious. Whatever may be the nature of this exhalation, it is certain that it is not confined to night or shade alone, as it is equally pernicious even when it grows in broad sunlight. These exhalations seem to float in the air, and are absorbed into the system either through the lungs or the skin, or through both at the same time. They are the most noxious, the most virulent, when they come in the shape of fumes arising from the burning shrub.

The poisonous effects of the *Rhus toxicodendron* are manifested first upon the skin by a slight eruption, preceded by itching. This eruption may be considered as erysipelatous inflammation. Though the eruption is always characterized in the incipient stage by the erysipelatous blush, it changes its form according to the part affected. Sometimes it presents itself in vesicles scattered without any regularity over the surface of a part affected. Sometimes it forms only an inflamed line, not unlike a scratch. Sometimes, as when on ancles or wrists, it appears like a bracelet encircling the part, and on feeling it, gives the idea of small shot lodged under the skin. These vesicles maturate, if not interfered with, early; they become inflamed, and the part increases in swelling till the skin breaks and matter issues. No part of the body is exempt from being affected by this eruption, and any slight circumstance determines its location. A scratch, a part in perspiration while exposed to the air, a sprain like that of the wrist, is sufficient to locate the affection, when a person is exposed to the exhalations of the shrub. Its favorite location is the face, arms, and *pudenda*.

The degre of susceptibility to the exhalations of the shrub are different in different persons. While there are some who are never affected by these exhalations, even by touching the shrub, others, on the contrary, cannot without the risk of infection, show themselves in its proximity. There are some persons who at one period were not susceptible to the poisonous effects of the shrub, but who have subsequently become very susceptible to them, and this susceptibility seems to increase with each new attack of the affection. I am of the opinion that in many cases the *virus*, by frequent exposure to it, has been accumulating its force in the system before it breaks out, and by the treatment usually pursued, it is not eradicated thoroughly; and hence those frequent attacks during a season favorable to it, or those periodical returns of the affection which some people experience. The success of my own plan of treatment of this malady confirms me in the above opinion.

Merely riding through the field where the shrub grows, is enough to cause some persons to be attacked by its poisonous exhalations. Those who are very susceptible to this influence, can frequently, by a peculiar sensation they experience in the nostrils, tell whether they are going to suffer its effects. The excessive itching of the part affected soon removes the doubt, if any exists, in relation to the matter, and the itching, together with the blush already alluded to, are unmistakable symptoms of this affection. The itching accompanies it through its whole course.

Handling in any way this poisonous shrub exposes the susceptible person to an attack of its *virus*. Persons touching cloths against which a branch of this shrub has rubbed; handling firewood to which a twig did or does adhere, are frequently equally liable to suffer. Walking in the field where the shrub has been destroyed and only some fragments remain on the ground, may expose a person to an attack. Some pretend to say that contact with the shrub is necessary to produce poisoning; but they are entirely mistaken, as I can assure them from my own personal experience and observation.

The degree of intensity of the affection varies according to the peculiarities of the constitution, from the most trivial to the most serious symptoms, sometimes involving the whole frame, sometimes endangering life, and even bringing on death. Several fatal cases have happened in this country among new comers; but the deaths in such cases were owing to the mismanagement of the practitioner. Death should be an extremely rare occurrence in this affection, if properly treated.

There are various plans of treatment of this malady, both among physicians and the people. Some of them are innocuous, and if they sometimes succeed, it is because the malady gets cured of itself. Others again are mischievous, and may prolong the suffering, or forever disfigure the part affected. They may leave unseemly scars, or minute capillaries engorged with blood, which thus present a sort of network of red lines on the surface, which never can be destroyed by any subsequent applications.

I do not, however, propose to detail here the means resorted to for the cure of this malady by others. I will confine myself to stating the principle upon which any remedy can succeed or fail; and then I will indicate the method I rely on, in the treatment of this affection. The principle upon which any means may succeed in this malady, must have for its object either the prevention of the development of the vesicles in the very inception of them, as a moderate degree of cold steadily applied, or their destruction by some counteracting agent brought in contact with the surface affected. It is upon this latter method that I depend, so far as the destruction of the *virus* externally is concerned; but as this on many occasions, is not enough, I have then recourse to some of the natural functions, such as the skin and the bowels. As in this affection capillary vessels are engorged, alkalies of moderate strength are the proper remedies for the cure of the affected surface; for they not only by their action upon the skin serve as gentle counter-irritants, but being absorbed in a measure by the skin, they cause liquefication of the engorged blood. Such being my views of this malady, I have no difficulty in combating it successfully.

On the very first appearance of this affection, wet the part well, and sprinkle it plentifully with powdered commercial soda, known to housewives as "washing-soda," and let it dry. One or two applications of this sort in the course of twenty-four hours, will cause the incipient eruption to vanish. I prefer this impure carbonate of soda because it is stronger in its alkaline action than the carbonate of soda of the apothecary. In any stage of this affection the same means can be used, provided regard is had to the strength of the alkali; for when the skin is broken and the vesicles exude matter, the strong alkali causes unnecessary suffering to the patient. Washing, therefore, with a warm and moderate solution of the same will subserve the best purpose. Where running sores have been formed after washing the part with the above mentioned solution, dry lint should be applied to it, which is to be removed on the next morning's dressing, by means of soaking the part in warm water first.

The above mode of procedure is the most simple, the most rational, and the most successful, as superficial means, for the cure of this troublesome affection. If the symptoms, however, should be of a grave nature, internal remedies also, should be resorted to, and the following in preference to any other: *milk of sulphur*, with *bitartrate of potassa*, in quantity to suit the case. Any other saline cathartics will do well. Sometimes even an emetic may be absolutely necessary; the judgment of the practitioner should decide upon its propriety.

As an external application, when the above mentioned alkali cannot be procured, the most ready remedy may be found in warm water, in which a large quantity of table salt has been dissolved; it should be as warm as the patient can bear it. Washing the affected part with this solution for several minutes at a time, morning and evening, relieves the excessive itching, and though slowly, yet surely, cures the malady. Weak *aqua ammonia*, applied with a feather to the affected part, is also a good remedy, though not equal to the carbonate of soda. Holding the part affected to the heat of a live coal, as near and as long as can be endured, a few times causes the eruption to disappear, if it be in the very incipient stage. This remedy is not, however, of an equal effect and adaptability to every part of the body. It should not be applied to the face, on account of exposing the eyes to the heat, and because that part cannot be conveniently kept covered up after the application. The view I hold of this affection authorizes me to recommend soap liniment, to be applied with a feather to the part affected, as a remedy that promises success. I have not tried it yet, but would not hesitate so to do the first opportunity, since I am persuaded that my view of the malady is the correct one, and the above means would meet the indications of the case.

Before I leave the subject, I have yet to mention what I should call a *sovereign remedy* for such cases—I mean the vapor bath. One or two baths are sufficient to destroy the *virus* of this shrub in the system by throwing it off through the pores of the skin. The vapor bath also acts as a preventive means against the exhalation of the shrub, since the system becomes less susceptible to their influence, for reasons already pointed out above. Persons that are very susceptible to this *virus* should take a vapor bath once a month or so, if they wish to be free from this annoying affection.

There are many prejudiced against vapor baths, because they are supposed to expose the patient to taking cold more readily. This is a mistake. Imprudent people are always running this risk, under any circumstances. Allow your body to cool gradually; do not go out into the open air as long as the face is kept moist with perspiration; change your linen as often as it becomes uncomfortably damp from the perspiration of the body; keep for a few hours a little more warmly clad than under ordinary circumstances, and then there will be no risk of taking cold in consequence of a vapor bath. There are persons whose conformation, or the condition of whose lungs, does not permit them to breathe vapor without great inconvenience or even danger. Such subjects should take the vapor bath by means of those chairs which are constructed for the purpose, and which enable the head to be kept in fresh air.

It will be perceived that I do not follow any of the writers upon the subject of the malady caused by the exhalations of the *Rhus toxicodendron.* I found their notions on the subject so absurd and unphilosophical, both in theory and practice, that I do not hesitate to discard them, considering that my experience and observation upon my own person and the persons of others, will be a sufficient apology.—*Trans. of Cal. State Med. Society.*

On Mentagra, or Sycosis.—*By R. E. Haughton, M. D., Richmond, Ind.*—This disease, vulgarly known as Barber's Itch, is one of some interest, and one about which there seems to be but little known, either by medical men, or the common people. It is a dieease, often seen in its manifestations; but its causes and its proper mode of treatment, are topics upon which there is a variety of opinions (and some, I think, which are very crude), that perhaps another notice, even at the risk of being thought also very crude, will not do any harm, and may induce a cure of it, in some person who may be a most unhappy sufferer. I have seen several cases, and the treatment is not very severe, nor requires to be continued very long, to be successful. This is a disease of the face, though it is not necessarily confined to this surface. It affects the follicle of the hair, or beard of the face, which becomes inflamed while thickening and ulceration of all the surrounding structures, takes place, and goes on, often till suppuration is established. This process may involve a large surface, as the whole surface of the face where the beard grows, or it may be confined to a few follicles, with induration and suppuration of the parts in the vicinity of those so affected. A hair is seen to be the center of each spot so affected, and I have often taken a pair of forceps and removed the hair, and found its bulb surrounded with matter, showing the hair follicle to be the seat and center of this disease. I saw one case in which the whole bearded portion of the face was a mass of induration, with occasional points of ulceration. The face was much swollen, and very sensitive, having been under treatment a long time without benefit. This patient recovered soon under treatment which I will detail.

Causes.—Dr. Wm. Jenner says this disease is produced by a species of fungi which he denominates the micro-sporon, mentagrophyte, which enters the hair follicle, and finds a nidus between the root of the hair and the wall of its follicle. He says this is the efficient cause of the inflammation which results. With all due deference to the opinion of such authority, I must say, it does not answer the history, progress or treatment of such cases, as I have seen them. It is thought to be a contagious disease, and transmitted from person to person, in the barber-shops, where many persons are shaved by the same razor, and by the same person. I think the matter, if transferred to any other person, or to any other portion of the body, might, if it found a proper resting place, produce the same trouble; but it is feebly contagious, probably not at all so, with proper cleanliness. (The true cause is found in close and oft repeated shaving.) Many men are very fond of being closely and neatly shaved, and the skin is so excessively tender, that this is not well borne, and by it an irritation of the hair follicle is thus produced (the cause being *continued*), inflammation and all its results follow, as has been described. There is no poison from the blade of the barber, neither any fungi to find its way into the center of the follicle, to originate this disease. There seems to be no necessity to go so far for a theory of disease, when it is readily explained by the operation of well known causes, which, in the physical world, always produce the same results. Sufficient irritation of any surface or structure, will develope inflammation, and, owing to the peculiar locality and circumstances surrounding such inflammation, I should expect the very trouble which I see produced. Dr. Jenner says, "Do not confound impetigo with metagra." He also says, "Acne of those parts may be taken for mentagra." Yet he tells us, in the same breath, "There is no vegetable parasite in the hair follicles in impetigo, nor in the sebaceous follicles in acne." These are forms of inflammation affecting the same structures, and differing but little, if any, except in degree; yet one is produced by a foreign body—a *parasite* (which must be destroyed, to cure the disease). I have talked with some very intelligent shavers, and they have but one opinion as to the mode of production; and one man of an experience of twenty years, and whose observation is entitled to credit, says, "that he can invariably produce this very disease, in any individual whose face is tender and sensitive, by a few close shavings." He then cited me to some of his regular customers, who had suffered, and who would not be shaved closely, and of whom he always was very careful upon this point. In reference to cure, he says, "It is often sufficient, when trouble has been produced, to suspend any shaving, and use some soothing emollient application to the surface, till the irritation has subsided." Here, again, we have evidence of the true cause, and when it is removed or prevented, the effect (mentegra) is not produced. Another reason for this position: men who do not shave, or who have not shaved for a long period of time, are not affected by this disease. Why? Merely the *cause* (as I give it), is not, nor cannot be, in operation.

Pathology.—The essential nature of this disease, has already been very clearly arrived at, under the head of causes. But to be explicit, without being tedious,—the disease is one of irritation, first, by shaving, and this produces inflammation of the hair follicle, by a pulling or dragging effort of the razor. Induration and suppuration are coëxisting conditions, and make up the principal features of the disease. Tuberculated swellings occur, with pus upon the summit and in the center, and a dry crust forms, and this is sometimes so general, that the face is one solid scab.

Treatment.—Mentagra is sometimes a very obstinate affection. This is, I apprehend, often so, from neglect, or from an improper mode of treatment. I have already said, that to effect a cure, shaving must be avoided, and if not too bad, or too long in existence, the disease will soon get well. Twedie says, "During the whole course of the disease the razor must be laid aside. Again, if necessary to apply any remedy locally, the solution of the bi-chlo. mer. in glycerine, is a very useful and curative agent, one which, in my hands, has been successful. The white precipitate ointment will also effect a cure. The iod. oint. will also do much good, where there is much induration. Dr. Thompson strongly recommends an ointment composed of a scruple of iod. sulph. to an ounce of lard, thus making an ointment. But I *have*, before resorting to any local application, resorted to *epilation*, removing each hair by a pair of forceps, with a quick avulsion. This means will also affect a cure, as it removes one source of irritation, when the hair itself has become a foreign body within its follicle. This mode has been effectual in bad cases, after many modes of treatment had been tried in vain, and it is remarkable how soon improvement takes place when this has been done. Some authors tell us the digestive organs must be attended to, and purgatives, tonics, and antacids, administered as required. I have not seen a case which required any constitutional treatment, and therefore have never used any. Such patients generally enjoy their general health, though the local irritation is quite severe. I never prescribe a remedy in any case, unless I have an indication presented for it, not even to satisfy the desire or wish of the patient, whose notions, many times, are very crude and absurd. I am well aware, that some of these views are not acceptible to many, but I have been investigating the causes and the history of the trouble, and I am clear in reference to it, and that the treatment based upon such views, will be successful most generally. In reference to the vegetable nature and origin of this disease, Dr. Dunglison says, "It requires confirmation." I think so too.

PROPOSITIONS IN THE TREATMENT OF PLACENTA PRÆVIA.—*By W. S. Atlee, M. D.*—1. The primary object in the treatment of placenta prævia is the arrest of hemorrhage.

2. This should be accomplished, if possible, with safety to both mother and child.

3. As both lives may be lost in trying to save both, the life of the mother should not be hazarded by undue endeavors to save that of the child. Or, in other words, the life of the mother should be preferred to that of the child.

4. In cases of parturition before the seventh month, and in cases of dead and non-viable children, the paramount and only duty is towards the mother.

5. In cases of sudden death of the mother from overwhelming hemorrhage during and after the seventh month, the urgent necessity is towards the undelivered child.

6. In such cases, immediate delivery should be effected, and long-continued efforts at restoration of the infant should be persisted in.

7. Immediately on the occurrence of hemorrhage, the patient should be placed in bed, with hips elevated; should have cold applied to her externally, and take opiates and astringents.

8. If these means fail, and the os tincæ is too small or too rigid to admit of appropriate manipulation, the tampon should be associated with the above means.

9. The best tampon consists of small compact sponges, moistened with cold water. Its styptic power may be increased by wetting the sponges with vinegar, lemon juice, a solution of alum or of tannic acid. The sponges should be introduced, one after the other, until the vagina is well packed, and, if necessary retained by a T bandage.

10. The condition of the os uteri may be ascertained without removing the tampon, by passing the index finger between it and the wall of the vagina.

11. Should no pain follow the use of the tampon, or no available change occur in the condition of the os uteri, the sponges should be removed in two or three days, or when they become offensive; and again replaced, provided the hemorrhage has not ceased.

12. If parturient pains exist at the time, or supervene after the introduction of the tampon, we may await its expulsion by the uterine efforts, or we may remove it, and then act according to the circumstances to be hereafter stated.

13. In deciding upon more active measures, we must consider the general condition of the mother, the condition of the child, and the condition of the organs of generation.

14. The character of these measures, as well as the time of adopting them, and the choice of one or the other, depend upon the above conditions.

15. The measures referred to are:

a. Partial separation of the placenta, rupture of the membranes, and delivery of the child by turning.

b. Partial separation of the placenta, rupture of the membranes, and the delivery left to nature, or aided by ergot, or by the forceps, or by both.

c. Total separation of the placenta before delivery.

16. When the os tincæ is open, or dilatable, so as to admit the hand readily, and the child is living, and the mother's strength not wasted, the placenta may be partially separated, the hand introduced through the gap thus made, the membranes ruptured, and the child delivered by turning.

17. Or, the same circumstances existing, the placenta may be partially separated, the membranes ruptured, the loose flap of the placenta compressed by the hand, ergot administered, and the delivery left to nature or aided by the forceps, and by firm, uniform compression of the abdomen.

18. If, however [the same circumstances existing] the child be dead, or non-viable; or, if the child be living and viable, and the mother greatly prostrated by the loss of blood; the placenta should be entirely separated, the membranes ruptured, the delivery left to nature, or aided by the forceps, and the contraction of the uterus stimulated by ergot and maintained by external pressure.

19. And, also, when the os uteri is rigid and undilatable, and the hemorrhage excessive and uncontrollable, hazarding the life of the mother,

even if the child be living and viable, there should be a total separation of the placenta, rupture of the membranes, and a watchful postponing of further action until the parts of the mother will permit the necessary manipulation.

20. Delivery by the forceps, or by turning, should not be attempted when violence has to be employed in entering the os uteri.

21. In great prostration from hemorrhage when the os uteri is dilated or dilatable, either partial or total separation of the placenta should be preferred to turning.

22. In great prostration, when the os uteri is not dilated or dilatable, and the hemorrhage cannot be controlled, the total separation of the placenta is the only safeguard.

23. In cases in which the os uteri is open or dilatable, the child living and the patient's strength maintained, partial separation or turning is to be preferred.

24. In cases of partial placenta prævia, after rupturing the membranes, the loose flap of the placenta should be pressed against the soft parts of the mother by the hand until the head of the child takes its place, and afterwards the delivery managed as heretofore directed.

25. So long as the soft parts of the mother are not prepared for the delivery of the child, it should not be attempted; and as soon as they are prepared, if the placenta be still adherent, and the hemorrhage continue, it should not be unnecessarily delayed.

The above propositions are offered as general rules of practice to guide us, but to be varied from as rare, exceptional cases occur.

PROFESSOR CHRISTISON ON HOMŒOPATHY.—In his very able address at the meeting of the British Medical Association, Professor Christison said that in the history of physic it was long the fashion to be never without a popular therapeutic theory. As every one knows, there was a time when the medical world, almost with one accord, believed in the doctrines of solidism, as ruling the nature of diseases, and not less the mode of curing them. At another time the doctrine of humoralism, not less prevalent, concentrated the attention on the fluids of the body as the seat of diseases, and the medium to be influenced by the remedies. Afterwards in this country the theory of Cullen taught us to look to peculiar states of the capillary circulation as the source of disease, and the proper object of the action of remedies, and the nervous system as the mechanism for developing the necessary action. A great revulsion had followed, and facts alone had come into demand, and though a tendency had been exhibited, in regard to some diseases, to fall back on a kind of humoralism, no therapeutic theory had for many years taken firm hold of professional favor, or outlived an ephemeral existence. It was to this unpreöccupied state of the professional world that they owed the partial success in these days of the grandest imposture in the history of modern therapeutics. (Hear, hear.) All other quackeries which daily flourished and perished before them were simply empirical quackeries of the coarsest kind, and success with them was a compound ratio of fortunate lying and lavish advertising. Hence it was that such quackeries presented little or no attraction to the medical mind; it was rare that a medical man had adopted any quack nostrum except his own. (Laughter.) But the famous author of homœopathy took his measures better. He came at a lucky hour when neither his creed nor he himself had any popular rival to contend with. He knew medical human nature well, and was quite aware that the theoretical had invincible charms for the imaginative. He had a prurient invention, and struck out at once what must be admitted to be a grand hypothesis—that like was to be cured by like—that disease was to be encountered by a parallel—and therefore, neutralizing artificial disease. It was true that he had tremendous errors to maintain in order first to found, and afterwards to apply this hypothesis. But still they were not so great as to be beyond reception by the credulous, and had he been content with stopping there, his Homion Pathos might have flourished. Intoxicated, however with his first success, he ventured a bolder stroke, and professed also the doctrine of doses so infinitely attenuated as to defy the external senses, surpass comprehension, and outrage common sense. (Applause.) Had he been somewhat more temperate in his "dilutions;" if, instead of dealing with billionths, trillionths, and decillionths of a grain, he had been content with palpable and visible hundredths or even thousandths, Hahnemann, escaping the shafts of ridicule, and descending from the heights of impossibility, would have made many more converts among the credulous in his profession. He might have founded a long-lived sect, instead of finding only a few fleeting admirers. Instead of a passing meteor he might have become a comet in the firmament of physic, brilliant though erratic; nebulous, indeed, and unsubstantial, but "perplexing nations." (Laughter.) Let them be thankful that this extravagance of his pretentions had laid bare their imposture; and let them leave him to the repose into which he and his doctrines were quietly subsiding. (Applause.)

STATISTICS OF OPERATIVE MIDWIFERY.—This is a report by the great Leipsic veteran, Dr. Meissner, on the results of his thirty-five years' midwifery practice, as far as operative midwifery is concerned. He observes, that a review of private midwifery practice must be taken from quite a different point of view to that of a public institution; for while in the latter the bulk of the cases are examples of natural labor, the private practitioner is only consulted, as a general rule when the case has become pathological. [These observations are, of course, only applicable to the mischievous continental practice of leaving cases in private practice to the almost exclusive care of midwives, the physician only being called in when the case has assumed a serious aspect, and, of course, frequently too late.] Moreover, in the Lying-in Hospital, the physician treats his cases in a suitable locality, with all he wants at hand; while in private, the practitioner is called miles away to neglected cases, having insufficient appliances, and is forced to treat his cases under the most disadvantageous circumstances.

Dr. Meissner's observations relate to 3811 women, who, he says, gave birth to 3980 children, as 136 were twin, and 2 triplet births. But there is here obviously some error in the numbers, as these do not correspond with each other. Of the 136 twin-births, in 54 there were two boys, in 35, 2 girls, and in 47, children of both sexes. Of the triplets, in one case there were two boys and 1 girl, and in the other, three girls. These 3811 labors called for operative interference in 3025 instances, dynamic aid being required in 924 of these to effect change of position. There were 1863 forceps operations (!), this instrument being required in 5 other instances for the removal of separated fungoid or polypous tumors. Turning was performed 351 times, extraction 247 times, perforation 32 times, the induction of premature labor 20 times, the Cæsarean operation 6 times, *accouchment forcé* 55 times, polypus operations 6 times, placenta separation or deliveries 447 times; 2086 boys and 1684 girls were born at full time, while there were 265 premature births or abortions, and 24 moles and polypi; 399 children were born dead, and of those living, 36 died within fourteen days. Twenty-five mothers died either before or during labor, or within sixteen days after it.

Turning.—Although this, when performed at the proper time, is usually an easy operation, executed within a few minutes, yet of the author's 351 cases, in only 222 did it prove completely successful for both mother and child. In 104 cases the child was born dead, in 2 cases the mother died from the delivery, and in 2, after this had taken place. This issue is chiefly to be attributed to the ignorance of midwives in the detection of cross presentations, and the delay that elapsed before aid was obtained. Dr. Meissner pefers turning by one foot, unless there is some indication for hastening the delivery. This rule is of especial importance when multiple births are expected, so as to avoid getting hold of two feet of separate children. In one of the author's cases of triplets all the children presented cross-wise, and six lower extremities were felt. All the children were safely delivered by operating upon one foot at a time. The one-foot delivery also gives the child the best chance of being born alive. *Cephalic* version is preferred by some practitioners, as giving the child the best chance; but it can only be tried when hastening the delivery is not an object. The author resorted to it successfully in 6 cases. In 4 of the 351 cases turning was performed by *external manipulation*, and in 3 by *spontaneous* version, the buttocks being forced by the presenting shoulder into the pelvis, and all the children being born dead.

Forceps Operations.—Of 1863 cases, in 1750 the head was the presenting part; and in 113 the forceps were used for its delivery in other presentations. Of the 1754 children delivered, 99 were born dead, *i. e.*, 12-19. This, in an operation which, carefully performed, can hardly be considered dangerous to life, is a high proportion; but the child had, in fact, not unfrequently died during pregnancy, and was in advanced petrefaction on delivery. In 10 instances the child was hydrocephalic or dropsical; in 3 the mother was already dead; there were 5 cases of spina bifida; 7 deaths took place from prolapsed funis, and 3 from the arm and head being tightly wedged in together. In 4 instances the mother had suffered from repeated convulsive paroxyisms. In other cases the application of the forceps had been repeatedly attempted by preceding practitioners, or the passage was narrowed by the presence of tumors. Dr. Meissner remarks, by the way, that the separation of the epidermis is not always a certain sign of the death and putrefaction of the child, as it may be produced by an acrid condition of the *liq. amnii.*

Extraction.—This was performed in 247 cases, and became necessary when, in breech knee, or foot presentation, the child's life was threatened by cessation of pain, faulty position, or prolapsus of funis. Only 145 of the children were born living; but then, 13 had died through pressure on the funis, before the author's arrival; 18 were in a state of petrefaction, 10 were already born except the head, 8 were immature, and in 5 others there was hydrocephalus, or other form of dropsy. In 42 instances extraction had to be performed on account of cessation of pain after turning.

Perforation.—In his thirty-six year's practice, this operation has only been performed by the author thirty-two times, and he had attended 3299 labors before he had his first case. He has always followed the maxim laid down by the first German practitioners, of never proceeding to the operation until assured of the child's death; and it has several times happened to him to see living children born in cases which have been left for days together to the powers of nature, and which in previous labors had been delivered by perforation.

Premature Labor.—So much has the performance of the operation for premature labor limited that of perforation, that while to the year 1842 he had to resort to perforation in twenty-eight instances, he has during the subsequent fourteen years only performed it four times. He has never induced premature labor prior to the thirty-sixth week, and has never found this too late, the bones continuing thus long sufficiently soft and yielding to accommodate themselves to the narrowest pelvis. When the reckoning is uncertain, he performs the operation thirty-six weeks after the last menstrual period.

Forced Delivery (*Accouchement Forcé.*)—By this term the author understands the whole series of operations (as artificial dilation of the os uteri, bursting the membranes, turning, extraction, or removal of the placenta,) which may be required for delivery when the further continuance of pregnancy is dangerous to the mother and child. It is especially called for in certain cases of eclampsia, placenta prævia, and obstinate vomiting. He enumerates under this head two instances of opening the adherent os uteri by means of the knife, and eight cases of its forcible dilatation. This last procedure was resorted to because, after the labor had continued three or four days, in place of dilatation of the os uteri, general debility and delirium set in. The author has resorted to it fifty-five times during thirty-six years.

In thirty-three of these cases both mother and children did well, although, as the dilatation was usually undertaken for placenta prævia, most of the latter were born some weeks too soon. As the majority of cases (31) were examples of placenta prævia, in which hemorrhage had continued long before the patients were seen by the author, it is not surprising that ten of the mothers died; but another statement of the author, that even when he arrived at his patients' he plugged the vagina, and awaited pains before proceeding to deliver, is somewhat extraordinary.

Cæsarean Operation.—Of the six examples of this that have fallen to the author's lot, five were performed on mothers being already dead, the children being saved in none. In the case of operation upon the living subject, both mother and child lived.

Placenta Removals.—The number of these (447) must seem very large; but it is to be remarked that perhaps not one-tenth of these cases were examples of abnormal adhesion requiring separation, the placenta being frequently detained from other causes, preventing the success of the ordinary manipulations, as "stricture of the uterus," spasmodic contraction of the uterus from too early interference, etc.

General Results.—Of the mothers, 41 were lost; 25 during, and 16 after delivery. Of the former 25, 11 were already lifeless when seen; and of the fourteen others, 1 died from rupture of the omentum with internal hemorrhage, 10 from placenta prævia necessitating forced delivery, 2 from nervous shock after favorable labor, and 1 from hemorrhage. Of the 16 mothers who died after delivery, 1 died from cancer of the stomach, 1 from pneumonia, 3 after repeated attacks of eclampsia, 1 from putresence of the uterus, 1 from typhus following the birth of a putrid, premature child, 4 from puerperal fever following operative procedures, 1 from "paralysis of the lungs," 3 from consequences of loss of blood, and 1 after several hours' operative attempts by a country practitioner.

Of the *children* 399 were born dead, as already stated, under the various operations. Besides these, 36 died within the first fourteen days after birth; viz., 4 from debility from too early birth, 6 from atelectasis pulmonium, 2 from trismus, 1 from fissure of the cranium after a forceps operation, 1 from chronic hydrocephalus, and 2 from want of breast-milk.

After remarking upon the remarkable consequences met with in practice of unusual pathological occurrences, and of the operations required for their relief, the author observes, that, as a general rule, forceps operations are found to be most frequent in cold, changeable weather which induces rheumatic affections of the uterus, not only rendering the dilatation of the os very painful, but delaying its accomplishment for days. This condition may be often prevented by clothing warmly the lower part of the person; and when it is present it should be treated by Dover's powder. The standing too much over the fire, also, may, by over-heating the abdomen, lead to a plethoric condition of the anterior wall of the uterus, which may not be without its influence in inducing morbid adhesion of the placenta. Such an occurrence is best prevented by abstaining from this practice, and bathing the abdomen with cold water. When adhesion has taken place repeatedly at the same place, in consequence of an indurated condition of a portion of the uterine wall, we should, after the termination of the puerperal condition, endeavor to induce absorption by mild mercurial or iodine frictions, tepid baths, together with hemlock and mallow injections. If these do not succeed, the baths of Krankenheil, near Tolz, in Bavaria, which have been found so useful in fibroid and hypertrophy of the uterus, should be tried.—*Monatschrift für Geburts Künde*, band ix., pp. 19-72.—*Med. Times and Gazette*, *May* 29, 1858.

INTERESTING CASE.—*Dr. Flint.*—Dear Sir: I send you the following case, which, if thought to be of sufficient importance, you are at liberty to publish:

April 16, 1858, a lad, aged 15, fell asleep lying upon the damp ground, and at the expiration of two or three hours, awoke chilled and with a sore throat. On the evening of the next day I was called, and found the lad suffering from a sensation of impending suffocation, caused by extensive inflammation of the tonsils and all the glands about the mouth, and from his peculiar and difficult breathing and loss of voice, I judged extensive inflammation of the larynx and trachæ. It was with much difficulty that I could separate the teeth two lines. The period for depletion had passed. Hoping to accomplish something by the revulsive effect of an active cathartic, I prescribed calomel, grs. xxx., followed by an infusion of senna and jallap, to be administered as speedily as possible, which was very slow in consequence of great difficulty in deglutition; sinapisms to the throat. At the expiration of four hours, the bowels were freely evacuated, much to the relief of the patient.

Prescribed calomel, grs. ii., opium, gr. i., every three hours, and antimony, grs. ii.—ʒ, a tea-spoonfull every half hour.

April 17, *morning.*—The patient much relieved of the distressing pain of suffocation.

April 18.—I was called in great haste, the patient supposed to be dying. On my arrival I was a little surprised to find my patient conversing with ease compared with previous day, and his general aspect much improved. The cause of the alarm I found to be, that the patient was thought to be strangling to death, and whilst strangling he had thrown off a perfect cast of the trachæ, measuring four inches in length, and very thick and firm. It being carelessly laid down, the house-dog seized and speedily swallowed it, probably supposing it to be his dinner. I lanced both tonsils which discharged pus freely, and on the following day (my patient being a robust lad) he was walking about the farm.

LYMAN CONGDEN.

Jackson, Thompkins Co., N. Y., Sept. 4, 1858.—[*Buffalo Med. Jour.*

RESEARCHES INTO ODIC PHENOMENA.—DISCOVERY OF A NEW COSMAL AGENT.—*From Letters of Baron Reichenbach, of Vienna. Extracted and Translated by Dr. G. Bachman.*—Baron Reichdenbach, of Vienna, recently published the results of his experiments on sensitive persons of marked nervous temperament, who are capable of perceiving the manifestation of an imponderable agent, hitherto unknown, and unappreciable by persons not endowed with a nervous temperament. The Baron gives to this agent the name of *Od.* He refers to a future one of the series of letters in which he publishes his observations, to explain the etymology of the term.

The first peculiar phenomenon, which was perceived by a lady of his acquaintance, and which led him to experiment further, is this: A large mountain-crystal, which had a flat base and pointed apex, was lying horizontally across the corner of a table, projecting at its two ends. The room was completely dark, which darkness was broken by a blue light, emanating from the sharp end, in an undulating, scintillating motion, and a yellow flame issuing, with a similar movement, from the blunt end of the crystal. On subsequent experiments with hundreds of other sensitive persons, as he calls them, he found this remarkable phenomenon confirmed, and also found that these persons, on approaching the crystal with the left hand within three inches, or so many feet, (if the crystal be a large one,) would experience a cool breath issuing from the sharp end, and a luke-warm one from the base end. The former is accompanied by a sensation of agreeable, exhilarating freshness; the latter by repugnance, and sometimes nausea. The Baron, startled at these singular manifestations, was at a loss to refer them to any known physical and physiological laws. It cannot be heat, for there is no source of heat, and the thermoscope remains unaffected by it. Electricity it is not, for there is no electro-excitor at play; the electroscope remains, also, unaffected by it, and conduction according to electrical laws, is fruitless.

Magnetism it cannot be, for crystals are not magnetical; nor can it be light, as mere light is not attended by sensations of warmth or cold.

The Baron being convinced of having discovered a new dynamid, carried his investigations further, and next tested its effects and presence in the sun. He placed the observer in the shade, holding a glass rod, or even wooden stick in the left hand, ordering the rod or stick to be held in the light of the sun. According to our received notions of conduction and radiation of heat, we should expect to hear the observer say that a sense of heat in his hand was the result of the test, but the contrary takes place. The hand will feel cool, and immediately on retracting the rod into the shade, a sensation of heat will follow.

These are the circumstances which reverses the order of things as perceived until now,—the rays of the sun do produce in certain conditions just what they are expected to obviate, cold—and this cold, the subjects perceiving it will tell you, has an effect analogous to that which that issuing from the pointed crystals manifests. If this phenomenon just described is of the nature of the *od*, it will, like the crystal, emanate light in the darkness. Thus sylogizing, the Baron conducted a copper wire from the light of the sun into a darkened room, in which representatives of the temperament mentioned heretofore were posted. Immediately the persons belonging to that class observed the wire glowing, and a flame of the size of a finger rising from its end. Go a step further and throw the colors of the rainbow through a glass prism, on the next wall, let the nervous individual catch the reflected colors by means of the glass or wooden rod, on the mere hand; the effect will be as follows: The blue color will create a pleasant, refreshing sensation, more so than from the light of the sun, the yellow will produce the known lukewarmness of the odic tests already mentioned, and the red will increase the latter effects and fatigue the arm

This seems to furnish an index at the same time, that caprices which individuals show in regard to color, may have another cause than mere optical impression. Aside from the odic effects of the colors, the Baron proves the *od* nature of the solar rays. Polarize them in the known manner, that you let them pass through a bundle of a dozen glass discs, on an angle of 35 °; then let the sensitive observer throw the rod alternately into the direct and reflected light, and the result will be an alternate sense of cold and lukewarmness, as the rod passes the direct or reflected light.

Further: place a glass of water under the direct or reflected light, or under the blunt end of the sharp end of the crystal, or under the blue color of the artificial iris and the yellow of the same, and that which produces coolness of the reägents cited, will acidulate the water, after being from five to ten minutes under its influence, and that which produces the lukewarmness will cause the water to taste bitter and nauseating to the sensitive, while the former he will drink with pleasure, and think it refreshing.

The experiments in respect to the solar light will be polarily reversed if the moonlight is experimented on.

The Baron promises to show, in his future letters, what the influence of his discovered cosmic dynamid is on the entire universe.

SUPERIOR SALUBRITY OF ENGLAND.—The Registrar-General, in his last report on births, deaths, and marriages, says, that it is now well established by extensive observation, that England is the healthiest country in Europe. France stands next to England in salubrity. In the continental cities the annual rate of mortality is seldom less than 30 in 1000; and frequently as high as 40. In London the rate of mortality is only 25 in 1000. Statistical records prove that "the climate of England is eminently salubrious;" and it has not yet been shown that the climate of any part of the Continent is more salubrious than this island—crowned with hills of moderate elevation, sloping towards the east and south; bathed by the showers of the Atlantic; drained naturally by rivers running short courses to the sea, cultivated more extensively than other lands, and producing those unequalled breeds of sheep, cattle, and horses, which flourish only in healthy places. The healthiest parts of England are not yet places of general resort, but the annual mortality in the various districts comprising watering places seldom exceeds 21 in a 1000 of the population, and is probably lower in these regions of the districts to which visitors resort. The lowest mortality at the English watering places, as they are rather vaguely designated, occurs at Eastbourne—only 15 in 1000; Worthing, the Isle of Wight, Mutford (including Lowestoft), Barnstaple (Ilfracombe inclusive), and Anglesey, 17 in 1000; Hastings, Upton-on-Severn (including Malvern), and Aberystwith, 18 in 1000; the Isle of Thanet, Newton Abbot, (including the east and south-east of Devon,) 19 in 1000. After these the rates of mortality rise gradually to 23 and 24, which numbers represent the somewhat less salubrious districts of Yarmouth and Bath. Clifton also stands as high as 23, but a part of Bristol is included. Tunbridge Wells stands at 20, Dover at 21, Cheltenham at 20, Warwick (Leamington), at 20, Derbyshire (Buxton, Matlock, etc.) at 20, Scarborough at 21, Harrogate at 20, Whitby at 21, Kendal at 20, and Bangor at 21. "It should always be borne in mind," says the Registrar, "in selecting places of resort, that through the peculiar nature of zymotic diseases, places usually healthy are periodically visited by epidemics, which can only be avoided by consulting recent returns, or by actual inquiries on the spot. The cleansing and sewerage of all watering-places require improvement, as their arrangements were made when sanitary science was at a low ebb."—*Med. Times and Gaz.*, October 9, 1858.

Quackery.—In quackery it is difficult to say whether the knavery of the charlatan or the silliness of the dupe is in the state of highest perfection. It little matters what the cajolery be, there are fools large enough and small enough to adapt themselves to it. A month or two's stay at a fashionable "hydropathic establishment," with the sequela of a little account of a cool hundred, and the box of pills, price thirteen-pence halfpenny, for curing the "swollen legs from varicose veins, or from whatever cause," of an old washerwoman, represent the poles of a battery which transmit their current of noxiousness through almost every grade of the social community. If there be one privilege which persons of a certain class seem to be more tenacious of than another, it is that of being allowed to be "taken in and done for." It is a patrimony common alike to the noble and the clown, and one they both seem determined shall continue to remain with them. But we were scarcely prepared for the revelation that *Yorkshire*, that county of "'cute chaps," had a climate wonderfully congenial to the growth of quackery. Yet such is the case it seems, if report speaks true,* and we must henceforth relinquish the conventional opinion, that Yorkshiremen are not easily taken in, and accept the truth, that if they can *bite* they can also *swallow.* One of the greatest quacks that ever preyed upon that portion of the public that has more *pains* than *brains*, was MORISON, the so-called "hygist." Although a fact not generally known, he was, at the beginning of his career, a *street* charlatan, and is said to have been the inventor of that style of quack oratory, or *patter*, which is employed at the present day by his illustrious successors. Now this fellow, MORISON, we are told by FELIX FOLIO, "as an itinerant, was most successful in Yorkshire," and the inhabitants of this county, however wide awake in their opinion, "were certainly not too far north for old MORISON." REBECCA and her children—we mean those of the principality of course—are, it seems, also easily attracted by the slightest manifestation of the genius of humbuggery—witness the following confession of a daring scoundrel:

"Talk about 'stonishing the people! I'll tell you what I did once in Wales. I had been drinking, and got regularly hard up. I wanted something fresh to take the people's attention, so I got a *black leather shoe lace*, suspended it in water in one of them there transparent bottles, and labelled it—

"'This Black Worm was passed from a Man's Stomach yesterday, aged 45 years.'

"Crickey! didn't it take! A black 'un had never been heard tell on before. I believe half the poor people in that part of the country came to see it; not one on 'em ever knowed but what it was a worm. I took good care to have the bottle sealed up, and I know I never laid out a ha'penny better in my life; it gave me a good start up again."

But the Londoner should not see the beam in his brother's eye only, but should remember how large is the one in his own. Galvanic rings, electro-galvanic chains and batteries, mesmeric passes, pepsine powders, Old Dr. JACOB TOWNSEND'S Sarsaparilla, and Young Dr. JACOB TOWNSEND'S still more superior article, together with a legion of other absurdities, have been philanthropically placed at the disposal of the inhabitants of the Great Metropolis by their inventors, and have been each, for the time, accepted as all that could be desired. Were the result of all this delusion only the loss of money to the dupe, we should hold our tongues, thinking the punishment well-merited. But the proceeds of quackery have often other than financial relations. Men are made to suffer in mind and body as well as in purse, and to suffer in body even to a fatal extent. If a Manchester cotton-spinner were to "wind *eight hundred* yards on a thick spool, and lebel it *one thousand*," and find a purchaser for his cheap "dodge," he would certainly rob his customer and degrade himself. But there is the end of it, or, the cotton out, the sewer must only buy more. But when a wonderful "herb-powder" is sold to cure the "asthma," or a notorious quack is consulted, who administers some teaspoonfuls of pulverized herbs, the unfortunate victim is occasionally cut short in his career —in other words *he is slaughtered.* Yet if so, who cares about it? What punishment does the charlatan receive for his quacking practices? What care is taken to prevent the recurrence of such mischief for the future? Our readers know what answers must be made to such questions. But change the scene; let some over-worked and fatigued practitioner be hurried, almost against his will, to a distant disaster, and let rumor arise that he mistook for a few days a dislocation for a fracture, or allowed a labor to continue unassisted beyond the time that is generally deemed proper for the parturient woman, and what an outcry is made about it! The whole neighborhood is in arms; the judge lectures, the jury express their lamentations, heavy damages are awarded, and the, perhaps, once-erroneously-judging practitioner is totally ruined. Why? because, as it is asserted, her Majesty's subjects must be protected from erroneous medical treatment; the medical practitioner undertook to do that which he did not properly accomplish, and hence must be punished accordingly. But the quack—what of him? Oh, nothing: you may swallow his lobelia powder of gamboge pills, be killed by his "universal medicine" or the vegetable panacea, have your leg tortured by his bone-setting into a fatal gangrene, and we shall permit you thus to be dealt with to your heart's —and the undertaker's—content. It is your own affair—we cannot say the enlightened jury interfere between the ministering angel and the afflicted martyr!

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* The Hawkers and Street-dealers of Manchester and the North of England Manufacturing Districts generally. By Felix Folio. Manchester: Heywood. London: Grattan. 1858.

A correspondent of the *Athenæum* (Aug. 14th) has denied the truth of the above, maintaining that the quack Morison, was a retired "officer in the army, lived in No. 17 Silver-street, Aberdeen, a house belonging to Mr. Reid, of Souter and Reid's, druggists. He obtained the use of their pill machine, with which he made in their back shop, as many pills as filled two large casks. The ingredients of these pills, however he may have modified them afterwards, were chiefly oatmeal and bitter aloes. With these two great 'meal bowies' filled with pills, he started for *London*, &c." Can the correspondent say, "Ize Yorkshire too?"

Opium Smoking.—Opium is not smoked in the same manner as tobacco. The pipe is a tube of nearly the length and thickness of an ordinary flute. Towards one end of it is fitted a bowl of baked clay or some other material, more or less precious, which is pierced with a hole communicating with the interior of the tube. The opium, which before smoking is in the form of a blackish viscous paste, is prepared in the following manner: A portion of the size of a pea is put on a needle, and heated over a lamp until it swells and acquires the requisite consistence. It is then placed over the hole in the bowl of the pipe, in the form of a little cone that has been previously pierced with a needle, so as to communicate with the interior of the tube. The opium is then brought to the flame of the lamp, and after three or four inspirations the little cone is entirely burnt, and all the smoke passes into the mouth of the smoker, who then rejects it again through his nostrils. Afterwards the same operation is repeated, so that this mode of smoking is extremely tedious. The Chinese prepare and smoke their opium lying down, sometimes on one side, sometimes on the other, saying that this is the most favorable position; and the smokers of distinction do not give themselves all the trouble of the operation, but have their pipes prepared for them.—*Le Huc's China.*

Application of Sugar when Lime has entered the Eye.—The *Indicateur de Mayence*, in relation to cases of workmen becoming blinded by the action of lime which has entered the eye, recommends, as a well-approved application in the case of such accidents, a strong solution of sugar, which is to be inserted drop by drop under the eyelids. This application can usually be immediately obtained, and completely prevents the caustic action of the lime.—*Dublin Medical Press*, from *Journ. de Chimie Méd.*, Aug., 1858.

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TRANSFUSION IN YELLOW FEVER—A SUCCESSFUL CASE.

By N. B. Benedict, M. D., of New Orleans.

Messrs. Editors :—In consequence of the pressure of indispensable engagements, I am able to furnish you only a brief outline of the circumstances attending the case of transfusion which was successfully practiced in this city, on the 25th of October last.

The patient, Miss J. B., was a young lady whose life, until early womanhood, was passed in north-western New York, whence she removed, in 1845, to Mississippi, and thence, in 1854, to New Orleans, where she has constantly resided during the past four years: never having suffered any serious illness within the last fourteen years.

On Monday, October 11th, she was exposed to a drenching rain, but felt no alarm at the sensations which followed, (supposing them to be merely those of a bad cold,) until the evening of the following day, when she was seized, at 9 o'clock, with a violent chill. The symptoms of yellow fever—the pains in the head, the back, and the limbs, the flushed face, the fiery-red eye, and the rapid pulse—could not have been more characteristic than in this case. It proved unusually obstinate—the treatment exerting little influence upon any of the symptoms, during the first stage of the disease. My near relationship to the patient induced me, on the third day, to request the assistance of Dr. C. B. White. On the fifth day the counsel of Dr. Wm. E. Kennedy was requested, and both those gentlemen continued their attendance, daily, throughout the course of the disease. On this fifth day, there was some bleeding from the mouth; but the flow was moderate, and occasioned no alarm. On the sixth, it continued, but was so slight as to be regarded as marking a favorable tendency. The average pulse, on these two days, was 86—ranging from 80 to 92. On the morning of the seventh day, the eye presented an appearance as if a drop of serum had collected in the outer canthus; the bleeding from the mouth had increased, and the appearance of the napkins employed to absorb the blood, agitated her excessively; but at the mid-day visit, although the hemorrhage was unabated, yet so much had the complexion improved, that the impression on the minds of the medical attendants was, upon the whole, favorable. The hemorrhage increased until a late hour in the evening, being aggravated by the uncontrollable perseverance of the patient in wiping away the clots. It suffered a slight check during the night, but returned on the following morning. Throughout the eighth, and most of the ninth day, it continued without abatement; and her efforts to clear the mouth by hawking, rinsing, spitting, and wiping, not only increased the flow, but occasioned great fatigue. To prevent these efforts, except by a resort to unjustifiable force, was impossible. The effects of such protracted hemorrhage were apparent in the blanched complexion, the colorless lips, tongue and gums, and the pinched features; and serious alarm began to be felt at a loss which resisted all the measures employed to restrain it. Some estimate may be formed of the quantity lost, from the fact that, in the single night of the eighth day, the blood diluted with saliva, saturated two sheets, so that no part of them remained unsoiled. Added to all this was the hemorrhagic tendency manifested by other mucous surfaces besides those of the mouth, during these two days. Towards the close of the ninth day, the hemorrhage from the mouth was several times interrupted for short intervals; but owing to the discomfort caused by the clots, she provoked its return by removing them. About 8 o'clock, P. M., she had a sudden imperfect syncope. Thence forward the bleeding became very slight, and although nourishment and stimulants were retained, the complexion was much blanched, and the mental condition was that of utter despair as to recovery. The rate of the pulse was steadily accelerated; the average for the seventh day being 98, for the eighth and ninth days 104, for the tenth day 115, and for the eleventh and twelfth days 120.

On the tenth day there was much complaint of soreness of the throat, the tongue, and the inner surface of the lips (which were found to be invaded by small superficial ulcers); of severe pains in the sides of the head, at the tops of the ears; of morbid sensations—at one time, as if ashes filled the throat—at another, as if the feet were much more elevated than the head; and there was uncontrollable desire to talk of affairs and of interests the most foreign to a sick room; yet, all was perfectly rational. In the course of this night, the hemorrhage ceased, and did not again return. The eleventh and twelfth days were characterized by a continuance of those nervous vagaries, and by the increased frequency of the pulse to 120.

At 3 o'clock, in the morning of the thirteenth day, she had profuse perspiration, and a paroxyism of hysterical weeping. The general surface, as well as the extremities were cold and clammy to the touch; the pulse so feeble and so rapid as not to be counted; and the restlessness, and desire to toss about the limbs, was excessive. Under the use of stimulants, this condition was relieved, the natural warmth was restored, and she fell asleep. At 7, A. M., the pulse had receded to 112, and was regular though feeble; and nearly every portion of the surface—the limbs as well as the trunk—was found to be covered with sudamina. At 10½, A. M., the apparent amendment was so marked that the medical attendants were more hopeful than on any previous occasion. The pulse had fallen to 108. From the sixth day to this time, nourishment and stimulants had been taken in quantities such as to encourage the belief that the terrible waste of blood would be repaired by the ordinary process of assimilation. At 11½ o'clock, she awoke from a quiet sleep, complained of strange and bad sensations, and said she thought she must be dying. She presently turned upon her side and was suddenly seized with vomiting, which continued throughout the day and part of the following night. Nothing was retained by the stomach, with the single exception of brandy, and even that only occasionally. After mid-day, an enema of mustard and water was given, but did not return. It was followed by other enemata of warm water, with no result except distention of the abdomen. The sinking and prostration were excessive; the pulse was scarce perceptible, yet its frequency was not found, at any time, to exceed 108; and she exhibited a constant tendency to lapse into stertorous slumber. Over the whole region of the aorta was distinctly heard the bellows-sound so characteristic of anemia. Injections of beef-tea, brandy, and carbonate of ammonia were repeatedly given, only to be rejected; but the action of the bowels seemed limited to the rectum. At every movement of the body distressing hiccough occurred, convulsing the whole frame, and lasting on each occasion, about a minute. A sinapism applied to the spine was succeeded by arrest of the vomiting, at 10, P. M., but there was no other improvement throughout the night of the thirteenth day.

On the morning of the fourteenth day, a stimulating enema was followed by discharges resembling coffee-grounds, and by relief of the distention, but there was no amelioration of the symptoms which steadily tended towards the fatal close. The mortal restlessness increased, the pulse became but the merest flutter, and was, much of the time, inappreciable, and its number could not be ascertained.

At the morning consultation, Drs. Kennedy and White declared their conviction that she could not possibly survive for more than three or four hours. I then said, "It is an old saying that 'drowning men catch at straws.' For the past twenty-four hours, I have been in torture with the thought of such a straw, and I cannot refrain from naming it: I mean transfusion. These fatal symptoms being occasioned by the loss of the last few ounces of blood, I cannot persuade myself that it is not a serious duty to inject into her viens a few ounces of fresh, healthy, living blood, as nearly as possible identical with that which she has lost." Both those gentlemen promptly embraced the suggestion that it was the only remedy that could save her life; but thought it impracticable for want of persons experienced in the operation, and a suitable apparatus. Having, in 1853, prepared a report on transfusion, (which was read before the State Medical Society, and a part of which was printed in *The New Orleans Medical and Surgical Journal* for that year,) I had become imbued with the conviction that the operation is one which, on certain occasions, cannot be withheld without criminality; and I had obtained from England the apparatus for the purpose, which was approved by the father of transfusion—Dr. James Blundell, formerly "Lecturer on Midwifery and Physiology at Guy's Hospital." The possession of such an instrument at once gave form to the proposition, and it was determined that if a person could be found who would undertake the most difficult and delicate part of the process—that, namely, of preparing the vein of the patient—the operation should be attempted. A little past the hour of noon, there were assembled at my house, Drs. Wm. E. Kennedy, C. B. White, C. C. Beard, D. W. Brickell, and L. Greenleaf. The apparatus was examined, its use described and illustrated, and every part of the process was fully explained and discussed. The task of preparing the vein was undertaken by Prof. C. C. Beard. An incision about two inches in length was made over the median vein in the left arm. A director was passed beneath the vessel, near the lower part of the incision, in order that it might be held under perfect control, and the loss of any blood from its distal extremity be prevented. An incision was then made into the vein, immediately beyond the director, to receive the beak of the syringe. It is due to Prof. Beard to say that nothing could exceed the skill and steadiness with which this operation was performed. The blood was obtained, by the assistance of Dr. Greenleaf, from the arm of a young gentleman who exhibits a remarkable example of perfect health, and who had experienced yellow fever during the epidemic of 1853.

It was my purpose to use the apparatus myself; but as the moment approached, my near relationship to the patient, made me distrust my firmness, and I requested Prof. Brickell to take my place. He consented to do so, but substituted for the syringe belonging to the apparatus, one of simpler construction, into the beak of which he absorbed the blood as it flowed into the receiver of the transfuser. By inverting the syringe and pushing upward the piston, the last bubble of air was expelled. The beak was then introduced, by Dr. White, into the orifice of the exposed vein of the patient, and Prof. Brickell, with consummate care, passed the blood into her arm, before it had time to cool or even to repose for more than a few seconds. All the apparatus employed was immersed in warm water, or wrapped with heated cloths, so as to prevent any reduction of the temperature of the transfused blood. The operation was commenced at a few minutes before 1 o'clock, and was finished safely and satisfactorily, in all respects, a few minutes past that hour.

Another person who was constantly with her, writing to a distant friend, used the following language: "There is no doubt that Death had begun his work, before this took place. Her extremities were cold; she swallowed with difficulty; her nose had the pinched look of death; her lips were depressed and bloodless, covered with a yellow, dry, parched skin; and her distress was very great. The effect of the transfusion was immediately apparent in the calming of the nervous system. The next morning, her lips were full and red; I picked off the yellow parched skin, and found them as natural as health; and she had no subsequent bad feelings except dryness of the mouth, and stiffness of the muscles of the throat. She was like a new creature and was saved."

I will merely add to this statement that the pulse which at half past 12, under the influence of mental excitement, became once more appreciable, numbered 136, and immediately before the operation, 125; at its termination it was 120; and three hours later it remained the same, but had acquired more fullness and strength. The voice recovered its natural tone, the face acquired color, the extremities grew warm, all nausea and hiccough ceased, and ordinary drinks were perfectly retained. From the time mentioned, to the present hour, her recovery has been uninterrupted. Her health has long since ceased to be a subject of any anxiety.

It was not doubted at the time of the operation, that the quantity of blood injected was equal to three and a half ounces. It was afterwards ascertained, by accurate measurement of the syringe, that its capacity was not quite equal to two and a half ounces. Small as the quantity was, it yet sufficed to turn the scale in her favor.

I have thus, Messrs. Editors, endeavored to give a plain statement of the facts as they occurred. I have been the more particular in these details, because I am informed that an erroneous impression has, to some extent, prevailed that I was the sole operator. The error is attributable to ignorance of the complicated nature of the operation, and to misconception of the flattering notices which appeared in a daily paper of this city, shortly after the event. Transfusion was not adopted without the consent of men who are among the first in the profession; and I venture the assertion that it never will be safely performed, on the human subject, without the assistance of four medical gentlemen at least. Nothing could be more mortifying to me than the acquiring of professional distinction by any means but such as are recognized by the medical profession as legitimate, unless it should be the acquiring of distinction which is unmerited. I trust that you will appreciate, therefore, the propriety of these explanations being made in your journal, inasmuch as justice required details which are improper in a newspaper.

Were the true nature of the statistical results of the operation of transfusion generally known, its performance would be demanded in many cases which are now consigned to a remediless doom. There is no fear that it will ever come to be employed as one of the common remedies. It is applicable to no pathological condition save that which is commonly called "collapse," induced by hemorrhage, by certain exhausting discharges, or by utter inability to receive or retain nutriment; and the only transfusion now sanctioned, either by physiology or by common sense, is that of *human venous blood into human veins, identical, as nearly as possible,* will ever come to be employed as one of the common remedies. It is applicable to no pathological condition save that which is commonly called "collapse," induced by hemorrhage, by certain exhausting discharges, or by utter inability to receive or retain nutriment; and the only transfusion now sanctioned, either by physiology or by common sense, is that of *human veinous blood into human veins, identical, as nearly as possible, with that which has been lost, and in quantity just sufficient to arrest the tendency towards death.* Prior to the year 1853, the total number of recorded cases, practiced under these conditions and restrictions, amounted to twenty-one. Of these, but two died—or less than one case in ten!

If I have any merit in this affair it is that I have been, for many years, the earnest advocate of transfusion, in those cases where alone it is proper; that I have labored to make it binding upon the consciences of medical men to practice transfusion whenever it is justifiable; that I have approved my faith in it by submitting to it, as the first instance which I am apprised of in America, a person whose life is dear to me as my own; and that before it was attempted I seriously declared to those professional friends, heretofore named, my willingness to take upon myself all the odium that might attach to a failure. I feel that I can never sufficiently thank those gentlemen for what they did for me in this the bitterest trial of my life; and that they have thus laid upon me new obligations of fidelity to the principles of our noble profession.

New Orleans, November 27th, 1858.

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COMMUNICABILITY OF YELLOW FEVER.

By R. B. S. Hargis, M. D.

U. S. Marine Hospital, *Pensacola, Fla., Nov.* 3, 1858.

Messrs. Editors:—In the last (October) number of the New Orleans Medical News and Hospital Gazette, you quote some remarks from the St. Louis Medical Journal, "showing conclusively the non-contagiousness of yellow fever." I do not question that statement, but will adduce another showing decisively that that terrible malady *is*, under *some* circumstances communicable from one person to another; and I shall not go beyond these walls for proofs.* I intend to be explicit but brief:

In September, 1854, Capt. Joseph Golder, of the bark "Alfred Exall," from Havana, was admitted into this institution with yellow fever. Having several cases of other diseases, and half a dozen or more convalescents from various affections, in accordance with a long established rule here, respecting contagious diseases, the patient was put into a separate ward, and all communication with him forbidden, except by his attendants. Being very sick, and consequently indifferent as to the rules, Golder allowed two of the convalescents to contravene them. Davidson, a convalescent, from a gastric disorder, wrote letters at his (Golder's) table, on the 20th of September; contracted the disease on the 23d; and died on the 24th, of black vomit. Bynton, recovering from a hæpatic affection did the same thing on the 23d of September, and succombed on the 25th, in a puddle of the "fatal ejection." Each case exhibited, during its progress, all the characteristic phenomena of the most malignant type of yellow fever. Cadaveric autopsy confirmed the ante-mortem indications in both cases.

This year the hospital being crowded to such an extent as to prevent insulation of yellow fever cases, six convalescents, from other affections, contracted the disease.

John Rice, recovering from an injury, had not been in an affected port during the season, unadvisedly slept two or three nights next to a yellow fever case, sickened, had black vomit, and recovered.

George Way, three or four weeks from Galveston, was in the hospital a week with intermittent fever, got well, and was to be discharged on the morning of the 6th of October, but complaining of pain in the head and back, and a sense of weariness, was suffered to remain—he died on the 9th, with black vomit.

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* Pensacola has the deserved reputation of being the healthiest city on the Gulf coast. Her sanitary condition this season has been excellent. The Marine Hospital is situated about a mile from the suburbs of the city, in a beautiful open grove, on a bluff overlooking the bay; a dense forest intervenes between it and the city.

I make this statement to show that this hospital is "situated beyond any *infected district.*" Hence, in the instances of communicability of yellow fever mentioned in the text, *the morbific cause* MUST *have been reproduced.* To adduce further proofs is idle—"*On ne cherche point à prouver la lumière.*"

John Bell, Thomas Harper, John Hefferman, and George Curtis, all from healthy ports, contracted the yellow fever at different periods. The first mentioned had had diarrhœa, the next intermittent fever, and the last a sore leg; he (the latter) died; the others recovered.

We have treated twenty-eight cases of yellow fever in this hospital since the 14th of September last; twenty-one recovered. Two of those who died were admitted *in articulo mortis.*

The following cases from the register will indicate in a measure the treatment, generally here, pursued:

Case I.—Charles Brown, aged 23 years, born in Boston; a robust fellow; admitted on the 14th of September, (1858), had been sick two days; had taken no medicine; complains of pain in the head, back and knees, and a sense of langour; has some nausea and thirst; is dizzy when sitting up; respiration prolonged; eyes injected; tongue furred; skin very warm; bowels confined. Let him have a hot mustard bath, semi-cupium; cups to the nucha; and an enema of sodii chloridum, and infus. lini., followed by pills of chlor. hydrarg. mitis ext. colocynth. co., and ext. hyosciam; also, infus. flor. sambuc. to be drank occasionally.

September 15*th.*—Has better appearance; skin a little moist; some headache and vertigo; bowels moved two or three times during last night. Gave him immediately sulph. quinine, grs. xx.; and ordered the dose repeated in two hours. Continue infus. flor. sambuc.

Same day, 5 *o'clock, P. M.*—Restless, complains of debility; skin soft; pulse 110 per minute; vascularity of the eyes increased; slight hemorrhage from nose and mouth; thirst. Prescription:

℞.	Ext. hyosciam,	grs. xv.,
	Pulv camphor,	grs. xx.,
	Muc. g. acacia,	q. s.,
	M. ft. mass, et divid. in pill., 8.	

Two every two or three hours, p. r. n., to induce quietude. Continue infus. flor. sambuc. Should hemorrhage increase during the night, give mistur. creasot. half an ounce, every hour or two.

September 16*th,* 5 *o'clock, A. M.*—Hemorrhage increased immensely; eyes sunken, voice husky; clammy sweat; pulse 120 per minute; blood oozing from nose, mouth and anus. Notwithstanding the chemical incompatibility of the substances,

℞. Tannin, grs. iij.,
Sul. quinin., grs. ij.,

Tinct. ferri chloridi, gtt. xij., every hour, with a sip of brandy julep; friction to the extremities, with dry capsicum; sinapism to the spine.

5 *o'clock, P. M., same day.*—Hemorrhage partially stopped; pulse fuller, and 100 per minute; skin warm; better expression of countenance; has taken some broth. Ordered the tannin, etc., continued at intervals of two or three hours, according as the sanguineous exudation increased or diminished.

September 17*th.*—Looks cheerful, complains of nothing but weakness; the blood had ceased percolating in the night after a copious discharge of coägula, mixed up with secretions and fecal matter. Let him have porter, and as broth is agreeable to him, let him have that also. He got well.

Case II.—Thomas Slater, aged 24 years, stout, but of strumous habit was attacked two days ago; had taken a dose of calomel and jalap which purged him freely; seems dispondent; answers questions indifferently; face red; eyes injected; skin somewhat above the natural temperature; capillary circulation sluggish; bowels unmoved since first day of attack. when he took the c. and jal., as above stated. Ordered a mustard pediluvium and pill of mass. hydrarg., ext. col. comp., and ext. hyosciam; infus; flor. sambuc. freely.

October 3*d.*—Bowels moved twice in the latter part of the night; skin warm and moist; pulse 110 per minute; tongue fured and dry; not disposed to answer questions, is dull; at times, partially stupid. Put emp. canthar to nape of the neck; pillul. hydrarg. gr. iij., every three hours. Continue flor. sambuc. infus.

October 4*th.*—Symptoms same as yesterday; pulse, however, increased in frequency and complains of "uneasiness" in his bowels; no motion since yesterday. Ordered an enema of an ounce of chloride of sodium dissolved in one pint of infus. lini, and a draught of acet. ammon., with a few drops spts. ether nitros every two or three hours.

Same day, 5 *o'clock, P. M.*—Pain in the bowels; nausea, with acid eructations; tongue covered with scarlet blood. Directed hot fomentations to the abdomen, and a spoonful of aqua calcis in some aqua menthæ every hour or two; sinapism to the epigastrium.

October 5th.—Nausea abated; bowels easy; skin of dingy appearance; eyes yellow; knitting of the brows; considerable hemorrhage from the mouth and throat. Prescribed:

℞. Tannin, grs. xviij.,
Sulph. quinin, grs. xij.,
M. ft. pulv., No. 6.

One every two hours with tinct. ferri chloridi, ♏vj., in a wine-glassfull of aqua distillat.

October 6th.—General appearance somewhat improved; hemorrhage decreasing; a little headache; bowels not moved since yesterday. Let him have an enema of solution of chloride of sodium in flax-seed tea. Continue tannin, quinine, etc.

October 7th.—Bowels moved yesterday by injection; no discharge of blood; skin moist, and of normal temperature; tongue clean; complains of debility; pulse 86 per minute; desires food. Directed ale and broth in small quantities; repeat salt enema.

October 8th.—Expresses himself much better; desires to sit up; slight perspiration. Continue ale, etc. Convalesced rapidly.

I believe that hemorrhages in yellow fever are salutary, and should no. be restrained unless profuse. I was induced to prescribe tannin and tinct-ferri chloridi in one case when the exudation of blood made its first apt pearance on the tongue, and had reason to regret it; for the hemorrhage ceased entirely after the second dose; the patient got restless; transient delirium ensued; black vomit intervened, and death soon afterwards closed the scene.

I cannot forbear to make a few additional remarks relating to your St. Louis cotemporaries and yellow fever. It seems to me the editors of the St. Louis Journal exhibit a lack of information in regard to the medica literature of New Orleans, or else you would have had no occasion to correct their erroneous expressions about unity of sentiment amongst the doctors of New Orleans in reference to the non-contagiousness, as well as *origin* of yellow fever. Now, if my memory deceives me not, you have a ternary division of the fraternity in your city. When the question of contagion comes up; firstly, you have the unqualified contagionists; secondly, the non-contagion, and thirdly the *contingent* contagionists; and the only compromise you can make is an agreement to disagree, when in my humble opinion, with such an *authentic record* of *facts* in *regard* to the subject, but one sentiment should possess the minds of all. It is this: That yellow fever, by some contingencies *is* contagious, under other circumstances *not.* This, though not very happily planned, is the "middle ground" for medical physicians of the present day to occupy, while they adjust their studying cap and endeavor to investigate the causes of this peculiarity of that singular disease.

In regard to the origin and cause of yellow fever much diversity of opinion also exists in New Orleans and elsewhere, which may be "found on paper." One says yellow fever originates in New Orleans independently of importation, and is caused by a hot sun and *whisky*; another, that it is not indigenous, is introduced from abroad, and requires a certain (unknown) condition of the atmosphere for its propagation and spread. Others, again, agree with the former, so far as relates to its local origin; but one asserts that it is caused by noxious exhalations from the city; another denies it emphatically, and declares that a vitiate, atmosphere has nothing to do with it; while another, equally earnest, attributes it to barometrical vicissitudes, or a certain condition indicated by a high dew point; another smiles at the absurdity of all these theories, and affirms that the cause of yellow fever is to be ascribed "to a febritious condition of the atmosphere," and so on; but, "to cap the climax," a new light arises, protesting against all the theories and hypotheses that were ever promulgated, and avows that *vomito prieto* is of domestic origin in America, and is caused by eating possum! Now, after all these various contradictory and absurd opinions are collected together, analyzed and compared with one another, our judgment irresistibly leads us to the conclusion that the cause of yellow fever cannot be attributed to *terrene emanations,* nor any condition of the atmosphere, whether electric, hydrometric, barometric, nor to any, nor *all* of these conditions combined; nor, I will add, does it bear any relation, whatever, to a redundancy or deficiency of ozone or "marine acid." Well, and furthermore, notwithstanding, in regard to the origin of yellow fever, the mention of Dr. Dowler's, Dr. Hart's, and others' cases occurring, in "out of the way places," I am forced to the conviction, "be its work of desolation where'er it may," *yellow fever is neither indigenous to this country, nor a native of any foreign clime.* I assume that it is in the holds of vessels, within the tropics a certain period of time, that are to be found the niduses in which the germs of yellow fever are engendered, developed, and perfected, and from them, under favorable circumstances, disseminated on reaching inhabited shores, within certain degrees of latitude.

Few or none will deny that yellow fever has often occurred aboard vessels at sea, under circumstances that forbid the supposition that it could have originated from any other focus of infection than that which existed within the vessels themselves. La Roche has adduced ample and irrefragible testimony to this fact; but by what process of reasoning he arrives at the conclusion that the noxious effluvia generated in the holds of vessels are "identical in nature" with that which is produced on land, I cannot conceive. His language certainly does not involve the inference respecting their propagation from *fomites;* for such an idea he could not entertain for a moment without the grossest inconsistency.

The combination of circumstances under which noxious gases are eliminated from the timbers, bilge-water and cargo of a ship are peculiar; materially different from those upon which depend the exhalations of filthy cities, prison houses, immigrant vessels in temperate latitudes, and paludal districts. Indeed, it is so palpable to the perceptions that it were inconsistent with the plain dictates of common sense to suppose otherwise. Moreover, the peculiar and distinctive characteristics of yellow fever, compared with other fevers, are so different, both as regards its semeiology, and morbid anatomy, that an attempt to identify them were not only supremely absurd, but absolutely humiliating to the understanding of the medical philosopher who would presume to make it. I write *sine invidia;* but am, nevertheless, in earnest. Again, admitting that the fevers of paludal districts are similar to yellow fever, as regards their extrinsic as well as proximate cause, it would be unphilosophical, if not absurd to suppose an identity because "similar causes produce similar effects." "Every thing like is not the same," is an axiom to which every day's experience testifies, and is just as applicable to the phenomena resulting from all scientific investigations, as to the appearances of nature undisturbed.

I have extended this extemporary lucubration further than I intended; yet, nevertheless, would proceed; but forego the pleasure, as I should trespass upon the claims of another paper upon the same subject, designed for future publication.

Very respectfully, yours, etc.,

ROBT. B. S. HARGIS.

SELECTIONS FROM CASES IN PRIVATE PRACTICE.

By I. L. Crawcour, M. D., Professor of Chemistry and Medical Jurisprudence, New Orleans School of Medicine.

On the Treatment of Neuralgia by Narcotic Injections.—About three years ago Dr. Andrew Wood, of Edinburgh, directed attention to a new mode of relieving neuralgic pains, by injecting solutions of opium under the skin over or near the affected nerve. Having tried this treatment in several cases, and having found it remarkably effectual, I think it a duty to lay the results before the profession. In the winter of 1855, a gentleman called on me, suffering from so called rheumatism of the shoulder. He appeared in the greatest agony, was unable to raise his arm without excruciating pain, and had passed several sleepless nights in consequence of the distress he labored under. He had exhausted all the liniments in use without effect; as the case was urgent, I determined to try the effect of subcutaneous injection. I accordingly threw up under the skin of the shoulder (as near the circumflex nerve as possible, a branch of which supplies the joint) about ten drops of a solution or sulphate of morphine, equal to about a quarter of a grain of the salt. The effect was almost instantaneous, the pupils contracted, he complained of feeling sea-sick, and in less than five minutes feel asleep. In four hours he awoke, completely freed from pain, and was able to move his arm freely in all directions, and had no return of the neuralgia. In a second case, a lady had suffered from severe neuralgic pains in the arm; I injected about five drops of the solution of morphine with the same result as in the first case, producing rapid somnolence, nausea, etc., but the pain recurred the next day in the fore-arms, a similar treatment relieved this, and after an interval of two years there has been no recurrence.

Some months ago, I was consulted by a gentleman who had labored for a long time under severe pains in the side, especially near the fourth and fifth ribs, there was no pain on pressure, the liver was not enlarged, no evidence of pleurisy, and I came to the conclusion he was suffering from intercostal neuralgia. I injected the intercostal spaces with the narcotic solution, five times at intervals of four days, the pains entirely disappeared and have not recurred. The instrument I use is the small syringe, invented by Provaz, for injecting aneurisms with the perchloride of iron. By means of the minute trocar and canula attached to it, it readily and painlessly penetrates the skin, and its graduation allows the quantity to be injected to be measured with the greatest accuracy. The solution I employ contains a quarter of a grain of sulphate of morphine in every ten minims. I rarely exceed the ten minims, and usually find five amply sufficient for all purposes. The most remarkable circumstance is the rapidity with which the effect is produced, absorbtion seeming to be instantaneous. Care must be used in the employment of the remedy, as the narcotic power of opium employed subcutaneously is at least five fold greater than when employed by the mouth.

Polypus of the Rectum Removed by the Ecraseur—Subsequent Hæmorrhage.—About six weeks ago, I was requested by Dr. Taney, of this city, to see him in consultation respecting a polypus of the rectum in an elderly gentleman under his care. The tumor measured about three inches in its length and one inch in its transverse diameter, and was attached, by a neck of about half an inch in diameter, to the mucous walls of the rectum. As the patient was very old, sixty-seven years of age, and as the ligature would have been tedious and painful, I proposed the ecraseur, to which Dr. Taney assented. It was carefully applied and tightened by half minute turns. The removal of the tumor occupied about two hours. There was very little pain after the chain became tightened, and no bleeding at the conclusion of the operation. The parts were left perfectly clean and smooth, and it would have been difficult to have seen where the tumor had been. About four hours after the operation, Dr. Taney called on me and told me, to my great surprise, that considerable hæmorrhage had occurred, and that the patient had voided a large quantity by stool. On going round with him, I found our patient weak and blanched, the bed clothes soaked with blood, and a considerable oozing still continuing. I instantly passed a plug of charpie saturated with the solution of perchloride of iron (one part perchloride, two parts water), and a few minutes after gave two very large injections of ice and water, which were retained. No further hæmorrhage occurred, and the next day the patient was well, although rather weak from loss of blood.

The case is curious, from the occurrence of hæmorrhage. Chassignac stating in his work, that bleeding never occurs after the use of his instrument, and yet, in this case, although every care was used, and although the instrument was tightened so slowly, as to occupy two hours in the removal of a small tumor, sufficient bleeding occurred, to produce considerable alarm and excite apprehension. I believe that this case is the first in which the ecraseur has been used in this city. I have subsequently removed some large hæmorrhoids, and there was not the slightest effusion of blood. It is probable in the first case there must have been an hæmorrhagic tendency.

SPECIAL SELECTIONS.

REMARKS ON LEAD COLIC.

By Dr. Briquet, Physician of "La Charité," etc.

Translated from the "Archives Générales de Médicine," by Jno. J. Castellanos, M.D., of N. Orleans.

"*Vidi in arte peritissimos hancce morbum non intellexisse,*" has been said by Tronchin with regard to lead colic, in a short treatise, in which he himself does not seem to have well understood the nature of this affection. I make bold to repeat the above sentence, and choose it as an epigraph to my work: may I, in this respect, prove more fortunate than Tronchin!

If there be a disease as to the seat of which medical opinion has been less at variance, it is assuredly lead colic. Since Paulus Ægineta, who had given to this affection (I scarcely know why) the name of pestilential colic, "colica pestifera," down to Mr. Tanquerel des Planches, to whom science is indebted for the last "ex professo" article upon this subject, the saturnine affection has always been designated under the name of colic, i. e., painful affection of the bowels. Colic of Poitoü, mineral, metallic colic, plumbers', painters', potters', founders' colic, colic of Poitiers, Madrid, Devonshire, Java, Surinam, metallico-nervous colic—such are the names it has occasionally borne.

It has been similarly considered abroad, by the English, who call it "belly-ache," *i. e.* bowel affection, as well as by the Spaniards who have attached to it the term "entripado," which likewise seems to signify affected in the bowels; thus, this affection has always been considered as resulting from a lesion in the intestinal canal, and it has always been supposed that the pain, its main symptom, was seated therein.

"We cannot be ignorant of the fact," says Mr. Tanquerel, "that the symptoms of lead colic are the result of lesion in the digestive tube extending from the stomach to the arms." We can deem this assertion as summing up the opinion of a large majority of physicians. But although they have easily agreed as to the seat of pain, it has been otherwise when the cause of that predominating symptom has been sought for, and then it is that they have shown the greatest dissidence of opinion—a certain proof of the insufficiency of their explanations.

The earliest observers thought that the pain resulted from the contact of the lead molecules with the mucous membrane of the digestive tube; they were ignorant of cases of colic produced by absorption of lead when applied to the surface of the skin.

Since Dubois, of Rochefort, many physicians have assigned as cause of the colic the over-distention of the intestines by gases, the escape of which was prevented by obstinate constipation. Post-mortem examinations, however, have never substantiated this opinion. Moreover, there are patients affected with lead colic in whom the anterior walls of the abdomen, beingretracted and flattened, do not allow the accumulation of gas. In patients with typhoid fever or hysteria the abdomen is exceedingly tumid, and yet they do not experience the pain felt in lead colic.

Certain authors, foremost of whom we may place Fred. Hoffmann, have thought that the pain resulted from compression of the contracted alimentary canal upon the ovoid excrements which it contains. Every one is aware that costiveness, which in some individuals may last twelve or fifteen days, is capable of producing some uneasiness and tenderness in some determinate spot in the abdominal region; but every one knows, also, that the pain arising from this cause is not similar to that produced by lead colic. Besides, the ovoid matter can be found in the large intestines alone, and post-mortem investigations have not proved this portion of the digestive tube to be contracted, any more than observation has pointed out satisfactorily the pretended constriction of the sphincter ani muscle.

Borden, and, after him, quite a large number of physicians, among whom we may mention Messrs. Palais and Cannet, have undertaken to prove that lead colic is the result of inflammation of the intestinal mucous membrane, but this opinion, which proved short-lived, is upheld by none at present.

A last hypothesis has been finally attempted, which has received the assent of many authors, and which I here quote, such as it has been exposed by Mr. Tanquerel. "The affection," says he, "attacks all the organs to which the great sympathetic is distributed, since all the symptoms of this colic can be reduced to the perversion and heightening of the sensibility and contractibility of the digestive organs. But as it is the nervous system which conveys to them sensibility and motion, it results therefrom that the excitement of the nerves is imparted to the muscular fibre: whence the convulsive action of the muscular fibres of the digestive canal—whence hardness and tension of the abdomen—

whence, those hard, uneven, movable tumors which are formed during the exacerbation of the pain. The longitudinal and transverse fibres being then unable to expel the fæces, exert those morbid and irregular contractions, of which pain is the result."

Thus we have lead which is here recognized as having the property of stupefying the tissues, paralyzing the muscular fibres, producing directly pain and convulsion in those very same fibres; an assertion which is contradicted by the observations made in lead palsy.

It is not astonishing that such explanations should be so defective and so little satisfactory, for they are intended to establish a fact which does not exist. The pain of lead colic has its seat in no part of the digestive tube; it resides in the muscular walls of the abdomen.

Astruc, who had given to lead colic the name of "rachealgia," entertained a forethought of this truth, and also Laennec and Barbier, who believed that this affection had its seat in the prolongation of the rachis; and finally, Messrs. Andral, Ranque, and Grisolle, who, bearing in mind the pain which it excites in the lumbar and thoracic regions, and in the muscles of the extremities, supposed that this pain was of a neuralgic character. But never has the correct opinion been more explicitly expressed than by Giacomini, of Padua, who expressively asserted that the pain of lead colic is not seated in the intestines, as it is commonly believed, but rather in the abdominal muscles and the diaphragm. (Treatise on Materia Medica, Art. Lead.) It is true that the above-mentioned Italian physician has been quite badly criticised by Mr. Tanquerel for his want of reverence to the opinion of the majority. "To say the least about it," says Mr. Tanquerel, "this opinion is very far from seeming true; it has but one merit in its favor, and that is originality. For, does rheumatism of the abdominal parietes impede or disturb the digestive functions? Besides, patients affected with lead colic do not refer the pain to the walls of the abdomen, but rather to other more deeply-seated parts." (Tanquerel, vol. 1, page 337.)

The muscles which form the enclosure of the abdominal parietes are, however, positively the seat of pain in patients with lead colic, and of that pain there is but one kind. Here are the proofs:

1. If palpation of the abdomen be made, in patients affected with lead colic, by resting one or two fingers perpendicularly, without bearing too much upon it, so as to press the integument and the subjacent muscle alone, a violent pain is always suddenly excited, which the patient ex-

presses by contortions and complaints, and with a countenance similar to that which is exhibited in a fit of colic, and this pain is so violent, that the patient forcibly withdraws the hand of him who presses upon the painful spot. This compression effects, in no wise, the contents of the abdominal cavity: first, because of its lightness; and secondly, because it produces the same result when the painful spot corresponds with the insertions of the muscles upon the ribs, the bony margin of the pubes and crest of the ilium, and with the fleshy masses of the "quadratus lumborum," sacro-lumbalis and dorsalis longus muscles.

The colic is always felt in those parts which are painful upon pressure, and so exactly, that there is no single case on record where the pain of lead colic has not corresponded to that region where palpation created pain.

In some cases, the abdominal parietes are so excessively painful, that they are thrown into a state of contraction, whence results the retraction of the abdomen, which was once considered the classical attribute of lead colic.

2. Motion, whether active or passive, of the muscular fibres of the painful parts, increases the pain, and the latter, in its turn, notably interferes with locomotion. Thus, patients do not breathe freely, nor can they take in long inspirations. Cough, or any effort, increases the pain in the parts already affected; and the motions which they perform while they change their position in bed, are equally productive of pain, particularly if made without precaution, or abruptly. Such patients are continually bent, and cannot straighten themselves; if the pain is situated in the recti muscles of the abdomen, they incline forward; if in the oblique muscles, sideways. In both cases, whenever any attempt is made to assume an erect position, they utter screams, indicative of the most violent pain. It is the same with these patients as with those affected with lumbago, with this difference only, that the effects show themselves in a contrary direction. The patient who suffers from pain in the sacro-lumbar and dorsalis longus muscles, throws his body backwards, and cannot bend it forward without great suffering; on the other hand, the patient who suffers from pain in the recti muscles of the abdomen, is bent forward, and is unable to straighten his body.

Much has been said about the relief which patients derive from lying upon their belly, but it has not been further remarked, that this sort of relief, which rheumatic patients obtain whenever they assume a new position, is not of long duration; for, really, patients with lead colic roll and toss themselves incessantly about, without obtaining, like the former, an easy position. In truth, Fernel quotes the case of a painter of Angiers, who could not be relieved of the pain in the abdomen, but by pressure exerted by several men, simultaneously, upon that region of the body. But this very rare case finds its counterpart in the belts with which persons of the lower class wear tightly around their loins, in order to allay the pain of lumbago. The relief thus obtained may be considered as the effect of pressure upon painful rheumatic parts, but does not disprove the existence of that species of pain which mere pressure excites.

3. Rest relieves, calms, and, by itself, may remove this pain. From the moment that it is granted that any movement of the affected muscle increases pain, it follows, as a necessary consequence, that entire absence of movement should diminish it. But this was but a mere provision, the exactness of which was to be tested by facts. Through motives which I will farther explain, I treated four patients with lead colic of moderate intensity, by mere diluents, and to prevent them from incessantly tossing and turning in bed, I used some insignificant application, to which I seemingly attached much importance, in order to engage their attention. Of the four, one continued restless, and did not obtain the least relief, after twenty-four hours; and the remaining three, who had kept much quieter, were cured. Two felt a decided improvement two hours after the application of the pretended remedy, and were well the next day. One of them was radically cured; and in another, the pain, which had completely disappeared, recurred after the lapse of five days. These patients had not taken one single particle of a cathartic substance, and one had only taken simple demulcents. We may, therefore, be led to believe that rest, which is recommended to these patients when they enter the hospital, has a large share in the success which the several methods hitherto employed for the treatment of this affection, assume, more or less equally, to themselves.

In my opinion, rest, together with the spontaneous elimination of the lead absorbed (this elimination must be actively kept up, at first), accounts for this success.

4. The abdominal pains of lead colic are sometimes accompanied, as Mr. LeBeau has first ascertained, by either hyperenia or anesthesia of the integument which covers the painful muscles. The same condition is also met with in hysterical women, whose superficial muscles are so frequently the uncontested seat of very acute pain.

5. Costiveness has not absolutely any appreciable influence over the

abdominal pains of lead colic; the patients, as will be seen farther, can always be rid of their sufferings if the costiveness persist, as in the onset of the disease.

6. If by therapeutical means, which we will mention elsewhere, the pain which pressure excites in the muscular parts be made to cease suddenly and completely, the colic will, from that instant, cease likewise. There is no more local or general pain, and if the remaining symptoms proceed from disorder in the digestive functions, pain entirely ceases and the functions are reëstablished. Finally, and in termination of my series of proofs, I must refute the assertion brought forward by Mr. Tanquerel, viz.: that rheumatism of the abdominal muscles does not, in any manner, disturb the digestive functions.

It is of very common occurrence to meet in the hospitals with women of the lower class affected with a species of rheumatism which they term colic. These subjects, almost always and at the same time, experience well marked disturbance in the digestive tube, which so distresses them that they refer their ailment to intestinal inflammation. Local treatment cures the pain, and the functional disturbance soon ceases.

Such is the series of arguments by which it can be proved that the pain of lead colic is seated in those muscles which constitute the abdominal parietes, and the following is the result of observation of the order in which the pain is distributed to the several muscles:

In Forty-three Cases—The pain occupied the whole extent of the abdomen, in 6; the whole extent of the recti muscles, in 10; the superior intersections of these muscles, in 8; the middle intersections, in 2; the inferior intersections, in 3; the right rectus muscles, in 5; its superior portion, in 1; its middle portion, in 4; the left rectus muscle, in 4; its whole extent, in 1; its superior portion, in 1; its middle portion, in 2; the oblique muscles of the abdomen on both sides, at the same time, in 6; the oblique muscles of the right side, in 2; the oblique muscles of the left side, in 2; the lumber muscles and the vertebral fossæ on a level with the loins, in 19; the anterior muscles of the thorax, in 3; the muscles of the right side of the thorax, in 2; finally, the pain occupied principally and with most force the points of attachment of muscles, in 8.

The pain, in the limbs, produced by sympathy, which existed at the same time with the abdominal pain, had been found to be throughout the inferior extremities, in 6; in the fleshy portion of both legs, in 6; in the toes, in 5; in the fleshy portion of the thighs, in 5; in the knees, in 3; in the testicles, in 3; in several joints, in 2; in the perineum, in 1.

The pain but seldom occupied one single portion of a muscle, but was most often seated in several muscles at a time, and the intensity of the disease was always proportionate to the extent of the affected muscular parts.

In order to resume, then, the pain was seated in the recti muscles in 36 cases—in the lumbar muscles and the inferior portion of the vertebral fossæ in 19 cases—and in the oblique muscles, as it does, probably for the same reason, in muscular rheumatism. In fine, the muscles of the right side of the abdomen were equally as often attacked as those of the left: this fact offers a wide difference from what occurs in hysteria, in which the pain affects in preference the left side of the body.

Why are the abdominal muscles the exclusive seat of pain in lead colic? Is it because that metal acts upon them by virtue of an electric action, as it does upon the extensors of the inferior extremities?—or is this pain also developed under the influence of a special excitement of the spinal prolongation, as Astruc, Laennec, and Barbier, supposed? This, however, is not peculiar to lead colic alone: there is a disease, hysteria, in which pain is the main symptom, and it is well ascertained that this pain, which has always been considered as seated either in the nervous filaments or the splanchnic organs, is exclusively seated in the muscular tissue; and this very fact has led me to seek for the seat of pain in lead colic, which I previously supposed to be the same as in hysteria. Nor am I loath to believe that the so constant pain experienced, in icterus, on a level with the pyloric region of the stomach, and which has so long been adduced as a proof of duodenitis in that affection, and which is so easily and invariably relieved with leeches or scarified cups—nor am I loath to believe, I say, that this pain is seated in the superior portion of one of the recti muscles of the abdomen. I have twice been enabled to ascertain the exactness of my supposition in the two only cases of icterus where I sought for the seat of pain

Does there exist a law under the power of which this myosalgia is produced? It may, perchance, result from lesions in the viscera contained in cavities enclosed by the affected muscles; but, still, it is a well established fact that, whatever be its origin, it may, at a later period, exist independently of any lesion of the corresponding deep-seated viscera; and that, by its duration, it has earned for itself its right of existence.

In attempting to show that the pain in lead colic has its main seat in the abdominal muscles, I am far from contending, as Astruc did, that the digestive tube was in its normal state, in this affection. In truth, anatomical researches have not, as yet, discovered the existence of any constant lesion; and pressure exerted upon the abdomen, in those regions where muscular tenderness is not felt, reveals no pain in the deeper-seated organs. However, I admit the existence of some disturbance in the alimentary canal, but I deem it of little consequence; and I maintain, that in cases of simple lead colic, not produced by the direct ingestion of lead, the lesion of the "prima viæ" is but secondary, and always dependent upon other lesions, to which special therapeutical means should be applied. Whence it results, that we must not seek in a lesion of the great sympathetic nerves the cause of all the accidents produced by lead, and that we are naturally led to refer those symptoms as corresponding with some disturbance of the spinal prolongation.

Since, then, the pain of lead colic is seated in the muscles, and since it is so exclusively to be found there, that out of forty-three patients, two alone did not experience muscular tenderness upon pressure being exerted, can we not avail ourselves of this so constant fact, in its application to the diagnosis of lead colic? I think so. There are cases in which all the symptoms of lead colic are found, and still we cannot refer these to the use of preparations of lead; there are others, in which we are able to ascertain a previous exposure to these preparations, but in which a moderate phlegmasia exists, provoking diarrhœa, and rendering the diagnosis difficult; others, in fine, exhibit but doubtful signs, which puzzle the physician. In all cases, however, if a violent pain follows pressure upon the abdominal muscles, we may be certain that, if the pain be not rheumatic, or hysteric (and this is easily ascertained), it is assuredly produced by lead poisoning. From what I have read of the colic of Madrid, and of warm climates, I am almost sure that the pain, in those cases, is also seated in the abdominal muscles; and this would be an additional reason why these affections should be allied with one another, at least with regard to their seat of pain—a circumstance which, as will be seen, is not of little weight in their treatment.

Here ends the first part of my work, the purely pathological part; it now remains to treat of the second, or therapeutical part.

(*To be continued.*)

EDITORIAL AND MISCELLANEOUS.

A NEW YEAR'S GREETING.

With swiftened wings old Time is taking us along through this busy life, and while the echo of one New Year's greeting is yet dying in the distance, we are called on to renew our offer of "the compliments of the season." To one and all, our friends and patrons, we now send forth our warmest greeting, wishing them all the fullest enjoyment of this short life-span, as well as careers of honor and usefulness.

New Year's Day! What strange emotions must crowd upon every bosom possessed of a conscience, when these simple little words remind us that the day is really at hand. We look back on the old year, now gone from us forever, with regret or with pride, according to the extent of the neglect or the improvement of the time which has been lent us. It is a glorious privilege, that of contemplating within the same hour the actualities of the past and the probabilities, or even possibilities, of the boundless future; yet no privilege is more abused by man. Man is truly the creature of to-morrow, nor does he even build himself property on this frail foundation. To-morrow is a quicksand, yet it will sustain an edifice if the structure be of ample area and the braces strong. This area is man's reason, these braces are the experience we gather from the shadowed past. For often does man cramp his own reason by studying to forget the most valuable, because sad, lessons of the past; and as often do we find him throwing himself into the sea of life, clinging only to Hope. From the realities, from the palpable relations between cause and effect, which we gather from the past, do we gather the best material from which to work out the probabilities of the future: these constitute the strong warp of the fabric we would weave, without which our labor is but little worth.

But the reader will say we wander from our path in moralizing. True to some extent only, for we *can* moralize in medicine; and what happier theme than

MEDICINE IN NEW ORLEANS; WHAT IT HAS BEEN, WHAT IT IS, AND WHAT IT WILL BE!

What has Medicine been in New Orleans prior to the winter of 1856-7? Surely not what it ought to have been. What is Medicine now in New

Orleans? A great, a glorious improvement on what it has been, yet not all that it should, that it *must* be. What will medicine be in New Orleans? All that its warmest friends can desire. New Orleans will be the "Medical Metropolis" of the South and West, if not of our whole country.

When we speak of "Medicine," we, of course, mean medicine in both an educational and practical point of view, for where the science and art is taught it must surely be best understood, more especially when their exists honorable rivalry in the business of teaching. Of course the liberal mind that is not engaged in the business of teaching will not charge us with arrogating to ourself, as teacher, any superiority, but understand us as holding the opinion only, that to live in the atmosphere where medicine is zealously cultivated for the purpose of teaching, will arouse every mind possessed of natural activity to efforts to keep up with the foremost in the race. To teach medicine, or anything else, successfully, the teacher must be the most zealous student, and the cultivation of the subject taught is the inevitable consequence.

Often have we said in these pages, that New Orleans possesses advantages for the student of medicine not equalled in this country. This condition of things has existed ever since medicine was first taught here—now some twenty-five years—yet, New Orleans has been comparatively an obscure point of resort for students of medicine. With all her treasure she never attracted more than 225 students—for many years, far less—until the winter of 1856-7. She had laborers, but she had no rival laborers, and the vast population of our own great State, of Texas, Arkansas, Mississippi, Alabama, Tennessee, Georgia and Florida, doled out to her annually a miserable pittance of their patronage, while they poured their treasures into the laps of the far north. We do not stop here to discuss the why and wherefore of this strange condition of things, but, passing over what may be not inaptly termed the dark age of medicine in our city, introduce the reader to a cursory history of the first two years of the new era.

In the early spring of 1856, the spirit of honorable rivalry in the matter of teaching medicine broke out in our city. For several years, there had been heard within the bowels of the professional sphere a low and ominous rumbling, not unlike the indications of the approaching earthquake; occasionally even a little sulphurous odor might have reached the attentive olfactory; but our people, being unaccustomed to such warnings, did not appreciate them: some thought the mountain *might* be in labor, but the wise ones knew it was only a "*ridiculus mus.*" At last, however, the eruption came; the earth opened on the corner of Common and Villeré streets, immediately opposite the Charity Hospital gate; but instead of molten lava, pouring out for purposes of destruction only, there was observed a systematic arrangement of bricks, stone, mortar, timber, etc., etc., etc., until at the end of a few months there stood on the once barren corner, in all its fair proportions, a building with a marble tablet over its door, bearing the humble yet significant inscription: *School of Medicine.* The conception had been clear; the gestation of normal duration, and the child was well develoved and cried lustily at birth. Of course all the old women in the neighborhood flocked to see it, and many and various were the opinions expressed. Some few thought it was sturdy enough, and would grow up; others thought it had "come before its time, and would die within the first nine days;" others, again, that it might live to creep, but could never stand alone; one thought that its head was too small for its body; another, that it was bow-legged; and another said it reminded her of a "gourd vine" she had read of some sixty years ago, in an old book of her father's. In fact, some thought it had better be strangled at once, and not let to go limping through the world; but others cried, that there was no use in strangling, as the young men of the profession were up to the "ready method" of Marshall Hall, and would surely resuscitate it. In fact, as is usual, the birth of this baby created a great sensation; its parents thinking it "the finest baby in the world," and other parents thinking it not equal to their own; some even throwing out dark hints about the owl's opinion of her young.

But badinage aside, a new era had now dawned in the history of Medicine in New Orleans, and from this moment she was to walk forward and take her place amongst the foremost. The richest and most extensive field in our country had hitherto been touched by but a few laborers, and its most precious treasures were yet deeply hidden. The projectors of a new school could see nothing but good to result from the effort; they felt that they had a right to be ambitious; they saw that the field was rich and more than extensive, and they felt that they had the great democratic right to "squat" and cultivate it; they saw room for honorable competition, and they felt that they had the industry to compete; they never had the unholy desire to pull down their competitors and build of their spoils, but they felt, and still feel, the just ambition to excel. On this platform was the New Orleans School of Medicine built.

Now, let us look at *Medicine as it is in New Orleans.*

In the winter of 1856-57, the aggregate of medical students in New Orleans rose from 225 to over 300; the New Orleans School of Medicine attracting 76, and her competitor having more than ever before. In the winter of 1857-58, the total number of students in the city reached 400 (or thereabouts), of which the School of Medicine attracted 126, her competitor having more than ever before. Now, in the winter of 1858-59, the total number of students (in spite of the late prevalence of yellow fever) is about 450; the New School attracting over 140, and her competitor still improving. In other words, instead of an aggregate for the three years of 675 students, as would (reasoning from the past 22 or 23 years' experience) have been the case without the stimulus of a new school, we have about 1150, or an increase of 475; and saying that these gentlemen average the reasonable expenditure of $500 each, we have the neat little sum of $237,500 thrown into the laps of our people, instead of being carried off from home. With these figures, or even two-thirds of them before him, can any man longer doubt that the new School has done good? Yet the croaker may say—"but all this would have occurred even if you had not started a new school." Well, a negative is rather hard to prove, but we would say in reply, that twenty odd years of medical teaching had failed to do much more than the half of it; the results we have cited followed immediately on the establishment of a new school, and our pupils tell us that they came to us.

All that has occurred we predicted, until now the New School of this city has become a fixed fact, and her career is onward. Grow she must, for there is room to grow, and the soil in which she is planted will be thoroughly tilled.

Need we ask what Medicine will be in New Orleans? The inquirer has only to look at all that has been accomplished within the past three years, then think of the unlimited and unrivalled advantages we possess, and he is loudly answered, that New Orleans will ere long rival Philadelphia. It is impossible that the profession of the South, will continue to ignore the treasures of home. New Orleans is better able this day to accommodate one thousand students of Medicine than Philadelphia ever was. Nor is this said in other than the best spirit: we do not desire to depreciate our neighbor, but she cannot complain of our making a fair comparison.

In three short years, then, competition has doubled the number of students in our city. Shall we be considered wild if we say that in five years more we shall be lecturing here to an aggregate of seven or eight hundred? There is no reason why it should be otherwise. Let there be no opposition amongst teachers, but *vive la rivalité honorable!* There is something noble in legitimate strife for excellence, while opposition is only degrading and injurious. The field is ample, the soil rich, and the laborers zealous; let our strife be for excellence only, and the future is brilliant indeed.

SAVE ME FROM MY FRIENDS!--In the No. of the Boston Medical and Surgical Journal, for November 25th, is to be found a communication from "P." in relation to some remarks made by him on the subject of Tilden & Co.'s "Sugar Coated improved Compound Cathartic Pills—*without Calomel,*" at a meeting of the Boston Society for Medical Observation. "P." says he administered these *sugar coated* pills to a lady, that they were "efficient," as promised by Tilden & Co., but, so far as from being mild, as he had promised, the patient actually "fainted under their influence," and "the effect of the medicine resembled that of croton oil"! Of course, "P." has laid aside all of Tilden's pills.

But listen to what the Boston Journal says in a sort of postcript to P.'s communication!—

"[We lately directed two of Tilden & Co.'s so-called "improved" compound cathartic pills to be taken by a patient, and we are sorry we did, since the result was griping of a very distressing character, and persistent diarrhœa for some time subsequently.—EDS.]"

Truly may the Boston Journal exclaim, *Experientia docet.* When we frowned on Tilden & Co., it thought us "unnecessarily harsh"; now it gives its own experience against these manufacturers of worthless drugs. We congratulate our neighbor on having its eyes opened, though Tilden & Co. are sufferers at the hands of their friends. Pity the Boston editors had not taken the sugar coated morsels themselves, instead of administering them to a lady patient. We really think they must have some prickings of conscience for having caused such suffering "unnecessarily." They could have ordered the officinal compound cathartic pill, and have had it sugar coated too, with the same amount of labor, and then they would have known what they were doing. We certainly think it "unnecessarily harsh" treatment of a lady patient, to prescribe a pill without knowing its contents.

THE HEALTH OF NEW YORK CITY AND ITS MEDICAL MEN.—We perceive from the New York Journals that there has been a large meeting of the profession of the city, and that they have memorialized the Mayor and Council on the subject of the health of the place. It seems to have been proposed to appoint a medical head of health matters, and then to enter on some general system of reformation. Well, we hope some good may come of the effort, but we very much doubt it. There is not a merchant, butcher, baker, tailor or broker in any City Council in the Union who does not think he knows more about sanitary measures than all the doctors put together; and what is of more importance still, all these gentlemen earnestly believe that legislation for their individual benefit is of paramount importance. Talk of sanitary laws, when here is Mr. Butcher's friend waiting at the door for a contract to supply the Work House or Prison with meat! Or talk of the health of the *people*, when here is Mr. Broker's friend waiting at the door for a contract to sell any surplus real estate the city may have on hand! The idea is too preposterous to occupy a quarter inch space in the brain of an Alderman.

Ah! politics and medicine are radical incompatibles. Happy indeed will be the day when politics and individual favoritism shall give way in the management of cities and public institutions for the sick; when Councilmen will begin to value life more than pecuniary contracts or individual aggrandizement, and when the guardians of the poor sick in public institutions will prefer men solely on the ground of their superior ability to do the greatest amount of good. Shall we not have to wait until that time when the lion and lamb will lie down together? To us the prospect seems dreary indeed. The lion is not more terrible to the lamb than politics to medicine. Again, we say, we wish our New York friends God speed in their self-sacrificing undertaking. Perhaps, if their efforts are crowned with even partial success, the spirit of reform may at last reach this benighted region; and not only will the general sanitary condition of our city be improved, but our hospitals, and other institutions where physicians are required, will be made the nurseries of useful and ornamental members of society.

THE DISCOVERY OF ETHERIZATION.—The American Medical Gazette for December publishes the names of more than one hundred physicians of Boston, praying Congress not to heed the pretensions of the impostor, W. T. G. Morton, who claims to have discovered Etherization.

On Medical Colleges.—*An Introductory Lecture to the Course of* 1858-59, *in the Medical Department of the University of Nashville: by J. Berrin Lindsley, Chancellor of the University.*

The season has come for acknowledging the receipt of introductories, and this happens to be the first on our table. On reading it we are forcibly struck with a peculiarity in its complexion. The author points out quite lucidly the defects in the general system of medical education adopted by our schools; condemns "the cramming, crowding, hurrying system" of lecturing; urges the importance of "practical instruction," and yet he is talking to a class of young men who are about to be educated under the very system he condemns. As we have said before in these pages, it is sheer nonsense for men to be thus blowing hot and cold with the same breath. Any Professor, in any college in the country, can preach precept; but the thing needed is *example.* To prate about reform without making the first step towards effecting that reform, is worse than not to prate at all. The author thinks "it an easy matter for the schools to make such improvements in their modes and means of teaching, as the voice of the profession may demand;" and "by concert of action among a few leading institutions, such changes can be uniformly made; and other schools, in self defense, will be compelled to adopt them." To all of which we say amen! Yet, where is the "leading institution" that takes the initiative? A new school started in this city, has clearly demonstrated that students and preceptors will aid in exertions to better the system of education; all the so called "leading institutions" acknowledge that improvement is necessary, yet not one *acts.* All prate, and prate eloquently (if prattle can be made eloquent), but here the matter ends, while hundreds of our students of medicine are stuffed on six or seven bench lectures daily, and have no real practical teaching at all. Let there be action, say we. Fear nothing, gentlemen; the student of medicine is willing to be well taught, and will patronize those who make the effort to teach him.

Dr. David Uhl, the American Medical Gazette says, is reported to have died recently at Bolivar, Venezuela, whither he had removed last year. Our readers will, no doubt, recognize the name as having been most discreditably connected with the great murder case in New York. The course of Dr. Uhl in that case should be a warning to professional men.

THE RUSSIAN GOVERNMENT AND QUACK MEDICINES.—From our European exchanges, we learn that the Russian Government has issued the most stringent orders to refuse admission at the various Custom Houses to a long list of notorious quack preparations. These are the people whom our countrymen are in the habit of stigmatizing as "barbarians." Surely this effort to crush the greatest evil of the age we live in, is no evidence of barbarism. On the contrary, it is the surest evidence of rapid advancement in civilization. Under this same Russian Government, the miserable fashionable humbug of the day, Homœopathy, has been subjected to the fairest test, and has been condemned.

LIST OF PAYMENTS TO DECEMBER 24, 1858.—Drs. O. J. Hood, vols. 4 and 5, $10; Maas (N. O.), vol. 5, $5; J. S. Lewis, vol. 5, $5; P. N. Wade, vols. 4 and 5, $10; W. H. Carey, to Dec. 1859, $5; A. N. Per kins, vol. 5, $5; J. B. P. January, to December, 1859, $5; B. B. Wilkins, vol. 4, $5; S. W. Hamilton, vols. 5 and 6, $10; J. H. Landry, vols. 5 and 6, $10; Greenleaf, vol. 5, $5; Lambert, vol. 5, $5; B. B. Wilkins, vol. 5, $5; J. B. Page, vols. 4 and 5, $10; J. M. McLendon, vol. 5, $5.

If there is a time when we have a right to expect remittances from our subscribers, surely it is this. Once again, we say, we are behind hand, and solely for the reason that our subscribers will not pay us what they justly owe. Pay up, gentlemen, not only all arrears, but for the new year in advance, and we will work for you willingly. Pay the printer, if not the editor.

BOOKS AND PAMPHLETS RECEIVED.

"THE HISTORY OF PROSTITUTION: ITS EXTENT, CAUSES, AND EFFECTS THROUGHOUT THE WORLD. Being an official report of the Board of Alms-House Governors of the City of New York. By WILLIAM W. SANGER, M. D., Resident Physician, Blackwell's Island, New York City; etc., etc., etc. New York: Harper & Brothers, 1858."

Through our enterprising friend, J. C. Morgan, bookseller, etc., in Post-office building, we have received from the publishers this most valuable acquisition to our medical literature. We are really sorry that want of space forbids our noticing the work *in extenso*, but we can assure the reader that money spent in purchasing it will be well spent. No praise of ours is necessary to elevate the author in the estimation of his professional brethren; he will be known by his works. Such an undertaking as this required great industry, unusual moral courage, and a well educated mind. All have been found in Dr. Sanger, and the profession, and the community generally, are laid under lasting obligations to him.

"AN EXAMINATION OF THE QUESTION OF ANÆSTHESIA, ARISING IN THE MEMORIAL OF CHAS. THOMAS WELLS, presented to the United States Senate. By the Hon. TRUMAN SMITH, Senator from Connecticut."

Only recently we made some cursory remarks in condemnation of the course being pursued by the impostor, Morton, of Boston, in endeavoring to establish his claim to the great discovery, the use of anæsthetics. Now we have before us a pamphlet, the object of which is to place this man, Morton, in his proper position, and to establish the claim of the lamented Dr. Horace Wells. The mass of evidence furnished here in favor of Dr. Wells is more than should be deemed necessary, and no fair mind can rise from the perusal without more than willingly giving poor Wells all that he ever claimed, or without entertaining a feeling of disgust for the impostor, Morton, which carries with it real shame that such an individual should be called a man. If this pamphlet is read by our members of Congress, they must at once award the whole credit of the discovery of anæsthesia to Dr. Horace Wells, late of Hartford, Connecticut, and they should forthwith make solid provision for the destitute family of a man who has done more for his fellow beings than any one, save Jenner, who ever lived.

As an humble member of society generally, we hardly know how to express our thanks to the Hon. Truman Smith for his able exposition of honest claims on the one hand and shameful rascality on the other. It seems to have been a labor of love with him, and he merits the thanks of all honest men.

"ON VESICO-VAGINAL FISTULA: AND ITS SUCCESSFUL TREATMENT. Illustrated by eleven cases. Read before the British Medical Association at Edinburg, July 31st, 1858. By I. BAKER BROWN, F. R. C. S., Surgeon Accoucheur, etc., etc. London: Savill & Edwards, 1858."

We are much indebted for this interesting pamphlet, dedicated to Dr. Bozeman, of Montgomery, Alabama. It forms part of the history of the cure of Vesico-Vaginal Fistula. We shall give it a safe place in our library.

"DIPTHERITIS: A CONCISE HISTORICAL AND CRITICAL ESSAY ON THE LATE EPIDEMIC PSEUDO-MEMBRANEOUS SORE THROAT OF CALIFORNIA, (1856-7); with a few remarks illustrating the diagnosis, pathology and treatment of the disease. By V. J. FOURGEAUD, M. D. Sacramento, 1858."

A highly interesting essay, and which we are sorry we cannot quote from as freely as we would wish. Dr. F. is a strong advocate of cauterization in this formidable disease.

"HINTS TO CRANIOGRAPHERS, UPON THE IMPORTANCE AND FEASIBILITY OF ESTABLISHING SOME UNIFORM SYSTEM BY WHICH THE COLLECTION AND PROMULGATION OF CRANIOLOGICAL STATISTICS AND THE EXCHANGE OF DUPLICATE CRANIA MAY BE PROMOTED. By J. AITKEN MEIGS, M. D., Professor, etc., etc. Philadelphia, 1858." From the author.

"A NEW MODE OF DRESSING FRACTURED CLAVICLE. By J. W. FREER, M. D., Prof. of Anatomy in Rush Medical College, etc."

We are under obligations to the author for the loose sheet detailing his ingenious method of managing fractured clavicle. We will try the plan if opportunity offers.

"ANNUAL ANNOUNCEMENT OF LECTURES IN THE ATLANTA MEDICAL COLLEGE, FOR THE SESSION OF 1859. WITH A CATALOGUE OF MATRICULATES IN 1858."

A WORD TO BOOK PUBLISHERS.—We are thankful for the attention shown us during the past year by publishers of Medical Books. We do not, however, consider ourselves "under obligations" to them, to any greater extent than they are to us for noticing their publications. All good books, we make it a rule to speak favorably of; all worthless ones we freely condemn. This is the course we always expect to pursue.

In future, we shall be especially obliged if publishers will send books intended for us to *any other dealer in this city than* T. L. WHITE. In our dealings with men, we prefer to keep that channel in which is to be found the necessary elements, politeness and liberality.

EXCERPTA.

Anæsthetics in Midwifery.—At a late meeting of the Norfolk (Mass.) District Medical Society, the subject of discussion being "The extent to which Anæsthetic agents should be used in Midwifery," Dr. Cotting, of Roxbury, made the following remarks:

We restrict for the present discussion the term "anæsthetic agents" to those administered by inhalation, and whose powers have been but recently discovered.

The volatile substances which have been used as anæsthetic agents within the last ten or twelve years are—*ether* (sulphuric, so called), *chloric ether* (a tincture of chloroform), *chloroform*, and *amylene.* Others have been tried, but these have obtained the greatest number of advocates.

The various objections to ether, particularly its unpleasant odor and the large quantity required, led to the trial of chloric ether. This having been shown to be only a solution of chloroform in alcohol, the adoption of pure chloroform was a natural sequence. As chloroform proved to be a dangerous and often a deadly agent, a return to chloric ether followed on the part of a few surgeons. This agent subsequently proved fatal, and is now seldom heard of. A mixture of sulphuric ether and chloroform was at one time advocated by one of the claimants to the original discovery, but was never received with much favor. Two years since, Dr. Snow, of London, who made the administration of anæsthetics a "speciality," thought he had discovered in amylene (made by distilling fusel oil with chloride of zinc) an agent which would prove less objectionable than any previously tried, and quite as efficacious. He had already published several reports in its favor, when a fatal case occurring in his own hands checked further experiments therewith. A similar fate awaited its use in France. In this country it has not been tried to any great extent. We have administered it a few times with indifferent success. Had Dr. Snow lived, he would probably have soon laid it aside, if, indeed, he had not already done so.

On the continent of Europe, chloroform is the chief volatile anæsthetic made use of; and, to this day, ether, and America, as connected with the discovery of its anæsthetic powers, are almost entirely unknown or ignored. In England, also, until quite recently, chloroform has been employed almost exclusively. But the many deaths caused by it have at last induced some of the London surgeons to administer ether; and one author —Mr. Erichsen—has, in his recent work, become its partial advocate. The continued presence in Europe of the chief opponent to the use of chloroform, may have had some influence in effecting the change alluded to, and may yet be the means of inducing continental surgeons to recognize the advantages of ether, and to acknowledge more freely the claims of our country to the greatest discovery of the age.

It has been claimed for chloroform that "it is far more portable; more manageable and powerful; more agreeable to inhale; less exciting than ether; and gives us far greater control and command over the superinduction of the anæsthetic state." "On the other hand," says Dr. Hayward, *Surgical Reports*, p. 246, "it cannot be denied that fatal effects have followed its inhalation, even when administered by the most judicious hands, that in some cases convulsions have been produced, and in others great disturbance of the brain, causing delirium. In some persons this affection of the mind has continued several weeks." And, again: "scarcely a medical journal comes to us from Europe that does not add one or more to the melancholy catalogue. It is wonderful to me that intelligent and educated men should continue to use an agent of such terrific power. That its effects are oftentimes salutary, every one will admit; but that they are not infrequently deadly, no one can deny."—Ibid., p. 260.

Not far from a hundred deaths directly due to the inhalation of chloroform have now been reported. Many others have doubtless occurred; but, as in midwifery it is difficult oftentimes to estimate the relative importance of the attending circumstances, there is some doubt whether the whole evil has been made known to us. Fatal cases have happened in the practice of the most experienced, judicious and careful practitioners, and where the result could not be chargeable to the impurity of the agent, nor to the quantity inhaled. Indeed the chief alarm arises from the fact that many of these deaths took place before any considerable quantity had been inhaled, and when the purest article had been used, and the greatest caution has been observed to secure its gradual administration. In some cases the fatal result has been almost instantaneous. There is no possibility of eliminating such cases. It may happen to one's first patient, or never through a long experience; but there are no means of previous discrimination. "Tenth or ten thousandth," the appalling risk is equally great in each untried case; and previous impunity will not insure subsequent safety.

We have said thus much because there is danger in the doctrine that the ill effects of chloroform result from the "impurity" of the article used, or the "want of due care" in administering it. This doctrine, so common abroad, has been quite recently advocated by a highly respectable practitioner in a neighboring State; and his "Paper" has been adopted by the State Society, and printed in their "Transactions." "Rashness" and "inexperience" are unseemly words from any one whose next case may, in spite of his experience and caution, cause him to implore a more charitable judgment of the cause of its fatal termination.

In Boston and vicinity, ether is the only anæsthetic the use of which is openly advocated by experienced practitioners. It has never been known to have proved fatal. The only reported case of death during its administration was shown to have arisen from other causes than its inhalation.

From these premises the conclusion is that *ether* (as in other cases) is the only anæsthetic agent which should be used to *any extent* in midwifery. The subject for discussion takes, therefore, the following form: the extent to which *ether* should be used in midwifery.

In our own individual experience in several hundred cases of normal labor, we have been led to observe—that only a very few patients were capable of taking just that amount which would deaden the acuteness of the suffering without at the same time diminishing the frequency and effectiveness of the uterine contractions—that generally, as suspension of consciousness approached, there was a marked and proportionally complete suspension of the expulsative efforts—that, with the greatest care possible under the circumstances, there was frequently more or less irritation of the air passages; often troublesome coughing; sometimes nausea and vomiting, attributable directly to the anæsthetic; also, occasionally strong tendencies to hysterical manifestations, which sometimes continued after the labor was over; with other minor inconveniences, such as unwonted impatience, jactitation, etc.; so, also, instances, not a few, of subsequent retention of urine; as well as post-partum hæmorrhage from imperfect uterine contraction, apparently due to the same agent—that, although something was apparently gained by the occasionally greater relaxation of the organs, the duration of these labors was unmistakably longer than those of a similar character in which an anæsthetic was not used; and, in general there seemed to be greater subsequent debility, and a slower getting up than was to have been expected—that we have never witnessed any undoubted evidence of subsequent *permanent* injury to the life or health of the mother or child arising from the use of ether during labor.

In abnormal cases—from a considerable experience in all the various operations from podalic version to craniotomy and other disintegration of the fœtus, both before and since the discovery of these anæsthetics—our conclusion is that while the judicious use of ether immeasurably increases the ease, certainty and effectiveness of obstetric operations, the insensibility of the patient, when desirable, and her comparative safety, are benefits to be obtained through its administration whose value is beyond all estimation.

In puerperal convulsions, whether identical with uræmia, according to the latest theory, or otherwise, anæsthetics during the paroxyms seem to be supplanting what but yesterday was considered the only orthodox practice. The convulsions seemed to be completely controlled by the use of these agents in the few cases in which we have had occasion to administer them.

Such has been our private experience. We do not know that it is at variance with that of any observant practitioner. Whatever suggestions we may have gained from the reports and practice of others, it is not improper to say that we here advance nothing practical which has not been confirmed by personal observation, the results of which alone are suited to this occasion and the object of the present discussion.

Bearing in mind, then, that the great object of our art is the diminution of human suffering; and that in the economy of nature the pains of parturition may have some ultimate beneficial purpose; and further, that as sufficient time has not yet elapsed since the discovery of the anæsthetic powers of these agents to fully disclose all the consequent effects of their administration, much must be left in each individual case to the intelligence, judgment, and tact of the medical attendant—bearing all this in mind, we conclude with the following generalizations:

I. That in ordinary cases of widwifery, while *ether* may be allowed in moderation when importunately demanded by the patient, it is quite as well in the long run, to say the least, to let normal, uncomplicated labors proceed uninterfered with.

II. That in painful, laborious, or complicated labors, and in cases of great tenderness or great rigidity of the organs, of extraordinary susceptibility to pain, and where there is great nervous irritability, or undue apprehension of danger, ether, if favorably received, should be used to the extent of overcoming the abnormal condition and suffering.

III. That in cases requiring manual or instrumental interference, ether should be used to the same extent, and upon the same general principles as in other operations involving pain and danger to the patient.

IV. That in puerperal convulsions, especially in those having the characteristics of uræmic eclampsia, ether should be given as soon as there are indications of an approaching fit, and be continued, if seemingly efficacious, until the paroxysm has subsided and quiet sleep is induced; or until other medicine, if desirable, can be swallowed—care being taken to allow a sufficiently large quantity of pure air, and not to continue the ether if coma supervene.

V. That as all the volatile anæsthetics yet tried, except ether, have been known to cause severe accidents, and even instant death, though given with the greatest care by experienced practitioners, and this, too, before any considerable quantity had been inhaled—ether only should be used as an anæsthetic in midwifery. Ether, likewise, should be administered with the greatest caution, so that the safety of the patient may not be unnecessarily put at hazard.—*Boston Med. and Surg. Jour.*

WATERY DISCHARGE FROM THE UTERUS DURING PREGNANCY.—Dr. Harvey made the following remarks on this affection at a meeting of the Cork Medical and Surgical Society, December 9, 1857: "In some books on Midwifery, watery discharge from the uterus is noticed as amongst the diseases to which pregnant women are liable. A clear, limpid, colorless fluid, oozing in quantity from a few ounces to pints daily, flows away, sometimes stopping for a short time, and recommencing; and in the majority of cases it continues nearly, or full to the time of delivery. The abdomen does not appear palpably reduced by these discharges, and a living child is commonly born at or near the full time. In the greater number of instances, also, there is evidence of the usual quantity of liquor amnii being present on the supervention of labor. Dr. Alexander's case, given in the third volume of the *Medical Commentaries*, shows this very prominently. In a case by Dr. Petel, also, in the *Gazette de Hôpitaux* of July, 1838, the liquor amnii is specially mentioned as normal in quantity.

"What is the source of this fluid, discharged as it is, to the amount of hundreds of pints in the course of a few months? The supposition of its coming from the cervical glands of the uterus, or from the vagina, both of which have been assumed as sources of it by different authorities, appeared altogether unlikely from the nature of the fluid, its quantity, and its mode of coming away in gushes of considerable quantity at a time. That it could come from the space between the decidua and chorion, or between the chorion and amnion, we have no pathological facts, so far as I am aware, to warrant our supposing such a source for the flow; whilst in the natural condition of parts such spaces do not exist; as, at a period of pregnancy before these discharges commonly show themselves (say the sixth month, or thereabouts), the cavity between the chorion and amnion has disappeared; and we know that the chorion and decidua are in contact throughout.

"Under these circumstances we seem driven to the conclusion that the amnion must be the source of this flow; that there may be occasional solution of continuity in this membrane, admitting of discharges from time to time, which either close again, or admit by the mechanical relations of the bag to the neighboring parts of the amnion, refilling to a certain extent by a fresh secretion of its particular fluid. In confirmation of this view may be mentioned cases recorded by Dr. Denman, Professor Burns, of Glasgow, and Dr. Pentland of Dublin, in which the amnion is said to have given way from fright or other sudden shock, the water being discharged without labor coming on. All these considerations tend rather to the view that the escaping fluid may be liquor amnii than to any other which has been propounded. In the case which I am going to relate the symptoms were similar to those which were present in the cases of watery discharge which I have been noticing, and in this instance, as will be seen, the flow was undoubtedly amniotic.

"Mrs. ———, mother of several children, was for more than a year, the subject of heavy sanguineous discharges, which were so little influenced by the treatment adopted that the existence of polypus was thought possible. An examination revealed considerable congestion of the os and cervix uteri, with superficial ulceration, which gave way to treatment generally and locally applied. During last summer her health was considerably improved, but occasionally menorrhagic attacks, which latterly observed more or less closely the monthly periods, showed themselves. Matters were going on thus when she suffered a considerable shock by her eldest boy meeting with a severe accident, in which his arm was fractured. On that day, for the first time (six weeks before delivery), she had a sudden gush of clear watery fluid from the vagina, and since that time to the date of these notes (5th November,) she was scarcely free from it; it would diminish or nearly stop for a few days at a time, to come on again in gushes, and in considerable quantity. The quantity escaping in one of these was seldom less, and generally more, than half a pint; and on the late occasion, when the flow was accompanied by a heavy sanguineous discharge also, she thinks the combined amount was fully a quart. It came on in the horizontal position as well as in the erect, and apparently without any cause. The size of the abdomen did not appear much affected by these at any time.

"The occurrence of the watery discharge suggesting the probability of pregnancy, notwithstanding the menstrual changes which had been going on with some regularity, and that, if pregnancy did exist, the ovum might have suffered hydatid degeneration, I proposed an examination for the purpose of ascertaining the point. I found an abdominal tumor occupying the hypogastrium to above the umbilicus, and on laying my hands over its surface, it gave a good example of the value of a diagnostic indication lately suggested by Dr. Oldham; it afforded distinct evidence of its being uterine by gradually and regularly hardening under my hand. The movements of the child were also felt, and fœtal pulsation, distinctly heard by the stethoscope, put an end to all doubts.

"I told the lady that she had passed some six or near seven months of her pregnancy without being aware of it, and that her labor would probably come on prematurely, all of which she entirely disbelieved, and I could not induce her to make the necessary preparations. Two days after, I was called to her—the first stage of labor having set in with unusual distress and irritation; the pains peculiarly sharp and unbearable; the os uteri was hard and unyielding, and the breech, presenting in the second position, was felt in close contact. I immediately put her on antimonial solution, notwithstanding which the os uteri took over three hours to relax. After a first stage of about four and a half hours, and a second of less than half an hour, a male child, of scarcely seven months' growth, was born. The presenting hip and buttock were perfectly black, evidently from the direct pressure to which thay had been subjected, in consequence of the loss of the liquor amnii. None whatever escaped with the child, and the sanguineous discharge was also unusually scanty. I do not think I ever witnessed so dry a labor.—*Dublin Quart. Journ. Med. Sci.*, and *Amer. Jour. Med. Science.*

REMARKS ON THE SOURCES AND QUALITIES OF HONEY.—By PAUL DELACY BAKER, M. D., of Eufaula, Alabama.—Messrs. Editors: In the October number of your valuable Journal I find discussed, "the question of poisonous honey;" and the importance attached to it by your interesting editorial, induces me to offer to your pages a few brief, and matter-of-fact observations in reference to the subject.

The chief object of this communication is to combat the erroneous, yet almost universal impression, that bees "*extract*" honey from flowers, and that as some blooms possess poisonous properties, the honey *extracted* from them must, of necessity, be more or less deleteriously impregnated. This impression prevails with all, I believe, who have *written* about bees and honey, and it plainly exists in the minds of the three writers, whose articles, in reference to this matter, were set forth in this journal. Now, I propose to show that honey is not the juice or nectary of flowers, and that it is never extracted or *collected from* flowers, but that like dew, it falls from the atmosphere, and is gathered by the bees and deposited in the cells of the honey-comb in precisely the same state in which it was collected, and that, therefore, in all probability, honey, "within itself," is ever a perfectly innocuous substance.

The following experience forced me unavoidably to this conclusion: About the middle of June, 1850, I was at an old hunter's house, in South-western Georgia, preparatory to starting with him on a deer drive. This man was a great lover and minute observer of nature; in his yard there was a great number of bee-hives, and he sold the honey in large quantities to the neighboring villagers. While at his house, I heard him complaining that there was a honey famine—that the hives were all ready for its reception, but that they were utterly destitute of honey, and that the bees would soon starve. I asked him, how such could be the case, when it was then the middle of June, and the country full of *flowers*, and why the bees did not collect it? To my great surprise, he replied, that bees did not get honey from flowers, but that it "fell from the clouds." I was amused at the idea, but, of course, wholly skeptical concerning it. He, nevertheless, assured me of its correctness; and to my question, why did we always find bees at work upon flowers?—he answered that, they were gathering pollen, from which they made bee bread for their young, and that they were also collecting materials for forming the honey-comb and arranging the cells—to convince me, he exhibited to me a hive, where, sure enough, existed the comb, cells, and all else, perfectly prepared, yet not a particle of new honey, and the old supply nearly exhausted. There was prevailing at the time a severe and protracted drought. Of course I had to believe what I saw, but was still an unbeliever as to the "honey falling from the clouds."

The evening of the same day we went fifteen miles into the wild woods, where our hunting party camped, far away from any dwelling. The old hunter and I slept under two beautiful young hickory trees, and at dawn the next morning he roused me up, exclaiming, with great enthusiasm, "the honey due has fallen!—get up, you unbelieving Thomas, and see for yourself." Upon rising, the first thing that attracted my attention was the buzzing of bees, and on looking up to the top of the hickory trees, I saw myriads of them working, and coming and going; the limbs of the trees grew low to the ground, and upon its being pointed out, my astonished eyes beheld, for the first time, the "honey dew," on the leaves, and occasionally actually roping down and dropping from the pendant points of the smooth leaves; I tasted it frequently, and at once recognized the peculiar flavor of the common honey. I saw, and felt, and tasted it, and my mind was convinced by these means, which God had given, to lead it to correct conclusions.

The only observable difference, between this substance found on the trees, and honey obtained from hives, was, as to their relative consistency—the latter being somewhat more inspissated; but I have no donbt that the difference is produced by time alone, and that if a portion found on the hickory leaves had been collected and preserved in a test vial, its properties would ultimately have been identical with that found in the hives.

My companion remarked that, in ten days all his hives would be ready for robbing, and that the next morning he would show me the bees working at home, and collecting honey from the leaves of the tall black gum trees that grew in his yard. He said, bees only collected it from the smooth-leaved trees of the forest. The same evening we returned to his house, and at daylight the next morning we went out into the yard, and the smooth-leaved black gums presented the same appearance as did the hickories in the woods on the previous morning. The bees filled the air, and I watched them fly from the trees straight to the hives—occasionully, those starting from the lower limbs would come to the ground before reaching the hive, and be compelled to take a second, and sometimes a third flight to gain it—some of these we caught and killed, and opened, and in every instance were their stomachs filled with honey. I tasted it, and found it identical with what I saw the morning before. Through a glass in the hive, I saw them storing the honey—they invariably *backed* into the cell and deposited their burden.

In less than two weeks I was at this man's house to see the hives robbed. They were full to overflowing, with the most beautiful and luscious honey. If I recollect correctly, there still had been no rain.

I am aware that there are flowers, such as the common honeysuckle and woodbine, that contain a sweetish juice, not visible I think, but readily detected by the sense of taste, but the flavor is not similar to that of honey. It is true, as the editor of the "Druggists' Circular" remarked, that "the saccharine matter of the nectaries of flowers is not exactly identical with the characteristic properties of honey." I would also observe, that this juice occupies a situation so far down, in the long tube of the flowers, as to be out of reach of the bee, and only attainable by the long delicate bill of the humming-bird, or the proboscis of the butterfly; and even could it be attained by the bee, it exists in such minute quantities as to preclude the possibility of a swarm's collecting from such a source, twenty or twenty-five pounds of honey in the space of two or three weeks, as was evidently done by the hunter's bees.

Such is the experience, and such the observations that have taught me, that honey is not the "extract" or product of flowers; nor is it the result of any elaborating function of the bee's stomach, but, that it falls like dew, and is collected as readymade honey, and requires no more extracting or elaborating process, than is necessary in collecting rain water in a cistern.

So much, then, as to "whether honey is, within itself, ever a poisonous substance;" but, what I have related, does not do away with the fact that persons *are* sometimes injuriously, if not poisonously, affected from or after eating honey; nor does it explain the why or wherefore of such accidents, but I think it *does* establish the fact, that such occurrences depend upon extraneous matters and accidental circumstances. If it could be ascertained, I think it would be found to be the fact, that whenever deleterious consequences resulted from eating honey, the honey-*comb* was masticated, and that no care was used in the selection of it; in this way, it would be quite possible to introduce into the stomach poisonous materials; for it is a well ascertained fact, that there *is* such a thing in the bee-hive, and among the comb, as "bee-bread;" that it is a brown, bitter substance collected from the pollen of *flowers*, as food for the young bees. This might be collected from poisonous flowers, and to it may pertain in full force your philosophic remark, that "the instinct of the bee may, in most instances, preserve him and his race from the toxic effects of the deleterious properties of flowers, and yet, what has served as his *nutriment* may be, for man, a most destructive *poison.*"

I have no certain knowledge of the source from which are collected the materials for forming the comb, nor what the nature of those materials is; but I would sooner look to them as the cause of the mischief, than to the honey—knowing as I do, the source from which *it* is gathered. Again: in considering this matter, it might be well to remember, that it is common to find in honey—in the comb—dead bees, and that to the sting of each is attached a bag of *poison*, and should these be accidentally conveyed into the stomach, *might* not the circumstance have some connection with the ill effects produced? Nearly all the cases of sickness or poisoning, I ever heard of, as occurring from the use of honey, resulted from eating wild honey obtained from an old hollow tree, and where it was consumed incautiously and in large quantities, during the *frolic* of robbing the bee tree, which is generally done *at night;* and it should not be forgotten, that the cavities, of the trees containing the honey, are the abodes of bugs and spiders of very poisonous natures, which by the fall of the tree might possibly be crushed and mingled in the honey, and in that way be conveyed into the stomach. In addition to the suppositions already set forth, I would remark that these old hollow trees are frequently covered with a vine known as the "poison vine," and would suggest the *possibility* that cut and bruised as it would necessarily be, in the fall of the tree, it *might*, in some way, be the cause of the mischief. The frequency of these cases of poisoning is by no means so great as to exclude the possibility of the agency of such circumstances in producing it.

It is, however, true that these accidental and isolated cases afford no explanation of the effect produced upon large numbers, as for example, on the Grecian army, as related by Xenophon. In reference to such examples, all I can say is, that if such an effect should be produced among soldiery at the *present day*, by the use of honey, there would exist in my mind, strong suspicion, that there was mixed with it an undue portion of "*old peach.*"

The only way in which I can imagine that honey could, in itself, possess toxic properties, is in the same manner that the dews of certain latitudes and localities are rendered poisonous. This is said to be the case; if so, the same causes and circumstances rendering the dew nocuous, might also deleteriously impregnate the *honey* dew.

VERATRUM VIRIDE.—Some reference having been made during the discussion, to the use of veratrum viride as a therapeutic agent. Dr. Thacker read to the (Cincinnati) Academy of Medicine a letter which he had recently received, on that subject, from Dr. J. J. McIlhenny, Supt. of the Lunatic Asylum near Dayton. Dr. M. says:—"I have used veratrum viride to a considerable amount in *acute mania*, particularly in that form where there appeared to be much arterial excitement. Indeed I may say I have used it in almost all forms of insanity, and been much pleased with its action.

"In five or six cases of acute mania, I kept them under its influence most of the time for two weeks, scarcely ever pushing it to the extent of vomiting. I must consider it an admirable sedative, one which possesses the power in a remarkable degree of controlling the action of the circulation, of allaying the excitability of the nervous system without leaving any of the ill effects of opium. In several of these upon whom I tried it the most effectually, I found them awakening, as it were, from an apparent long dreamy spell, half sane and half insane, several of whom improved regularly, never having another paroxysm. Others went back to their former state of mind, but much less disposed to rave and be boisterous. My favorite mode of prescribing veratrum viride is as follows:

℞—Tinc. Veratrum Viride, ʒ i.
Spts. Nitre dulc, ʒ vij. M.

"Of this I give a tea-spoonful every half hour until I find some effect, then prolong the time of its administration to suit circumstances. I give it in many cases of mania, in quantities that might astonish practitioners in general. Whether such cases are less susceptible of its action than sane persons, I am not fully apprised, but think they are."—*Cin. Lan. and Obs.*

MOTT ON THE STRUCTURE AND FUNCTIONS OF THE PROSTATE GLAND. —Reported by Dr. GEORGE F. SHRADY.—The discussion on the structure and functions of the prostate gland commenced at an extra session of the Academy, in June, was renewed at the regular meeting in August. Dr. Valentine Mott, who was present was called upon to open the discussion. Although he was not aware at what point the subject had been left, he would at that time make a few remarks upon the relative anatomy of the prostate, reserving the discussion of its diseases until another session.

The prostate gland, said Dr. Mott, was situated in the pelvis, equi-distant from and on a line with the rami of the ischia, above the membranous portion of the urethra, and upon the rectum, where there is no interposition of peritoneum by the lower part of the bladder, called the bas-fond. It is ordinarily supplied with a fair amount of vessels, and in a surgical point of view, it is important to remember that the little vessels which supply it in its normal state are unimportant; but if we regard them closely in disease we will find they assume a great importance. Let us examine these vessels; let us take up the veins. They are very remarkable—remarkable in their number—remarkable in their size, and are principally interesting in connection with the troubles which arise in this organ. When these veins are cut, even in the middle period of life, a great flow of blood will follow; so great that after the lateral operation for stone, I myself have been very much alarmed at its quantity, and have feared the consequences of it. During the whole course of my surgical experience, now extending over more than half a century, I have rarely been surprised at any great loss of blood, except from these parts. It seems to be unimportant until about a week after the operation. At that time, I have sometimes been informed of the sinking condition of my patients, and have found them almost pulseless from the loss of an enormous quantity of venous blood. Generally, at the commencement of the operation there is some arterial hæmorrhage, which is trifling in comparison with the subsequent venous hæmorrhage, which, in some instances, has compelled me to plug up the opening with a sponge, taking the chance of the blood accumulating in the bladder.

The veins that are about the neck of the bladder run in such a way that they are always necessarily cut in the operation I shall presently decribe. Under certain circumstances, these veins are in a varicose condition; nor is this condition confined to the later periods of life, for we meet it frequently in early life. The collection of veins in this region is so large, and they are so numerous, that they go by the name of venæ pampiniformæ. They run immediately over the proper capsule of the prostate and open upon the free surface of the bladder, and are undoubtedly the source of the very serious hæmorrhage that sometimes takes place in life, when there is no disease of the prostate, no tumor of the bladder or other cause to account for it.

These veins are ordinarily the only vessels of any considerable size involved in operations around these parts, but occasionally arteries are met with running between the proper capsule of the gland and the ilio-vesical reflection. These arteries are of considerable size, coming, as they sometimes do, from the internal iliac instead of the internal pudic, and distributed to the bulb, crus and dorsum of the penis of that side. These vessels have been described by Haller, Burns, Barclay, and also by one of the Munroes. Whenever a profuse hæmorrhage occurs from an operation about the prostate gland, in which the gland is cut, if the internal pudic artery is not wounded, then it is gratifying to know that an artery of the description referred to runs in that direction, the hæmorrhage from which, when cut, can be arrested only by the sponge.

The relative surgical anatomy of this gland is exceedingly interesting. Though for many years not well understood, it is, perhaps, now fully known and developed.

The prostate is described as consisting of two lobes, and perhaps we are not all prepared to know the fact that these lobes are very ill defined after the adult period; but we know the fact, that before the fourth or fifth month, it consists of two distinct lobes, separated by an isthmus which becomes in adult life the third lobe.

This gland has its proper capsule, and is a secretory gland; but whether conglomerate or follicular, is yet a subject of discussion.

The observations of Amusat have fully settled the question that the prostate gland is entire above the urethra—that it runs over the anterior part, and therefore surrounds it.

Besides this capsule there is another—the fascia iliaca.

This fascia covers the iliac muscles, goes over the pelvis under the great vessels, then down the side of the pelvis, and from thence to the bladder, being previously attached to the side of the bladder, where it forms a perfect septum between the perineum and cavity of the pelvis. This is a most important fact to be remembered in its relation to the operation for lithotomy.

The ilio-vesical fascia passes from the pelvis to the side of the bladder at an angle which, when we are erect, is some 10° or 15° more above the plane of the pelvis than when we lie down. From the side of the pelvis to the bladder it is covered by the levator ani muscle. When we get down to the side of the bladder, just above its neck, there is a close layer of this fascia, which goes over the prostate, enveloping it next to its proper capsule. So far its anatomy is well understood.

Every sub pubic operation, with the exception of the recto-vesical, involves the direction of this fascia, and upon maintaining the integrity of this septum—thus guarding against urinary infiltration—the success of these operations, ceteris paribus, depends.

I maintain that operations for lithotomy ought always to be performed with reference to this septum, otherwise the patient's life is greatly in danger.

Nothing has glorified modern surgery more than the study of relative anatomy. It has dignified operative surgery as a science, and he who does not cultivate this study operates in the dark. This knowledge is beautifully illustrated in the anatomy of the groin, in the important relations of the epigastric artery to the internal and external abdominal rings. It is not every one who teaches who points out the relations of these parts. To operate successfully, the surgeon must first familiarize himself with the parts to be operated upon.

The common operation for lithotomy most in fashion is the lateral, known from the time of old Friar Jacques. It divides one side of the prostate only, and goes parallel with the iliac fascia, and not above it, and does not open into the cavity of the pelvis. He who performs the operation with reference to these points will be most successful.

I say as I have said for many years, that all these operations are better done with some sort of a knife than with any other instrument whatever.

Without arrogance, I may say that I have performed more operations for stone than usually falls to the experience of surgeons. The number amounts to 164, and I have never used in the operation anything but a knife or bistoury. Upon the whole I prefer a straight bistoury. I was occasionally in the habit of using Mr. Thomas Blizard's knife. Of the gorgets introduced by Cline and Physic, I consider that of the latter the best. Cline's is a dangerous instrument. I have seen him use it. Physic's is a dangerous instrument also, unless one well understands the relative anatomy of the parts. This knowledge was not in the fashion of his day.

The knife goes through one side of the prostate and below the loose layer of the fascia iliaca. Contrary to the opinion and method of many, I cut through the whole prostate. I never wish to tear it. If you are below this fascia you may cut to the ramus of the pupis without danger. Those not familiar with the use of the gorget, by depressing the hands elevate the blade, and consequently go too high up and incur the danger alluded to.

I never cut the internal pudic artery with the knife, because I cut downwards and outwards in such a way as not to encounter it. I look upon it that the operation can be more easily performed in this manner.

Some cut through eleven-twelfths of the gland and tear the rest. I know enough about wounds to say that lacerated ones are hard ones to heal any how, and you had better make a clean cut. The idea has been entertained that by cutting entirely through you cut this fascia, but having an eye to the course of it there is no danger.

The bilateral operation is entirely free from wounding the int. pudic artery; but I must say that I never saw the elder Cline, the preceptor of Sir A. Cooper, operate for stone, that he did not cut that artery. I have seen the hæmorrhage so profuse that it would run off the table in a stream, and saturate the bed, and even run through it; I choose to avoid it.

In regard to opening the bladder different ways, for the purpose of evacuating urine, there is no danger, except in the perineal cut.

The operation per rectum is unattended with danger, not involving the fascia, and is therefore resorted to when the bladder is to be emptied. One thing I will say, that during the many years I have practised surgery, it is gratifying to me to look at this fact in connection with my life, *that I never punctured a human bladder.* I am always horrified when I hear a man has punctured the bladder. I have never seen a case of stricture that could not be relieved by the judicious application of instruments.

In that disease of the prostate confined to old people, I have never failed to introduce the long catheter, even though others had repeatedly failed in the attempt.

At a special meeting of the Academy on September 9th, Dr. Mott continued his remarks as follows: Besides the covering mentioned at the last session, at which the prostate gland was discussed, this gland has a closer covering or capsule, which is the ilio-vesical reflection of the fascia iliaca as it comes from the side of the pelvis and goes to the side of the neck of the bladder. We have here also the levator ani muscle covering both portions of this fascia iliaca. The prostatic portion of the urethra, must be cut in the lateral operation for stone in the bladder.

The *sinus pocularis* or *utriculus prostaticus,* situated a little in advance of the veru montanum, has been considered by Weber as a rudimentary uterus: a homologue of that organ, probably because Morgagni found in two instances that the vesiculæ seminales terminated in it. In a surgical point of view this little cup should be remembered, as a catheter or bougie will be easily caught in it. This can always be avoided by an experienced surgeon, by withdrawing the instrument a little and tilting it up before passing it into the bladder.

The veins about the neck of the bladder in a person advanced in life will have in them what pathologists denominate *phlebolites.* It is from these veins that proceed the alarming hæmorrhage of which I have already spoken. It seems to be confined to operations that are performed on persons over fifty years old. For myself, I have operated on individuals over seventy, and have not found these veins so troublesome in advanced age as in middle life. Neither the unilateral or bilateral operation avoids these veins; the only way is to operate above the pubis, which is seldom done in this country.

Dr. Mott then proceeded to speak briefly of the *Diseases of the Prostate Gland.*

The morbid anatomy of the prostate is exceedingly interesting, involving as it does so many terrible and afflicting affections. It is a very singular fact, although we are well acquainted with it, that this body will frequently enlarge when other parts of the body are diminishing and shrivelling in the decline of life. But this is not the only disease of this organ. There are affections of it that are very formidable besides the mere hypertrophy.

And first of *Atrophy.* This gland has been found diminished in its natural size; its proper capsule has been seen filled with an aqueous fluid. The atrophied condition of the gland has been described by many pathologists, and particularly by Sir Benj. Brodie and Mr. Cooper. It is not necessarily confined to youth or old age; it follows no stated law, but arises at any time.

Inflammation of this gland occurs more frequently than we are aware of; whether it be of the secretory portion, or of the filamentous or areolar tissue of the gland I shall not stop to inquire; my impression is that it is an inflammation of the entire tissue of the gland. The symptoms are, remarkable tenderness on introducing the finger into the rectum. When this extreme sensitiveness exists with difficulty of passing water, it is evidence to my mind that the gland is very much inflammed. Suppuration may occur in this gland, as we know from cases recorded by such observers as Lallemand, Sir Benj. Brodie and others.

Inflammation of the prostate may be occasioned by the violent use of instruments, sexual indulgence, onanism, urethritis, etc.

Suppuration of this gland generally takes the route into the urethra, so that Lallemand states that he has seen the openings from the prostate large enough to admit the end of a catheter, from which openings pus escaped into the urethra. It will sometimes make its way through the rectum, sometimes into the perineum, both of which I have seen.

Inflammation of this gland may be followed by the deposit of a large quantity of pus. Brodie says as much as a pint was discharged at several different times from a case he saw.

Ulceration is a process not common in the prostate. It has occurred from the introduction of instruments into the urethra, which induced inflammation, accumulation of pus, pressure on the gland, and ulceration.

Tubercles are occasionally met with in the prostate, and from softening progress to ulceration. Lallemand records an instance in which thirty abscesses and as many tubercles were found in this gland. The prostate is therefore liable to the same condition of things we find in the testes, mesenteric glands, vesiculæ seminales, and the lungs. Dr. Gross relates a case of tubercles in this gland, and states that they follow the law of tubercles in other parts of the body.

Again the prostate is said by some to be liable to *cancer.* If we adopt the language of Mr. Walshe, that all forms of disease that are heterologous, are cancerous, we consequently may have it here. I prefer, however, the old arrangement of cancer and malignant disease, or what does very well, hard and soft cancer. I am well aware that they both have the microscopic characteristics of cancer. These malignant forms of disease occur within and about the prostate. The body of the prostate is not so apt to be involved as in other forms of hypertrophy.

There are three instances on record—one by Sir A. Cooper, one by Mr. Stanley, of London, and another by Mr. Langstaff—of malignant disease in this gland in children, showing that it is not necessarily confined to old people.

The great Dr. Fothergill, of London, died of malignant disease of the prostate. Some few years ago I had a case near this city, which was evidently a fungous growth about the prostate, and resembled that of Dr Fothergill. I have seen another instance where the tumor was not of the prostate, but a polypous growth, which acted as a valve, and gave rise to the strongest symptoms of stone in the bladder. This should teach us that nothing but a sound grating against another hard substance should justify the operation for stone in the bladder.

The last affection of this gland I shall name is *senile hypertrophy.* It is a curious fact that it should occur in advanced life, and be confined to the gland itself. A dissolute life is not necessary to produce it. The most tormenting part of this enlargement is the third lobe of Sir Edward Home —the pathological lobe of Velpeau. It is of a remarkably hard texture. Brodie speaks of it as having a stony hardness. Rokitansky calls it a fibrous enlargement, others denominate it cystic hypertrophy on account of the cyst found in it.

This enlargement comes on very gradually, and is not at first accompanied with much pain. The first symptom is difficulty in evacuating the last remnant of water that is in the bas-fond of the bladder. Change of position has to be frequently resorted to, and persons with this difficulty make water better standing up. Some are obliged to get down on their elbows, so as to give the bladder an opportunity to force the urine over the third lobe.

With all our resources of treatment we can make very little impression upon the prostate affected by senile hypertophy. In a difficulty of this kind it is of great importance that a person should not have his bladder over distended. Everything that removes irritation is of the greatest importance, and therefore it is often requisite to relieve the bladder by means of the catheter. I know of no other treatment than palliative; I have seen more good derived from a seton in perineo, at the same time keeping the bladder empty and the bowels soluble, than from anything else.

The question of the removal of this third lobe has been raised, and several instruments devised for that purpose are here submitted for the examination of the Academy.

Finally, gentlemen, let me call your attention to one other affection analogous to that called by old father Pelletan "engorgement chronique de la membrane du larynx;" I mean the *uvula visicæ* of some—the *luette vesicale* of others. Dr. Baillie first described it as a membranous fold, in the form of a bar across the vesical orifice of the urethra, and has given a plate of it in his Morbid Anatomy.

Immediately at the neck of the bladder there will be an enlargement of the mucous membrane, sometimes from above downwards, but more generally laterally. This gives rise to much difficulty in passing the urine. The catheter passes readily. Sometimes it is very difficult to form a diagnosis between it and enlarged prostate. In the uvula vesicæ the catheter passes very readily. In prostatic enlargement there is frequently great difficulty.

Any instrument passed into the bladder will generally be followed by a small discharge of blood, which alarms the patient, who thinks it arises from want of skill on the part of the operator, whereas it is desirable and beneficial. In the opinion of Guthrie this is a curable affection. Le Roy d'Etiolles has great confidence in scarifications. These, with the application of caustic, constitute the principal means of treatment.

The same principle of treatment has been applied to a similar affection of the lips of the glottis by two distinguished fellows of the Academy, Dr. Gurdon Buck and Dr. Horace Green.—*Am. Med. Monthly.*

FACIAL NEURALGIA—*Cauterization of the Helix.*—Our readers will perhaps remember the singular treatment proposed in Sciatique which consisted in cauterizing the helix with iron at a white heat. However strange this process may appear there can be offered in its favor a certain number of well established cures, and it is by no means abandoned by practitioners, although in truth the failures have been thus far quite as numerous as the successes. The same means have just been employed in Facial Neuralgia, by Dr. Texier, who has published in the *Moniteur des Hopitaux*, a communication on the subject. Dr. T. had seen the process applied in this affection by Professor Jobert de Lamballe, and he resolved to repeat it on the first occasion. A young women had suffered for six weeks with intolerable pains on the right side of the head and in the lower jaw: she had no rest, and at times experienced inordinately painful exacerbations. M. Texier effected with the instrument at a white heat, the cauterization of the helix at the moment when the pain was at its maximum. The patient uttered a cry and began to weep, then shaking her head as if to see whether she had been dreaming, she declared herself entirely free from pain. The cure was permanent.

Another young woman was for two months troubled with a trifacial neuralgia on the left side, for the cure of which various expedients had been tried in vain. The pain in the jaw was so severe as to render mastication impossible, and she lived only upon soups and milk. The cauterization of the helix was quite as successful in this case as in the preceding.

A third observation gave precisely the same result. But M. Texier informs us that this process does not always succeed; very far from it, since with eight patients he effected only one cure. It seems that to apply the method with any confidence of success the neuralgia should be fixed and without complications. Cauterization, reserved for cases in which the affection is, so to speak, idiopathic, without any particular character but a violent, steady pain in the course of the trifacial, may become a valuable resource, and we ought to be the better satisfied thereat, because in general such cases are the most difficult to manage, as science possesses little more than uncertain and too often inefficacious means of controlling them. *Jour. de Méd. et de Chirurgie*, and *Ib.*

TUBING THE LARYNX IN CROUP.—A new method of treating croup, when there is danger of asphyxia, has been proposed by M. Bouchut at a meeting of the French Academy of Medicine. The introduction of a tube through the glottis is recommended, instead of tracheotomy. The tube is from six to fifteen millimetres in diameter, and from eighteen to twenty-four in length. In order to fix it securely in the larynx, the vocal cords are placed between two projections, which prevent either the ascent or descent of the tube, which is further secured by silk cords to a collar about the neck. The tube is allowed to remain until the symptoms of asphyxia have disappeared.

It may excite astonishment that a part so sensitive as the larynx under ordinary circumstances, should tolerate the presence of such a body. Nevertheless, the tube is perfectly well borne, and interferes in no respect with the functions of the epiglottis. It is sufficiently short to disappear entirely in the larynx, its upper surface being on a level with the ventricles.

Bouchut has twice applied the tube at the Hospital St. Eugenie; once to a girl affected with diphtheritis of the ear and larynx, at a time when there was cyanosis and complete anæsthesia. It remained thirty-six hours in the larynx, which was freed from the false membrane. The patient, however, died of pneumonia, and the effect of the diphtheritic poison, but she was saved from asphyxia and tracheotomy.

The second case was that of a boy, three years old, affected with croup. At a time when asphyxia seemed imminent, the tube was introduced without difficulty, and remained forty-two hours, without interfering with the function of the epiglottis, or producing paroxysms of suffocation. He twice expectorated large tubular false membranes, which had formed in the bronchi. Gradually, however, the difficulty of breathing increased, and tracheotomy was performed.

From these two facts we may infer—

1st. That a metallic tube can be introduced and allowed to remain in the glottis.

2d. That by this simple and safe method we can introduce air as well as by tracheotomy, when there is danger of asphyxia in croup, or any other disease of the larynx.

3d. That after the prevention of the asphyxia, we can still treat the croup either locally or constitutionally.—*Boston Journal.*

MENSTRUATION IN AUSTRIA.—By DR. FRED. SZUKITS. Of 665 women born in Vienna, and menstruating there for the first time, the mean age for its first appearance was fifteen years and eight and a half months; in 210 cases, in the beginning of the fifteenth year; in 87, in the eighteenth year: in 84, in the sixteenth; 6 women menstruated first at the age of eleven years, 3 at twenty-two. The period during which menstruation may primarily occur in Vienna comprises twelve years. In Paris, in the same latitude, it occurs one year earlier.

Of 1610 women from the country, the age of first menstruation averaged about sixteen years and two and a half months. 418 menstruated during the fifteenth year, 251 in the sixteenth; then followed in order the seventeenth, eighteenth, and nineteenth. More than half, 888, menstruated after the fifteenth year; less than one-fifth—304 before it. Two menstruated at ten, and the oldest—two likewise—first menstruated at twenty-five. The period during which menstruation may primarily occur, comprises therefore sixteen years in the country—four more than in the town of Vienna. Of these 1610 women, *a.* 603 were from Upper and Lower Austria Proper; of them, the mean age of first menstruation was sixteen years and three months; here also the greatest number, 142, menstruated in the fifteenth year, 88 in the seventeenth, 81 in the sixteenth, and 72 in the eighteenth year; the youngest, 5, were eleven, and the oldest, 2, twenty-five years old; the period of primary menstruation comprising thus fifteen years, or three more than in Vienna. *b.* 430 were Bohemians, of whom the mean age of primary menstruation was sixteen years and two months; 148 menstruated first at fifteen, 51 at seventeen, 49 at sixteen, and 46 at eighteen; the youngest was ten, and the oldest twenty-four, at her first menstruation; the period of primary menstruation comprising fifteen years. *c.* 273 came from Moravia, the mean age of primary menstruation bein sixteen years and three and three-quarter months; 69 menstruated first at sixteen, 59 at fifteen, and 32 at seventeen; the youngest, 8 in number, were twelve, and the oldest, 8, twenty-two years old; the period of primary menstruation comprising thus only eleven years; one year less than in Vienna, and four less than in Bohemia and the rural districts of Austria Proper. *d.* 180 women came from Hungary, and of them the average age of primary menstruation was fifteen years; 39 menstruated first at fifteen, 19 at fourteen, 16 at thirteen, 12 at seventeen; the youngest was ten, the oldest twenty-two; the period of primary menstruation comprising twelve years. *e.* 67 women came from Silesia; the mean age of their primary menstruation was sixteen years and one and a half months; 21 menstruated first at sixteen, 14 at fifteen, 8 at seventeen, 87 at 18; the youngest was twelve, the oldest twenty-two years old; the period of primary menstruation comprising eleven years. *f.* 66 women came from Bavaria; their average age for primary menstruation was sixteen years and ten months; 17 menstruated first at seventeen, 15 at sixteen, 9 at eighteen, and 8 at fifteen; the youngest was eleven, the oldest twenty-three; the period of primary menstruation comprised thirteen years.

Brierre de Boismont fixed in Paris the age of primary menstruation at fourteen years and ten months for the poor, fourteen and five months for the middling classes, and thirteeen years and eight months for the rich; and with this the observations of Chomel, Andral, and Recamier agree. Of our author's 2275 cases, 136 were of the middling classes—in them the average age of primary menstruation was fifteen years and two months; 730 handworkers—medium age fifteen years and ten months at their first menstruation; 1207 female servants, of whom the mean age of primary menstruation was sixteen years and two months; and 202 day laborers, whose mean age at their first menstruation was sixteen years and one and a half month; these results agreeing with Boismont's observation, that menstruation is earliest among the rich, and latest among the poor. Among the 2275 women, the ages at which menstruation first occurred are thus divided:

At	10 years	2	women	menstruated.	At	18 years	259	women	menstruated.
"	11 "	15	"	"	"	19 "	147	"	"
"	12 "	36	"	"	"	20 "	95	"	"
"	13 "	149	"	"	"	21 "	34	"	"
"	14 "	204	"	"	"	22 "	30	"	"
"	15 "	628	"	"	"	23 "	4	"	"
"	16 "	335	"	"	"	24 "	2	"	"
"	17 "	283	"	"	"	25 "	2	"	"

The average age of primary menstruation in Austria being thus fifteen years and seven and a half months. As influential agents in hastening or retarding menstruation, Sz. enumerates geographical position, climate, populousness of a town, nationality, mode of life, occupation, food, clothing, dwelling, training, morals, and bodily organization; according to some authors, hereditary tendency is slso influential. Although in Austria menstruation most commonly occurs between the fifteenth and seventeenth years, many cases have been recorded of its much earlier occurrence. Wilson observed a case in which it occurred in the fifth year; the breasts being as large as those of a marriageable female. Scanzoni records one case at eight years; D'Outreport, one at nine months; she had protuberant breasts, and menstruated every four weeks till her death in her twelfth year. Boismont records two cases—in one menstruation commenced in the third month, in the other in the third year. Similar observations are recorded by Mad. Boivin, Dieffenbach, and Martin Wall. In France, early menstruation is more common than in Austria; as Boismont in 1200 cases, found fourteen who had menstruated before their tenth year. According to Szukit's observation, in scarce one-third was primary menstruation painful; while in France, according to Boismont, this was the case in more than two-thirds. In one case of dysmenorrhœa our author observed trifling acne rosacea on the chin and sides of the nose—the woman was twenty-seven years old. In three cases, each menstrual period was attended by an eruption of uricaria over the entire body; and in two girls, one fifteen, the other seventeen, the molimina of primary menstruation were attended by inflammation of the cellular membrane, which again disappeared on its sessation in three to five days after. Strong healthy women generally menstruate more sparingly than feeble, anæmic, or tuberculotic females; the latter, if not laboring under amenorrhœa have profuse watery menstruation, lasting from ten to fourteen days. Of 1013 women, 240 menstruated sparingly, 560 moderately, and 187 profusely; of the latter, 26 passed clots without there being any uterine lesion present. Of 1013 women Sz. found that only 642 menstruated regularly every twenty-eight to thirty days, 371 menstruated irregularly, 269 menstruated from every eight days to every three weeks, 128 only every five or six weeks. The duration of menstruation is influenced by the same causes which influence its early appearance—its medium duration is 3 4-5 days. Three times, Sz. saw menstruation occur for a shorter or longer period during pregnancy; also during lactation it was frequently seen. Trustworthy cases are recorded in which menstruation only occurred during pregnancy or after delivery. During fourteen years, and in 8000 cases, 14 occurred of total absence of menstruation; four of these had borne children repeatedly, the others never; most of them had from time to time, every three or four weeks, molimina menstruations; in none was there any vicarious menstruation; in two cases imperfect development of the uterus was discovered. Of 263 women menstruation ceased in 99, between forty-six and fifty years; in 77, between forty-one and forty-five; in 42, between fifty-one and fifty-five; in 26, between thirty-six and forty; in 15, between thirty and thirty-five; and in 6 cases between fifty-six and sixty years. The youngest woman who had ceased to menstruate was thirty-two; the oldest sixty-one, at the period of cessation. This may occur suddenly, without interruption to the general health, or it may take several years for its completion. Boismont reckons the average of this at two years. Sz. found the mean duration of uterine power to be thirty years. 125 women menstruated from twenty-one to thirty years; 106, from thirty-one to forty years. The shortest period of menstruation was twelve years, and happened in two cases; the longest, forty-five years, likewise occurred twice. Boismont found one case with a duration of only five years, and another with a duration of forty-eight. Of 863 women, 64 were never pregnant; 124 conceived once; 73, five times; 63, six times; 74, seven times; 38, eight times; 32, nine time; 27, ten times; 11, eleven times; 15, twelve times; 13, thirteen times; 5, fourteen times; 2, fifteen times; 7, sixteen times; 2, seventeen times; 3, eighteen times; 1, twenty times; 2, twenty-two times; 4, twenty-four times—863 women of whom 61 aborted; each woman averaging about five conceptions. The most usual period of conception in Europe is between the fourteenth and fiftieth year; but cases of child-birth in the sixtieth year, are recorded by Meissner, Bernstein, Osiander, Mende, and Busch. Haller saw one case of child-birth at sixty-three, and another at seventy; as also Labatt in Dublin, and Capuron, in Paris. [*Zeitschrift d. Wien Aerzte*, and *Ed. Med. Jour.*]

TREATMENT OF DIPHTHERIA.—[As this disease is now in our midst, and as the treatment here laid down coincides with that which we have found successful, we copy it from the London Lancet.—D. W. B.]

Treatment—1. A temperate, dry, well-ventilated room as can be obtained, no one being allowed to sleep in it except an attendant. Crowded bedrooms and animal effluvia are an exciting cause.

2. A calomel purgative, varying in strength, according to the age and size of the patient; and in children, where symptoms of laryngitis appear, a rapid exhibition of the chloride of mercury, such as a grain to two grains every hour till the breathing is easier, and then every three or four hours, till the false membranes are loosened, and the bowels evacuate green stools, or vomiting. Care is needed not to carry the mineral too far, but it can be borne in proportion to the strength of the patient and the sthenic form of the attack. Children who have been healthy, and are teething, have most inflammatory symptoms.

3. The decoction of cinchona with hydrochloric acid, varying the dose of the latter from one minim to ten every four hours, in from a teaspoonful to two tablespoonfuls of the former.

4. Gargle with chloride of sodium and vinegar, a tablespoonful of each in a teacupful of hot water; also inject this up the nostrils when they are becoming obstructed. This excels all other gargles; it relieves the breathing and the fœtor, and causes the ulcers to heal.

5. Apply the stick of nitrate of silver to every part where false membrane or exudation can be seen. By means of Dr. R. Quain's tongue depressor, one can see far and wide; but when the patient will not submit to this, and when the disease spreads beyond the reach of the caustic case, a probang and a clean sponge well saturated with a strong solution of nitrate of silver will answer.

6. Rub the external fauces with compound iodine ointment night and morning; and when erysipelas may appear, apply the stick, and lay on a plaster of strong mercury ointment.

7. Keep the room and all else sweet and clean.

8. A nutritious diet is necessary. A little mutton every day; boiled milk, rich gruels, and beef-tea, with hot port wine-and-water, (half wine, with sugar and lemon,) for all above ten years; and warm milk-and-water for minors. All things should be taken warm. Cold drinks are an exciting cause.

The disease is not infectious, except, perhaps, under extraordinary circumstances.

I could illustrate these remaks with a few cases of different intensity; but I fear to encroach upon your space.

I am, Sir, yours, most truly,

RICHARD CAMMACK, Jun.

Bennington, Boston, Sept. 1858.

FATAL DISEASE OF THE APPENDIX VERMIFORMIS.—By Dr. CASPER WISTER.—The appendix vermiformis exists only in man and two superior species of the quadrumana, the ourang and the ape. It is the type of the prolonged cæcum common to all other maniferous animals. During fœtal life it is continuous with the cæcum, and of the same size, and is developed from the cul de sac of the latter. Bishoff admits that the earliest recognition of its origin was by Meckel, in a fœtus seven lines in length.

The appendix is ordinarily three inches long, with a diameter of two lines, but is subject to great variety of size as well as position. Occasionally it is entirely absent, while Welga has seen it nine inches long, with many convolutions, and Autenrieth four, and as large as the colon.

It is entirely enveloped by the peritoneum, which forms for it also a true mesentery, holding it loosely in position. The muscular coat is not in bands, as in the colon, but surrounds it as in the rectum. The lining membrane is continuous with that of the cæcum, and forms a valve at the entrance, the falciform edge of which looks towards the right and downwards. This valve, in most instances, prevents the introduction of foreign bodies, but permits free exit to mucus, of which in adults there is a large supply, with rarely any admixture of alimentary matter; in infants, on the contrary, it is filled with meconium. The glands within are very numerous, and of the solitary variety. The extremity of the appendix is found at times in the pelvis, more commonly posterior to the colon; its position is entirely uncertain.

The function of this body is supposed to be similar to that of a gland; it furnishes an acid secretion which causes the chyme in the cæcum suddenly to acquire the property of reddening litmus paper.

The subject of the following notice was a boy of five years of age, fair skin, light eyes and hair, and delicate physical development, the son of a father lately dead with phthisis.

The child was visited professionally on the first of November; he was at that time out of bed and in his ordinary dress. His expression was languid; he had much fever, with a pulse of 130; his tongue was covered with a slight white deposit, through which the papillæ were projecting, the tip and edges being unusually red. He complained of pain in the abdomen, describing it vaguely as "stomach-ache," without giving it any locality. This pain was spasmodic, recurring at short intervals; sharp and griping, but followed by complete relief. At this time there was no pain upon pressure, or soreness over the abdomen. On the afternoon of the day but one preceding, an ordinary walk had been followed by complaints of much weariness, and in the course of the night by the first expression of pain, and this without any chill. Throughout the following day there had been some fever and pain, but this was not thought of sufficient gravity to require medical interposition. There had been no motion of the bowels during the forty-eight hours immediately preceding the first professional visit; and at that time the case had the aspect of an ordinary slight attack of colic, with the sympathetic fever natural to a child of sensitive organization, teeming with sympathies ready to respond to any slight disturbance of the economy. A mercurial purgative was directed, and a febrifuge at intervals. There had been no unusual article of diet taken, except a moderate quantity of chestnuts. The night was much disturbed, with an increase of both fever and pain; the latter still in paroxyms.

The morning of the 2d, in consequence of the purgative not having operated, a copious enema was administered, which produced a large fecal discharge, none of it of a dry or impacted description, but contained distinct evidence of the operation of the mercurial. The child expressed a sense of relief; and, after a dose of oil and a fomentation adjusted over the seat of pain, he was confined to bed.

The following morning, Nov. 3d, there was a spontaneous motion of the bowels, without any improvement; there was now tenderness over the abdomen, although during the paroxysmal pain pressure with the hand was grateful. This pain continued the chief subject of complaint, engrossing the child's mind, and provoking constant expression of distress. We commenced frequent doses of small quantities of calomel, with full proportions of ipecacuanha and opium, and employed gum in solution largely as a diet.

On the 4th, the symptoms wore more grave, greater pain upon pressure, some tympanitis, and soreness attending all motion of the person; the spasmodic pain still, however, caused most complaint, and recurred at short intervals. Leeches were now applied over the entire abdomen, all other treatment being continued except the use of ipecacuanha; this was withdrawn in consequence of slight vomiting. After leeching there was less complaint of pain upon pressure, and soreness and nausea disappeared, while stimulation was found necessary.

On the morning of Nov. 5th, he presented excessive tympanitis, constantly recurring paroxysms of pain, great restlessness, sunken features, some tenderness upon pressure, and a pulse of 140. The child was, after consultation with Dr. Wood, placed under the full effect of opium, with an increase of the mercurial, and a blister over the abdomen. The rectum was relieved of a large accumulation of gas by the introduction of a catheter, but all motion of the bowels was in vain solicited. Under the full effect of the opiate there was less exhibition of pain and restlessness, without any arrest of disease; tympanitis greatly increased, and compressing the stomach caused regurgitation of fluid, and much depressed the vitality of the entire organism.

Nov. 6. Small quantities of oil of turpentine, with full doses of the officinal solution of morphia, were administered, accompanied by further stimulation. Gradual sinking under the unchecked march of disease, aided by the accumulation of gas, terminated in death on the 7th of the month, seven days after the commencement of treatment; this being the result of a disease deficient in symptoms sufficiently distinct for satisfactory diagnosis, and only decided in the steadiness with which it resisted treatment.

After death the abdominal cavity was found dry, with indications of peritoneal inflammation, consisting most conclusively in a few patches of pasty lymph upon the intestinal surfaces. There was an engorgement of the vessels of the intestines, but, being in longitudinal bars from the diaphragm to the pubis over the entire mass, there was some doubt as to the time of its occurrence, particularly as signs of incipient decomposition existed elsewhere. The intestines, from the pyloric orifice of the stomach to the sigmoid flexure of the colon, were greatly distended with gas, and contained, besides, a large amount of fluid fecal matter. The stomach was empty and much contracted, the accumulation of gas having pressed it against the diaphragm, and in so doing caused the latter to encroach upon the thoracic viscera, thus rendering the last hours of life a most painful exhibition. The lining membrane of the intestinal canal was healthy throughout, and the cause of death was not manifest until the appendix vermiformis was examined. This was perforated at the lower extremity; the opening being large, with ragged edges, indicating ulceration, and partially filled by a plug of concrete matter of the size and much the shape of a date-stone, composed of small seeds, many of these from figs, associated with concrete matter deposited in layers, and arranged in concentric circles.

In this instance, the extremity of the appendix rested in the pelvis upon the rectum, immediately over the fundus of the bladder; here an effusion of the contents of the intestines had occurred, but in small quantity, owing to the plug having filled the ulceration it had caused in the appendix.

The matter effused had given rise to excessive inflammation and the formation of much lymph, an attempt having been made to form an abscess and enclose the offending fluid. This deposit of lymph extended over a surface of three inches in diameter, was dark in hue, passing into gangrene; and, although adhering strongly to the intestinal surfaces, left them, when removed, healthy in appearance. Those surfaces of the sigmoid flexure of the rectum facing each other in the natural position were strongly adherent for a distance of three inches, and consequently prevented peristaltic motion at this point. This explained the retention of gas after the peritoneal symptoms became decidely marked, although there had been a free passage of fecal matter in an early stage of the disease.

Again, the son of a very distinguished physician of this city, 22 years of age, passed Sunday evening in the society of his family and a small collection of his father's friends; he was gay and apparently in good health.

A few days previously he had complained of vague pains in the abdomen—not, however, of a degree to attract particular attention.

He retired on the evening above mentioned without any unusual sensations, but awoke before daylight with intense colic. This increasing, his father sent for a medical friend, after exhausting his own resources and failing to procure relief. Throughout Monday the symptoms of colic continued without abatement, and as the day wore on only presented indications of peritoneal complication. There was a large, though not excessive accumulation of gas—the most prominent feature of the case being excruciating pain, from which only partial relief was procured by complete narcotism. The bowels resisted every attempt to procure evacuation of their contents after the earliest stage of the disease.

On Monday night and Tuesday morning the agony of the patient began to abate, but only to be followed by collapse, with cold damp surface and failing pulse; he gradually sank, and died in the course of the morning, after an illness of thirty hours.

The abdomen was found after death much distended by gas, free from effusion, and not affording any evidence of excessive peritoneal inflammation. The effect of disease in this case could be traced with much confidence, as the examniation was made on Tuesday afternoon, a few hours after death, and but a short time was allowed for decomposition to embarrass investigation.

The appendix was much distended at its lower extremity by a concrete mass composed largely of raisin seeds. Its walls had not been perforated, and contained, with the foreign matter, an accumulatien of pus, the whole forming a mass the size of a walnut. The appendix partially embraced the ilium, adhering to it strongly, and completely strangulated the small intestines, which were, for a distance of six inches above, of a dark mahogany color, and passing into gangrene. At the point of strangulation there had been much inflammation, with large effusion of lymph; the walls of the tumor were thin, bursting under pressure of the fingers; but, having retained its contents during life, there was no discharge of foreign matter into the peritoneal cavity, giving rise to peritonitis and masking the earlier symptoms of colic, as in the case just described.

Moreau cites a case identical with this, the ilium having been strangulated by adhesion of the appendix to the mesentery; while Marteau has seen the jejunum, and Scarpa the colon, embraced in the same manner and with a like effect.

Klockhof reports a case in which the appendix adhered to the colon by its extremity, thus forming a loop through which the small intestines had passed, producing strangulation.

Merling, in examining a subject, found the appendix adherent to the colon by the lower extremity, and forming a free communication between, different sections of the intestines, but was unable te learn anything of the previous history of the individual, or under what train of symptoms this union occurred, evidently followed by entire recovery.

Again, the appendix is at times metamorphosed into a hydropic capsule, as reported by Rokitansky, from the presence of a concretion in the tube closing the canal and preventing escape of mucous secretion. This causes the extremity to become dilated, and the mucus to pass into the condition of a serous membrane secreting an albuminous fluid.

Besides the foreign matter collecting in the appendix mechanically, tuberculous and typhoid deposits are laid down at times in its walls, giving rise to ulceration. When, in addition to these varieties of disease, we consider the fact, that a long time may be required to establish the existence of serious symptoms, since it is asserted that a concretion may exist and only give rise to blennorrhœa, a correct diagnosis is only equalled in difficulty by its importance. Ulceration of the appendix is not necessarily fatal, but forms abscesses at times pointing in various directions, according to the variety of directions assumed by the appendix; and it is of great moment that these should be anticipated and discharged at the earliest period possible, and the risk of an internal rupture of their walls diminished. In the cases cited above, the earliest symptoms were of colic, excepting the obstinate constipation, the bowels having been freely evacuated without relief. In both, the earlier symptoms were overlaid by peritonitis; while in one there was an attempt to form an abscess, the constitution failing before it was accomplished.—*Transactions of the College of Physicians.*

ON A NEW MODE OF CAUTERIZATION.—By M. MAISONNEUVE.—In proportion as the field for the employment of caustics has been extended have the procedures made use of become improved. Thus, the poisonous caustics, as arsenic, have given way to alkalis or strong acids, unattended with danger of absorption; while these, again, have been displaced by the metallic chlorides, especially the chloride of zinc. This, indeed, possesses in an eminent degree all the qualities sought for in a caustic, viz.: absolute hæmostatic power, extreme destructive energy, complete harmlessness, and the greatest facility of application. Nevertheless, cauterization has continued to be regarded as but an exceptional remedy, its defects rendering its employment as painful for the patient as difficult for the surgeon.

First among such defects is the insufficiency of the destructive action, three or four applications being required to compass the result, especially when the lesions calling for its use are situated at some depth in the tissues. Of still more consequence is the difficulty the surgeon experiences in exactly adapting the caustic to, and maintaining it in contact with the parts intended to be acted upon: rendering the method dangerous or impossible for the destruction of deep-seated tumors of the rectum, pharynx, neck, etc.

On reflection the author believed these inconveniences to be much rather due to the imperfection of the procedures employed than to the method itself. In fact, if we examine the different procedures, multifarious as these are, we find that they all pursue the same method of cauterizing from outwards inwards, in successive layers, so as to disorganize the tissues from without inwards. For a long period indeed, a single procedure was resorted to, viz.: covering the diseased part with a layer of caustic, and repeating the applications according to the thickness of the substance to be destroyed. The pain and delay thus induced are well known. Recamier and others afterwards conceived the happy idea of circular cauterization, by means of which tumors were attacked at their circumference and separated from their base without their total destruction becoming necessitated. It, however, was only occasionally applicable, and participated in the other defects of the procedure of cauterizing from outwards inwards, of which, indeed, it was only a modification.

M. Maisonneuve resolved to practice cauterization upon another principle, and his success has surpassed his expectations. By his method, not only is the surgeon spared the delays and difficulties of the old procedure, but he is enabled, almost as well as with the knife, to at once penetrate to any depth, to act with safety in the vicinity of important organs, and to execute delicate dissections; and that without the effusion of blood, and with a promtitude that admits of his having recourse to anæsthetic agents. In place of being applied externally, and acting from outwards inwardly, the caustic is carried at once into the depth of the tissue to be destroyed, and acts from within outwardly. Of all the caustic substances he gives preference to Canquoin's, consisting of chloride of zinc one part, and flour three parts, with water sufficient to form it into a paste. This paste is spread out into a sort of cake, and cut into pointed strips of various forms and dimensions, termed by M. Maisonneuve "arrows," and which are then desiccated so as to acquire the requisite solidity and resistence. When the tissues are soft and friable the arrows have consistence enough to penetrate into their substance; but when the skin is entire or the tissues are indurated, a preliminary puncture with a pointed bistoury is required, the arrow being at once introduced so that no blood whatever issues out. There are three descriptions of this form of cauterization described by the author:

1. *Circular or Radiated Cauterization.*—For this, arrows of a conical form are employed, and are disposed circularly, at intervals of a centimetre, around the base of the tumor, constituting together a cone or plane circumscribing the tumor, and isolating it from the healthy parts. As the portion of living tissue comprised between each arrow is very thin, its destruction is brought about in an hour or two; and the tumor, deprived of all vascular or nervous communication, dies without its having been necessary to operate its direct disorganization by means of the caustic. This special property of insuring the mortification of voluminous tumors in a few hours, while acting on only a very thin layer of the tissues, giving rise to no effusion of blood, and scarcely any traumatic reäction, and preserving from the terrible accidents of purulent infection, will constitute this procedure one of the most precious resources in surgery. This form of the cauterization is especially adapted to tumors of a certain size, projecting considerably from the surface of the body, as tumors of the breast.

2. *Parallel Cauterization.*—For this the caustic is cut into little pointed laths, very like our vaccine points. They are not placed circularly around the base of the tumor, but parallel to each other over the whole of its free surface, so that its interior consists of a kind of bundle of caustic arrows, between the interstices of which the tissues are reduced to great thinness, and are rapidly destroyed. This does not, like the former procedure, confine its action to the interruption of the vascular and nervous communications of the tumor, but operates its direct disorganization by penetrating its entire substance. It causes more pain, as it implies the introduction of many more arrows; but its action is no less rapid and efficacious, and it is applicable to special conditions. Thus it is especially so in tumors difficult of access, or deeply placed, and making but a slight projection at the surface; as in tumors of the axilla, groin, and neck, and especially in fungous degenerations of the cervix uteri, vagina, rectum, etc.

3. *Central Cauterization.*—For this the arrows are cut fusiform, and are introduced into the centre of the tumor. A puncture is made in the

tumor by means of a bistoury to a little beyond its centre or a little cavity may be thus made in this centre; and immediately on withdrawing the knife, one or more of the arrows are pressed in until they have completely disappeared amidst the substance of the tissues. A thick eschar is produced, without any serious disturbance being manifested externally. It is discharged by the same orifice the caustic was introduce at; and the surgeon can then repeat the application of the arrows until the whole of the tumor is emptied from within outwards, reducing it to a kind of shell, which gradually collapses and cicatrizes. This, although a less powerful and energetic means than the two former, is of great value in the destruction of certain tumors inaccessible to all other means, as interstitial tumors of the uterus, or for the removal of certain superficial tumors, without injury to the skin covering them, as enlarged glands of the neck, axilla or groin. M. Maisonneuve has also employed it with success in tumors of the tongue.—*Moniteur des Hôpitaux*, No. 115.

DIPHTHERIA AND ITS CONNEXION WITH A PARASITIC VEGETABLE FUNGUS.—*Letter from Dr. Wilks.*—Sir: Opinions still vary as to the true nature of diphtheria, and therefore as to its connexion with a parasitic fungus (oïdium albicans). As on several occasions the white film on the throat has been found to consist of this fungus, it has been conjectured whether the malady is not really one having a parasitic origin, and the belief has been rendered more probable from the fact that several new diseases have of late prevailed throughout the organic kingdom, both animal and vegetable, which are clearly traceable to parasites; for example, the oïdium of the vine. Unfortunately, those practitioners residing in districts where diphtheria has been endemic have been silent on this point, and it has only been by occasional observers that the fact has been made out. In the few cases of the disease which I myself have seen, a fungus has always been present, and thus my belief was, until lately, growing strong that in this observation would be found the true character of the malady, or, at least, that the parasitic growth was intimately connected with it, the question still remaining open whether the formation or growth of the fungus is the primary process, or whether a diseased condition of the surface must not previously exist to prepare a suitable nidus for its development; a question still debated in connexion with other parasites as the porrigo lupinosa (faveuse), a disease in which some cutaneous inflammation is generally found, and thus creating a doubt as to whether this is excited by the fungus, or whether an herpetic or pustular eruption does not previously exist to form a suitable soil for the sporules which are afterwards sown in it. Let the formation in the throat be primary or secondary, it still remains important to know whether its presence is an essential part of the disease; indeed, the spreading character of the pellicle its separation and destruction by corrosives, are all facts which seem to indicate that many features of the disease are due to its existence.

My attention being directed to this matter, I took the opportunity to examine the films which occasionally form on the mouths of those sick with various diseases; and on submitting them to the test of the microscope felt some surprise in witnessing in all fungous growth which I have not been able to distinguish from that of diphtheria. Thus, I lately had a woman die under my care in Guy's Hospital, with acute cerebral and spinal meningitis, pleuritis, etc., of a supposed phlebitic origin, and on examination of the pharynx after death, a pellicle was found composed of the parasite. Again, a child 4 years old presented itself among my out-patients apparently dying with croup, but on examination was found to be suffering from an extension of diphtheritic disease into the trachea. The throat and tongue were covered with a white pellicle, a portion of which being placed under the microscope, displayed very readily the oïdium; the only difficulty about the case being the statement of the mother, that the child had suffered with a throat affection for several weeks. Mr. Hardy (a student) at my request kindly followed the child to Woolwich, and made a post-mortem examination. The throat, trachea, etc., were covered with a pellicle, as before said; and on removing this to find a cause for the chronic symptoms, a polypus of a papillary character was seen growing from one of the vocal cords, with thickened tissue around.

Here was an explanation of the chronic symptoms; and upon this had arisen an acute inflammation, accompanied by the fungus. Another case was that of a man who died last week under my care in the Hospital with softening of the spinal cord. A few days before his death his mouth and tongue became covered with a white secretion, which very rapidly formed a complete layer over the whole buccal surface. An examination of this by the microscope showed a remarkably fine specimen of the fungus, the mycelium and sporules exhibiting themselves to perfection. On mentioning these circumstsnces to Dr. Barlow, he stated that he had under his care a child with a white film on its mouth (the case not being one of diphtherite), and he sent me some of the secretion for examination, when I found it to resemble the specimens already named; and the same occurred in one or two other cases which I have seen, but need not detail. These facts are sufficient to show that a vegetable fungus may spring up on the buccal mucous surface in various cases of disease, but requiring probably some previously morbid condition for a nidus. Is it not so in diphtherite? Is the disease, strictly speaking, a malignant sore-throat, and the formation of a pellicle an accident; or is the latter an essential part of the affection? In the case of the child just mentioned, if no post-mortem examination had been made to discover the chronic disease, the case would have been called diphtheria; and in the man with spinal paraplegia, the condition of the mouth would have been sufficient to have marked it a case of the same kind had there been no other affection present. Such cases may throw some light upon the opinion of those practitioners who, not residing in diphtheritic districts, and who see only isolated cases, regard the disease as a mere modification or peculiar form of some ordinary maladies, as cynanche and scarlatina, and this may in some instances be correct. In speaking of the parasitic growth found in the above-mentioned instances we are aware of the objection which can be made—that the fungus of diphtheria is peculiar (supposing it always to be present), and that found in the mouth of other sick persons is in connexion with apthæ, and is another variety. In answer I can only say that I failed to discover in the above cases any difference, and, moreover, the character of the pellicle and its rapid extension over the whole mouth, throat, and tongue, was totally unlike ordinary aphthæ.

My object in bringing the subject before your readers is, that some may extend these observations, and note how far throat affections, with these peculiarities, are prevailing in other patients besides those with true diphtheria; and also that those gentlemen who are seeing much of this latter disease, will confirm or not the observations made by myself and others that the pellicle is *always* composed of a vegetable parasitic fungus. When more facts are ascertained on this point we shall be better able to judge of the characters of the disease.

I am, etc.,

SAMUEL WILKS, M. D.

17 St. Thomas St., Southwark.—*London Medical Times and Gazette.*

NEW MODES OF ADMINISTERING IODINE.—Efforts have lately been made in France to administer iodine in a more efficacious manner than had hitherto been done. M. Leriche, of Lyons, has published valuable articles in *L' Union Médicale*, wherein he endeavors to show that iodine, combined with vegetable substances, advantageously replaces cod-liver oil. He proposes a syrup made of the juice of water-cress and iodine, and also an iodine wine. The syrup has the advantage of not fermenting, and contains exactly one grain of iodine per ounce. The wine is composed thus: Bordeaux wine, eight ounces; concentracted infusion of red roses, about thirteen drachms; tincture of iodine, one drachm and a half. Each ounce contains one grain of iodine. From one to six tablespoonsful may be given daily, according to the indications and the age of patients. In the space of three years M. Leriche treated thirty-eight scrofulous patients with the wine; twenty-one were perfectly cured after a treatment steadily pursued for some time; eight did not improve at all; and nine improved but slightly, either because the treatment was carried on imperfectly, or because it was left off too soon.

M. Boinet, on the other hand, well known by long-continued investigations respecting the use of iodine, read on the 28th of September last, before the Academy of Medicine of Paris, a paper, in which he proposes to use iodine as an article of food. The author administers iodine as found in nature—viz.: combined with those plants which contain the greatest quantity of the alkaloid. The latter being thus given in minute doses, in a continuous and almost imperceptible manner, yields most advantageous results. M. Boinet uses fuci, marine plants, cruciferæ, salts containing iodine, and some mineral waters holding iodine in solution. His excipients are, ordinary bread, ginger-bread, cakes, biscuits, chocolate, wine, beer, syrups, etc., some being especially calculated for children. Trials were begun by M. Boinet as far back as 1849, upon subjects suffering very severely from the various well-known scrofulous symptoms, and most of them were cured after continuing the iodized food for several months. The author has not found that iodine administered for a long time produced loss of flesh and atrophy of certain organs. Far from having these effects, the iodine in his hands, has invigorated patients, and favored the development of organs. Messrs. Chatin and Trousseau are to report upon the paper.

THE USE OF GALVANISM IN TOOTHACHE.—As the anæsthetic power of galvanism, as employed in dental surgery, is at present attracting much attention, it may be interesting to some of your readers to learn that its application in cases of toothache, will, as far as I am at present able to judge, instantly and effectually remove pain.

I have lately used this remedy in a bad case: the patient suffered excruciating agony, lost her rest, and was unable to attend to her domestic duties in consequence. The tooth was so much excavated and decayed, that I do not suppose any dentist would have attempted the operation of plugging. A slight touch of the conducting-wire instantly removed the pain; and its subsequent application for a few seconds was attended with complete relief, which appears to be permanent, as there has been no return of suffering for the period of five weeks which has elapsed since the operation.

I am, Sir, your obedient servant,

CHARLES TAYLOR, M. D.

Walton Lodge, near Liverpool, Oct. 1858.—*London Lancet.*

NEW TRIBE OF ABORIGINES, WITHOUT HAIR.—The discovery of a new tribe of aborigines is thus reported in the *Sidney Empire*: "A gentleman who, in May last, was at a remote station down the Balonne, called Gooee, about 100 miles below Surat, fell in with four blacks, who had come to that part of the Balonne a few days previous, and who appeared to belong to a tribe unknown to white men. They presented the remarkable peculiarity of being entirely without hair, and they stated that neither the males nor females of their tribes had hair on their bodies at any period of life. The complete baldness gave them a strange unearthly appearance, at which it is said the Balonne blacks were at first very much terrified. These aboriginal strangers said they saw white men's bones, and equipments beyond the river Barrow or Warrego, from which they had come. It is conjectured that these remains may be those of Leichardt and his party, and we believe the whole particulars have been communicated to the government, with the view of a fresh search being made to clear up the mystery of the long missing travellers."

INHALATION OF SAL-AMMONIAC IN CHRONIC CATARRH.—Dr. Giesceler, in the Druggists' Circular, speaks in high terms of the inhalation of sal-ammoniac in chronic catarrh of the bladder, etc. A drachm of dry muriate of ammonia is heated over a spirit lamp, and the patient inhales the vapor.

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THE PIOUS PHYSICIAN, OR THE CLAIMS OF RELIGION UPON THE MEDICAL PROFESSION.

A DISCOURSE

Delivered upon the occasion of the death of the late JOHN M. W. PICTON, M. D., in the First Presbyterian Church, New Orleans, Dec. 26th, 1858, by B. M. PALMER, D. D., Pastor.

TEXT—"*Brethren, be followers together of me, and mark them which walk so, as ye have us for an ensample.*"—Philippians, 3d ch., 17th v.

Through ten successive Sabbaths this drapery of mourning has been presented to your view, a fit emblem of the deeper sorrow which has shrouded our hearts. This earth can sustain no greater loss than in the death of a truly good man. There is one example less of successful virtue encountering the temptations of life; one illustration less of the power of principle nerving to the discharge of duty; one instance less of heroic self-denial in living for the good of others; and one exhibition less of that passive fortitude which endures bereavement and misfortune. The extinction of a single light must be deplored in a world so darkened by ignorance and sin as ours; and tears of honest grief must fall upon the graves of those whose lives were a continued exposition of the precepts of virtue and the maxims of religion. All this, in all its breadth, may be said of our deceased brother, in memory of whom this Church is now dressed in the weeds of mourning.

Dr. John M. W. Picton was born in the town of Woodbury, N. J., on the 16th of Nov. 1804. Blessed with pious parents, the seeds of religious knowledge were early sown, which, though long dormant, bore abundant fruit in his later years. His character was moulded under the influence of a mother, who was eminent alike for her intellectual attainments, and for her deep piety. His father, a Clergyman in the Presbyterian Church, still survives in the enjoyment of a green old age, bowing down under this heavy sorrow, and exclaiming with all the pathos of David, "My son, my son, would God I had died for thee."

At the early age of fourteen he was entered a pupil in the Military school at West Point; where, notwithstanding his youthfulness, he sustained himself with credit, graduating with distinction, possessing alike the love of his associates, and the esteem of his Preceptors. Having requited the State by seven years' service in the Army, he was married in his twenty-sixth year, and selected the Medical profession as his future calling. Two years of laborious study in the Medical school at Philadelphia prepared him to enter upon the practice of physic in this city; where he soon distinguished himself, attracting the attention of the entire Faculty, not only in this country, but also in Europe, by the splendid success of some surgical operations. Amidst the labors of an exhausting practice, he found leisure to pursue his professional and scientific studies, and thus to keep pace with the advancing knowledge of his times. Few men ever exhibited a fresher interest, or possessed more extended and accurate information, upon those questions of philosophy and science which so deeply stir the age in which we live. Next to the topics of experimental religion, his mind reverted to them with the greatest freedom; and his searching criticism easily penetrated those fallacies by which the unwary are seduced into skepticism and unbelief. It was indeed refreshing to observe a mind naturally most highly endowed, and richly cultivated and disciplined, determining for itself the boundaries of human reason; in the province of faith bowing with the docility of a child before the oracles of God, yet moving with adventurous step in all the departments which lie open to the investigations of science and the deductions of logic.

During the progress of a religious revival in the year 1838, Dr. Picton was first brought to avow himself a Christian, and united with this Church under the Pastoral care of Rev. Joel Parker, D. D. How consistently, through twenty years, he has adorned the Gospel of our Lord Jesus Christ, it were useless for me, in the presence of this Congregation, to proclaim. His professional calls, though often sudden and exacting, he was generally able so to arrange as not to interrupt his attendance upon the ordinances of the Sanctuary; and few men ever spoke from this desk without observing his fine intellectual face lighted up with thought and feeling, evincing the degree of his sympathy with the holy truths of the gospel. In 1845, he was chosen by the free and unanimous voice of this Church, to fill the responsible office of Ruling Elder, the duties of which he continued faithfully to discharge, during a period of thirteen years, till the day of his death. The maturity of his judgment, the clearness of his views, the strength of his convictions, the fervor of his piety, and his practical knowledge of mankind, preëminently fitted him for this high station; and those associated with him in office will cheerfully testify to the value of his counsels, and to the prudence and zeal with which he labored to preserve the purity, and to promote the piety, of the entire Church.

It was, however, in the seclusion of domestic life that Dr. Picton's character shone with peculiar beauty; as he was seen stripped of that reserve, which to some extent veiled him before the world. It was there, that relaxing the rigor of authority as his children advanced to maturity, he gently suggested the cautions of experience to temper the anticipations of hope. It was there he bore his household, morning and evening, before the Mercy-Seat above, and prayed as only a parent can plead for the eternal welfare of his children. But I must not rudely lift the curtain drawn around the sanctuary of his home; nor stir anew the grief of those who mourn over his loss with a bitterness of sorrow known only to themselves. Alas! we now know how much death can do! At a single blow, the tender husband, the loving father, the enterprising citizen, the genial friend, the prudent counsellor, the kind physician, the experienced instructor—all have fallen; all these relations laid prostrate at once in the grave which holds the manly form of one whom we shall no more see on earth. Yet at his grave we realize the unity of the Church of God, and that what is lost on earth, is treasured forever in Heaven. He who once mingled with us in the praises of this Sanctuary, is only transferred to a higher service, and engages in a more blessed worship among the Sons of God in the Temple above.

"Thou'rt gone to the grave, but we will not deplore thee;
Though sorrows and darkness encompass the tomb,
The Saviour has passed through its portals before thee,
And the lamp of His love was thy guide through the gloom.
Thou'rt gone to the grave, and its mansions forsaking,
Perhaps thy tried spirit in doubt lingered long;
But the sunshine of Heaven beamed bright on thy waking,
And the song that thou heard'st was the Seraphim's song."

It would be a pleasing task to fill up this meagre sketch, and to draw out at length the character and life of our deceased brother and friend. But the solemnities of this occasion demand of me a higher service than to pour the voice of praise into "the dull, cold ear of death." Had we power to bring him back to earth, he would be the first to put the crown from his own head upon that of his Master, and to say in the words of the text, "Brethren, be followers together of me, and mark them which walk so, as ye have us for an ensample." Especially might he address thus the class of which he was an honored representative, and persuade those of his own profession to take the yoke of Christ, and become obedient to the Gospel. I can pronounce no higher eulogy than to present him to your thoughts as the model of a Pious Physician; and as his voice, to urge the claims of experimental religion upon the Medical profession. It will be my aim therefore, in this discourse, to show the need of true piety as furnishing the brightest trait of excellence in the character of the Physician.

1. *The first consideration may be drawn from the principle of benevolence, in which the Science of Medicine undoubtedly originates.* It is impossible to determine what would have been the precise condition of the human race, had sin never been permitted to enter the world. Probably some of our most important and engrossing pursuits would be vacated, by the withdrawal of evils which now create the necessity for their continuance. We can at least confidently affirm that sin is the parent of those physical sufferings, which it is the object of Medical science to alleviate. This is the testimony of the Apostle Paul, "By one man sin entered into the world, and death by sin, and so death passed upon all men, for that all have sinned," In this passage, death is unquestionably a generic term, including all the sickness, and pain, and disease which find their consummation in death. No adequate explanation of them can be given, but that they are judicial visitations upon man for his violation of the Divine law; so that we "have the sentence of death in ourselves, that we should not trust in ourselves, but in God which raiseth the dead." But these sufferings alone could never have originated the science of Medicine, unless they had been contemplated with an eye of benevolent compassion. The various diseases which ravage the earth, might have swept on in their destructive march, without an effort or even a wish to impede their course, but for the instinct of human sympathy which leads us to mourn over the sorrows of others. While, however, the holiness of

God and the sanctity of his law are avenged in this punishment of sin through disease and death, his boundless compassion has furnished the remedies which human wisdom shall both discover and apply. To the minerals which are imbedded in the earth's bosom, to the plants which grow in such magnificent profusion upon her soil, to the medicinal springs which bubble upon her hill-sides, he has imparted healing virtues, designed to soften the asperity of his judicial inflictions. It matters not by what happy accident the curative properties of any single fountain or plant were first discovered, for in it, as in a seedling, lay an undeveloped science. It was

> "A spark struck into tinder to light the lamp of knowledge;
> A slight suggestive nod to guide the watching mind;
> A half seen hand upon the wall, pointing to the balance of comparison."

The same benevolence which before wept with unavailing sympathy over the sorrows of mankind, now sought eagerly

> "To learn upon a hint, to find upon a clue."

Nature has been searched through her whole domain, yielding up her secret and long hoarded treasures to the relentless interrogations of science. Her products have been subjected to the tortures of analysis, till their hidden and essential principles have been eliminated and held apart. The human system has been laid bare to the inspection of the anatomist, until the seat of every organ has been defined, their respective functions accurately described, the reciprocal influence of each upon the rest carefully noted, and the action of every remedial agent experimentally demonstrated. In short, a benevolent curiosity has so pushed its researches, that in the lapse of ages Medical science has grown into a grandeur of proportions which compels the homage of mankind. Whatever gibes it may encounter from the thoughtless, because at first empirical and uncertain in its researches, it never fails to vindicate itself in our hours of peril and of pain, and to wring the homage which it is the shame of many so reluctantly to pay.

These hints are scarcely superfluous if they recall your attention to the fact that however the profession of Medicine may take its spring in the sufferings of mankind, it owes its birth not less to that instinctive benevolence of the human heart, which cannot look upon distress without a wish to heal; and which walks up and down the earth, crying aloud, "is there no balm in Gilead? is there no physician there"? Of this benevolence the Medical profession is the special exponent; beyond all others it represents, as a class, the power of this sentiment, than which there is none more noble in human nature. Other men *may* be benevolent—the physician *must* be. Other men do good upon *occasions*—the physician *habitually* and by the very import of his calling. Other men relieve the destitute and the suffering by indirection, giving their substance for the erection of Hospitals and Asylums, distributing their alms by a proxy; the physician, in his own person, gives the sweat of his brow and the labor of his hand, the anxieties of his soul and the toil of his intellect. Amid the sultry heats of summer as well as in the frost and sleet of winter—in the glare of noon, and in the midnight hour, when deep sleep falleth upon men—when worn with watching, and exhausted with fatigue—with the body struggling against incipient disease, and with the heart often bowing under the pressure of private sorrow; at all times the calls of humanity must be promptly obeyed; and they must go with equal cheerfulness into the hut of the pauper as into the palace of the prince. I am uttering, not one of the common-places of flattery, but the profound conviction of my own heart, when I say that no class in society renders so large an amount of unrequited service to their fellow men, as the Medical class. Their ethical code, which only gives formal expression to the purest impulses of our common nature, forbids the question of fee or reward to be raised, when first summoned to the bedside of the sick. Too often it is their misfortune to experience only the basest ingratitude of those, who in the first moments of relief would have kissed their hand as the kindest of deliverers. I know not who are more competent to bear testimony to the benevolent spirit in which the practice of Medicine is conducted, than the clergy. We go hand in hand with the physician into the chambers of suffering, and meet daily at the same bed-sides; and none should be able to judge better than we of the charity and kindness of Medical men. It is indicated in the noiseless tread with which they enter the darkened room; in the subdued and sympathising tones with which the sufferer is interrogated; in the gentle pressure of the hand upon the body as it is quivering with pain; in the cloud of anxiety which gathers upon the brow, as the disease works on defying their skill, to its fatal result; and in the overwhelming sadness with which as mourners they come to look upon those whom they vainly strove to save. Knowing how justly this tribute is deserved, I gladly seize this occasion to say, that through an official intercourse with Medical men, extending over nearly eighteen years, I have never found one of their number heartless and insensible—not one, however professionally cold and hard his exterior, who did not at proper times betray a true sympathy with the sufferings of his fellow men—not one who failed to represent the spirit of benevolence which underlies his calling—not one who did not deserve to be loved as a brother, and trusted as a friend. Though doubtless, like every other class, made up of mingled good and bad, I am sure that in no profession are the practitioners more pervaded with the characteristic spirit of their calling than in this, whose very commission is to go about doing good.

But how much more beautiful this benevolence, if instead of being a blind instinct of our nature, it were sanctified by religion and wrought with in the soul as a grace of the Spirit? How much more would the kind physician realize the ideal of his own profession, if, ascending the stream of this benevolence to it source, he could first love his Maker with supreme affection, and then love his fellow men as the children of a common Father? How much more complete his sympathy, if, looking upon sorrow as the effect of sin, he could compassionate men in this the spring of all their miseries? How much more God-like his vocation, if recognizing this brotherhood of race, he could come to them as the messenger of God—an angel of mercy, conveying that compassion which has its seat in the bosom of the Deity, and which through his agency is so freely distributed? Following the example of Christ, whose life was a life of self-denial, and whose miracles were miracles of mercy, he would not be a solitary worker amid a field of thorns, but a co-worker with God in carrying forward his purposes of love to the lost sons of men. His charity would not then be a single and earth-born excellence, but the grateful utterance of his indebtedness to infinite grace; and his professional career would be a sublime hymn of praise to One whose name is Love.

2. *True piety should adorn the Physician, as being the highest improvement of our spiritualnature, the recognition of which both the science and practice of Medicine equally compel.* It has been urged as a reproach against Medical studies, that they nourish a tendency to materialism. Dealing with the body as the seat of all diseases, tracing the immediate and material causes which produce them, and analyzing the properties of every remedial agent which can be expressed in the formulas of Chemistry, it is alleged that man comes to be viewed at last as only a higher species of animal, with a more delicate organization, it may be, but

still corporeal and fleshly only, like all the beasts of the earth. It should not surprise us if superficial minds should rest in shallow conclusions like this. They are easily reached by a first induction, and do not require the large comparisons by which all final truths are attained, and which can only be instituted by broader intellects. But that the master minds in this noble profession, whose original research is continually widening the circle of Medical knowledge—that these should acquiesce in generalizations of this sort, is utterly incredible; nor do I believe that the verdict of the Medical Faculty throughout the world would pronounce for a philosophic and scientific heresy so appalling as this. On the contrary, it falls within the observation of every intelligent physician, that man's soul rules the body in which it dwells, whose influence must be estimated in the pathology of almost every form of disease. Often the body is overwhelmed by a complication of disorders, any one of which is competent to hurry it to the tomb; yet the sinking frame is undergirded by an invisible and spiritual power, and the patient lives on apparently by the simple energy of his own will. Again, by an irrepressible sympathy with the mind, the body, almost free from disease, droops under the morbid influence which this spiritual tenant has the power to exert. Its juices are dried up from within, and after a feeble resistance it succumbs beneath the spiritual spell by which it has been withered away. It is precisely here, in the plane of this intersection of matter with mind, and in their reciprocal influence upon each other, that the most insoluble problems in Medical science are to be found. Dark as may be the mysteries which hang around the origin of many diseases, they are not hopelessly beyond the reach of scientific research. Future experiments may reveal the tests by which they shall be laid bare to Medical inspection. But who may hope to detect and imprison for scientific manipulation that immaterial essence which we call the soul, or to lift the veil which conceals its inner working from the gaze of profane and curious eyes? It is only by collecting and classifying the facts of our own consciousness, and by eliminating thus the laws of our spiritual economy, that the secret processes of thought and feeling can be traced. These metaphysical inquiries are so far from being impertinent, however they may be derided by some, that the skilful practitioner is often guided by them alone in determining the character of many affections of the body, which otherwise were wrapt in impenetrable obscurity. I am greatly in error, if this is not one of the special advantages enjoyed by the family physician over the occasional

Medical adviser. The former has opportunities of which he dilligently avails himself, to study, not only the idiosyncracies of the physical constitution, and the traditional morbid tendencies which descend from parent to child, but also those intellectual and moral characteristics which continually interpose to modify, in the individual, the operations of disease. It is for this purpose that he visits his patients in seasons of health as well as of sickness; whilst apparently solacing himself with the delights of social intercourse and friendship, he is in fact drawing out all the features of his patient's character in its sound and normal condition, and is often able to predict with almost unerring certainty how, under the invasions of disease, each bodily organ shall be affected.

The Physician cannot, therefore, ignore man's higher and spiritual nature without exhibiting himself to the world a mere empiric. He cannot rise into the loftier walks, nor solve the deeper problems, of his own profession, after discarding principles that are fundamental in his science. As a mere practitioner he must often fail of success when called to "minister to a mind diseased." It is difficult to restrain the expression of disgust which is excited by so low a view of human nature. As the philosophic materialist, in his repudiation of the human soul, spunges out that image of God in which man was made, and finds himself shunned as an apostate from mankind; so the Medical materialist, by lowering an august profession to the level of simple quackery, should not be surprised to find himself dismissed to the grade of the petty hucksterers who vend their nostrums in his own art. But that materialism forms no part of the medical creed is abundantly shown from the ready access which clergymen gain to the bedside of the sick and dying. With very rare exceptions, Ministers of the Gospel who possess ordinary discretion, are hailed as the Physician's most useful allies. Obvious reasons must exist in the nature of the disease, or want of confidence must be felt in the Pastor's prudence, in order to justify his exclusion from the chamber of sickness. What is this but a free confession that man's immortal part rules the mortal; or that his immortal interests should not be perilled through neglect?

This recognition by Physicians of man's spiritual and religious nature sweeps on to grave and practical conclusions. How shall the Physician compass the highest excellence in his chosen calling, if he cannot appreciate the cravings of the human soul, nor understand the blessings by which those cravings may be met? Simply therefore to reach the highest type of professional skill, he must thoroughly comprehend the whole complex nature of man; and know by experience the value of religion in meeting the wants of the human soul. How shall he, if ungodly, bring himself in closest sympathy with that large and increasing class of patients who feel the transcendent importance of those very interests which he overlooks or denies? Let it be remembered that this Gospel of Jesus Christ, in its sublime march to universal conquest, is gathering larger multitudes beneath its sway; and whatever respect may be shown to simple professional skill, true piety with the pious will always be a bond of sympathy. If too, this soul is of paramount value to the patient, is it of less moment to the Medical adviser? If in all his professional practice he defers to it as the power which should rule over the corporeal part, why should not its supremacy be acknowledged in himself? He is not less than others a full man—and that religious nature which commands his homage in others, claims as much when seen in himself. Each patient becomes thus a monitor, bidding him find the highest improvement of his own character in that true piety, which is but the lost image of God restored to the soul. It is plain, therefore, that Christianity presses its claims upon the Medical profession by their own confession of judgment; and that the cap-stone of excellence is not added, and the climax of perfection reached, until the fear of the Lord crowns their other attainments, and consecrates the whole to the glory of God.

3. *The Physician in the practice of his art, is continually led up into the presence of God, which should of itself impart to his character a religious tone.* The realities of the eternal world perpetually cast their shadows upon his path; and making due allowance for the effect of familiarity in blunting the sensibilities, it is hard to see how he can wholly escape from the influence of the solemn scenes in which he is always an actor. He is compelled daily to witness the impotency of this world in securing the happiness and well-being of its votaries; how a single spasm of pain withers every enjoyment, and how complete a mockery all the advantages of wealth and position appear in the chamber of death. He cannot but see how feeble is the tenure by which the gifts of earthly fortune are held; and with what reluctance the hand uncloses to release the bounties upon which the grasp had been growing tighter in the season of prosperity. He hears the melancholy confessions which in that honest hour are extorted from hearts long estranged from God, and listens to the earnest testimony which is then borne to the transcendent importance of religion. That spell of fascination by which the world holds its victims,

while flaunting its gay visions before the eye, is effectually broken as the curtain is partially drawn aside which conceals the retributions of Eternity. If too, on the one hand, the vanity of this world is so painfully demonstrated, on the other, the reality and power of true religion are equally illustrated. It is often his privilege to hear the language of exulting hope from the lips of those who by nature are the most fearful and timid of our race; and he cannot but feel that the notes of triumph which die away in the last panting breath of the Christian swell out immediately in the music of that eternal Song which is heard forever in Heaven. Thus, to appropriate a striking metaphor of John Foster's in one of his inimitable letters, the Physician, mingling in these scenes of death, stands "at the high altar of Eternity to pronounce the inviolable oath;" he engages in "a kind of sacrament inconceivably solemn, at which he is summoned by the voice of Heaven to pledge himself in vows of irreversible decision." Alas! that these pregnant instructions should fall upon ears which heed them not! that he should turn away his feet from those hallowed shrines in the vestibule of Eternity, to chase, eagerly as before, the fleeting phantoms of earth! that he should treasure only with a romantic interest these reminiscences, which in fact are foreshadowings of the unknown future which he must so soon explore.

But there is another way by which the practitioner of Medicine is conducted before God: he is surrounded by mysteries in the prosecution of his art, which are always suggestive of the Deity. It is the fashion of many to cry out against mystery in religion, as though it was peculiar to that revelation of God which is made in the Sacred Scriptures. Intoxicated with the partial knowledge he has acquired, man grows insolent in his demands, desiring to be himself as God, knowing both good and evil. Yet he can enter upon no path of inquiry without finding all the resources of his reason baffled. "It is the glory of God to conceal a thing;" and the mysteries both of nature and of grace are but the veil which the Deity throws around Himself, to fill us with awe as we stand in His august presence, and to raise the anticipations of fuller disclosures in a better world. Perhaps, in no one of the learned professions are we more confronted with the supernatural, than in the Medical. Its literature is occupied with discussions revolving for ages around the same centres, whose heart has never yet been penetrated. What life is, which it is the aim of this science to conserve—where in the human body it holds its seat—how it should pervade with equal energy every portion of our frame

—why it should depend upon the union of matter with spirit, and disappear instantly when this connexion is severed—by what ties the soul is held in companionship with that body which it informs and actuates—what disease is, and how it should be originated—what are the laws of its propagation, and how transmitted from sire to son through succeeding generations—how it should undergo such modifications, as almost to lose its essential character, and enter into new combinations, baffling even the experience and skill of the most learned; all these questions need only be propounded to show the haze of mystery enveloping this noble art. However confident the man of science may be that all these matters are determined by certain fixed laws, yet the subtlety of these laws, escaping hitherto all research, requires the intervention of God by whom they are enacted and upheld. Should future scrutiny penetrate their working, it is only to resolve them into larger and more comprehensive rules; enthroning the Deity in the centre of an interior circle, before whom man must still bow in adoring worship.

As in the theory, so again in the practice of Medicine, we come continually in contact with God. To-day, a case presents itself in which from all that appears to view, the Physician would antecedently say, the man ought to recover; yet in spite of these hopeful prognostications and in defiance of all the skill and science which can be brought to bear, the patient dies. To-morrow, another case occurs in which from all the symptoms no other judgment can be formed but that the man must die; yet in the face of this prediction, and in opposition to all the aggravations of the disease, the crisis is happily passed and the patient survives. What thoughtful practitioner can fail to recognize in these daily recurring facts the existence of another Power which is at last the arbiter of life and death; and that upon the blessing of this invisible but superior being, his own success in the healing art depends? In whom then more than in the Physician should the devotional sentiment be elicited? and how naturally does true piety seem to crown the character of one, who, in every step of his career finds himself touching his Maker?

4. *The final argument urging the claims of religion upon Medical men, is drawn from the opportunities they enjoy of doing good to the souls of men.* Physicians more freely than other men move through all the grades of Society, mingling with high and low, the virtuous and the vile. In other pursuits men are more or less restricted to that circle in which Providence has cast their lot; or if they cross the artificial zones of society,

it is with a casual and distant intercourse. In this particular, Clergymen make the nearest approach to Physicians, in that they both occupy at once all the points in the social scale and are equally at home with all ranks. Yet the latter have preëminently the advantage in gaining access to multitudes, which however coveted, is often denied to the former. The vicious habits of many remove them from clerical influence by an interval over which it is difficult to pass. When broken down under their excesses, the man in black is precisely the last person whom they wish to see. His presence alone conveys a rebuke whose sting is felt as it awakens all the pangs of remorse; and his approach is too often considered as the death warrant which hastens the day of doom. None of these associations embarrass the Physician. His entrance into the chamber of sickness is welcomed as that of a friend and deliverer; and his mission is not regarded as one of rebuke, or even of admonition. But whilst soothing the pains of the sufferer, what a golden opportunity is offered of pointing the weary soul to the rest that is in God, and directing the penitent to that Saviour who offers pardon through His own blood. But these opportunities of usefulness draw along consequences that are momentous as the bar of judgment. Little as men may reck of it, they will be held to the strictest account for all the good they omitted to do; and no division of labor by which the duties of life are parcelled out can absolve any from the obligation of doing good to all men as they have opportunity. From the nature of his calling, the Physician is always touching wires that vibrate in Eternity; and woe be to him, who, reckless of spiritual things permits a soul to slip through his hands without a warning of the destiny to which it is hastening. I need not pause to show how all this infers the need of true piety in the Physician, who would meet faithfully all the trusts involved in his profession. To win a soul from death requires far more than the cold testimony of one who speaks only by report. It needs the heart of love and the tongue of fire in one who has himself "felt the powers of the world to come," to make a true evangelist to the soul struggling at the last moment with its everlasting fate; for which reason the Physician, who is often the only messenger of God to such a soul, should be a warm and zealous Christian.

In venturing these suggestions to gentlemen of the Medical profession, I trust I shall not be charged with presumption. It is seldom they are addressed as a class; and in a Pastor's ordinary ministrations it might appear alike obtrusive and invidious to single them out as objects either of criticism or counsel. But on this occasion when we meet to pay a just tribute of respect to the memory of one whom we equally honor, who was as conspicuous for his Christian virtues as for his professional attainments, it seemed proper to hold him forth as the type and model of his class. He was a beautiful exponent of all that I have attempted this day to discuss. What can I say more in his praise than that you "mark them which walk so as you have him for an ensample;" especially in this, that you equally exemplify what it is to be a *Pious Physician.* It is little for me to say that I profoundly honor your noble profession—a profession venerable for its great antiquity, ennobled by the genius which in every age has enriched its treasures, and surpassingly beautiful in the principles of charity and beneficence in which it is founded. Let it only be crowned with grace; let religion hallow it with its blessing; let true piety adorn those who pursue it; let its mission be one of salvation to the bodies and souls of men, and its rewards will be laid up to be enjoyed beyond the grave.

REMARKS BY PROFESSOR TROUSSEAU, OF PARIS, ON TRACHEOTOMY IN CROUP.

Translated from the French, by AUSTIN FLINT, M. D., Professor of Clin. Med. and Auscultation and Percussion, in the N. O. School of Medicine.

IN the recent discussion before the Academy of Medicine, at Paris, on the proposition of M. Bouchut, to practice *tubage of the glottis* in croup, the success of tracheotomy in this affection has been introduced. Professor Trousseau, as our readers are aware, is an ardent advocate of the latter operation, and has probably performed it oftener than any other practitioner. It is rarely employed in this country, and almost never in England. As to its propriety, opinions are divided. In view of this unsettled position of the operation, the arguments and facts presented by Professor Trousseau will, we think, be of interest to our readers, and we present a translation of so much of his late address on the tubage of the larynx before the Academy of Medicine, as relates to the subject of tracheotomy in croup. His address is in reply to remarks offered before the Academy by M. Malgaigne. The translation is from a report, contained in the *Gazette des Hôpitaux*, Dec. 2, 1858.

"M. Bouvier has already disposed of the reproaches which M. Malgaigne has addressed to me for having called tracheotomy an operation altogether French. M. Malgaigne is correct in supposing that I am familiar with the passage in which Van Swieten enumerates all those who have practised the operation before him. But *tracheotomy in croup* is, nevertheless, entirely French. It is not demonstrated that Martyn and Andrey resorted to it in that affection, and Caron peformed it in a strange manner. These are the only three operations, which are to be found before those of Bretonneau. Now, during 1858, there have been three hundred in Paris; and tracheotomy is French as much as the knowledge of the relations of rheumatism to cardiac affections, or as much as the knowledge of Bright's disease is English, although it had been previously showed that rheumatic patients were sometimes attacked with disease of the heart prior to the observations of Bouillaud, and that patients with disease of kidney had albuminous urine before the observations of Bright.

"M. Malgaigne assimilates tracheotomy to the great operations in surgery, and refuses to admit that in a large number of cases death may be the result of the disease and not of the operation. But if a surgeon amputate in a patient affected with pneumo-thorax, and the patient die of the pneumo-thorax, would M. Malgaigne place the death to the score of the operation, as if the operation had been performed on a subject whose general health was excellent? I confess I have erred in saying formerly that tracheotomy is devoid of danger. I admit that there are sometimes dangers from it. But let us see if they are as frightful as M. Malgaigne regards them.

"I do not insist on the fact that the greater part of insane persons who attempt suicide by cutting the throat, recover. I will ask special attention to a table prepared by a young German physician, showing the statistics of tracheotomy, as practised in cases of foreign bodies introduced into the air passages. Of ninety-six operations there were seventy-three cures, and the author adds that in a certain proportion of the unsuccessful cases, the operation was made contrary to well established rules of surgery, and in other instances the foreign bodies were lodged in inaccessible points, the patient succumbing to inflammation provoked by the presence of the foreign bodies. Among the twenty-three unsuccessful cases, there were then a good number in which the want of success should not be imputed to *the operation*. These facts demonstrate that tracheotomy is far from being so grave an operation as it is represented to be by M. Malgaigne.

"More recently, M. Robert has saved in my service, at Hotel Dieu, by tracheotomy, resorted to *in extremes*, a patient affected with pultaceous laryngitis, and another with œdema of the glottis. I have myself been called to a laborer who was suffocated. I followed the priest and found a man dead; I opened the trachea, and by artificial respiration resuscitation was accomplished. There existed an enormous diffused phlegmonous inflammation of the neck, which led to œdema of the glottis. The patient died, or rather *re-died*, the following night, but without any evidence of oppression, and the tracheotomy made, not *in extremis* but *port extrema*, wrought truly a miracle.

"But I hasten to come to the matter of croup. Is croup a disease which ends in cure, often, very often? This is the question. If the answer be in the affirmative, tracheotomy should rarely be performed. If there were a period in which croup is always curable, the operation should never be performed in that period; but if, on the other hand, it be always incurable in one of its phases, it then always claims the operation. Now, I find in the *Nosographie* of M. Boullaud, that of forty cases of croup treated exclusively by general measures, there were thirty-nine deaths. I have cited in my report the statistics of Dr. Ferrand, who enumerates, under the same circumstances, sixty deaths out of sixty cases; and I have also stated to you that in the single district of Sologne, I have myself seen, with M. Leblanc, fifteen patients out of seventeen succumb to the disease, the two which were saved being the only ones treated with topical meaaures.

"Croup is then a very serious disease. Let us see how it should be treated.

"I do not admit, with M. Malgaigne and Bourier, that croup exists only when there is a false membrane. The first stage, I hold, with MM. Bretonneau and Guersant, consists of an inflammation with tumefaction of the membrane, giving rise to a hoarse cough, croupal, but without paroxysms of suffocation. In this phase croup may be cured and tracheotomy is never indicated, although the cure may not be the rule. In the second period, characterized by false membrane, by cough, rare and extinct, by paroxysms of suffocation, with intervals of complete calm, the cures are much less frequent. Of this fact I call to witness the physicians of the *Hôpital des Enfans*, and all of my colleagues who have often occasion to observe croup in private practice. At length, when suffocation is constant, and the paroxysms are only execerbations, without any intervals of calm, how many children are cured? Ask M. Barthez, who has seen, of sixty-seven patients, sixty-seven die in this period! Is it not with a positiveness almost absolute, that we must say death is inevitable in this period? Do we not say this of phthisis when it has reached its third stage, notwithstanding that once in one thousand cases our prognosis may not be verified? In the third period of croup the certainty of a fatal result is as great as in phthisis, and if so, who will impute the result to tracheotomy performed in this period? Since death is imminent, inevitable, what can happen worse? And does not every one accord to tracheotomy the power of prolonging life for one or two days, and giving to the patient a possibility of being cured? All that is gained then, I insist, is due exclusively to tracheotomy.

"What are the conditions of the success of this operation?

"First of all, and before all else, the operation should be well performed. And our *internes!* and our *externes!* M. Malgaigne will say, *Mon Dieu.* It may happen to them to make mistakes, as it will happen to the most skilful surgeons. I have seen Auguste Bérard penetrate the œsophagus as well as the trachea, with the first stroke of the knife, and in another case lose his patient instantly from hemorrhage, but he was not prevented thereby from performing the operation afterward admirably.

"It is a prime point to avoid hemorrhage, which is always possible with a little patience. This you are aware of as well as I. You know, also, that there is an immense difference between cold croup, and diffused diphthéritis (diphtherite généralisée). Thus it appears from the theses sustained by the old internes of the Children's Hospital, that in the first cases thirteen of twenty-four were cured, and in the second; one of six.

"The preliminary treatment is also all important. Leeching, vesication, and repeated emetics place the patient in conditions most unfavorable, and it would be better to abstain from all treatment. On this subject M. Lefebre writes me that of seven children treated after this plan, on whom the operation was performed at the Children's Hospital, five still live; the sixth died of pneumonia a month and a half after the operation, and the remaining case did well to the fifth day when the patient passed from under observation.

"Although the melancholy statistics of M. Bouchut have been construed very rashly, it is not to be concealed that eminent surgeons have been very unfortunate in their operations of tracheotomy. We meet with collections of cases like the following:

"1. MM. Gosselin, Déguise, Huguier, Jayavay, Monod—Ninety-five operations and ninety-three deaths.

"2. MM. Alphonse, Guéria, Michon, Langier, Robert, Nélaton, Sevon, Depaul—One hundred and seventeen cases and one hundred and six deaths.

"3. MM. Velpeau, Jobert, Desormeaux—Eighty-four cases and sixty-eight deaths.

"But to these figures I will offer the following data which have been furnished to me by the physicians themselves to whom they belong:

	OPERATIONS.	CURES.
"MM. Bardinet and his confrères at Simoges..	57	17
Saussier (of Troyes)	6	3
Beylard	13	4
Moynier	17	8
Archambault	21	8
Lalois (of Belleville)	6	3
Nard (of Montbord)	2	1
Petel	2	1
Total	131	48

"These are not the only private practitioners who have registered equal success. There are a host of young surgeons, with whom M. Malgaigne has been brought into contact at the different *concours*, who have been equally successful. Prior to the adoption of the consecutive treatment now in common use, they had divided among them twenty operations and twenty deaths; but since they have followed the precautions which I have lately indicated, they have saved seventeen out of thirty-nine cases—more than one third. The following are the details of this last series of operations:

	OPERATIONS.	CURES.
"MM. Richet	9	5
Follin	7	2
Brocha	12	6
Richard	5	2
Demarquay	6	2
Total	39	17

"Shall we say, then, with M. Malgaigne, that the cures are due to inexperience? Indeed, gentlemen, the cures are due to experience, the experience of the surgeon aided by that of the physician, and the facts which I have cited should convince surgeons that surgery is but *medicine armed* and nothing more.

"What, then, are the precautions which, if they do not secure, will, at least, contribute to success? First of all, the choice of a canula with a calibre exceeding that of the glottis; next, the double canula, without which it is almost impossible to maintain the free passage of air; the employment of the cravat, and of a thick cravat, not a single gauze which does not prevent the formation within the trachea of crusts, which gives rise to the same obstruction as those of coryza in the nasal passagss; the maintenance of a warm temperature and of a suitable humidity; the cauterization of the wound, repeated every three or four days, without which it is covered with false membrane and becomes the point of departure for resorption and purulent infection, gangrene, diffused phlegmon, suppuration of the lymphatic glands—accidents which we observe so often in connection with blisters, and which belongs to croup, not to tracheotomy.

"I should add that even in the adult, and irrespective of croup, the wounds of the trachea are often the seat of a horible fœtor, or resorptions which give rise to general disorders extremely grave, and it has seemed to me that even in these cases cauterization renders complications less frequent.

"Finally, an extremely important matter is alimentation; and its value is such that it is proper to resort to force in its administration, and to employ the stomach tube in the cases, so frequent, in which croup induces paralysis of the palate and larynx.

"I affirm that, with these measures, tracheotomy will have, without doubt, intrinsic dangers, but that it will be attended by success in proportion as the precautions which have been indicated are appreciated and observed."

The foregoing remarks by Professor Trousseau were received by the Academy with applause.

MARKSVILLE, La., 28th December, 1858.

MESSRS EDITORS: In one of the late numbers of the London Lancet, I believe there is a small article written to illustrate the efficiency of nit. potass., when topically resorted to, in arresting erysipelatous inflammation, whether occurring idiopathically, or as the consequence of mechanical violence. I have never seen the article, my attention being first drawn to it through the suggestion of an intelligent confrère. Having had quite recently a very violent and aggravated form of erysipelatous inflammation to manage, I was resolved to give the remedy a fair trial, which happily resulted in complete success. The subject was an elderly lady, seventy years of age, who was attacked quite suddenly with as violent a form of erysipelas of the face, as ever fell to my lot to witness during a long course of practice. It was produced evidently from exposure to cold. When called to see her, I found the whole face very much inflamed, painful, and burning; several large and deep seated blisters had formed on both cheeks, the fluid from which was so acrid as to excoriate, in fact corrode the healthy surface over which it flowed, when these were opened. The patient I considered in extreme danger. There was low muttering delirium alternating with coma, which made me fear a metastasis to the brain, high irritatiev fever, with rapidly increasing prostration of the vital forces. The usual local, as well as general measures in such cases having been tried in vain, I ordered the whole face to be bathed, from time to time, with a saturated solution of the nitrate of potass. Strange to say, a complete abatement of the most painful and distressing symptoms was the immediate consequence of the first application.

The morbid action, diffused as it was, seemed to be suddenly arrested. No other treatment was pursued. Desquamation of the cuticle ensued. The whole surface is now dried and smooth, presenting the usual normal appearance. I offer no theory explanatory of the modus operandi of so simple a remedy in suddenly arresting a train of phenomena unquestionably indicating great and imminent danger. It may have acted as a local antiphlogistic, a local alterative, instituting its own in the place of a preëxisting pathological condition, and this is about as much as we can say in relation to the matter.

I am, very respectfully,

Yours, etc., G. E. ELMER, M. D.

VOCAL FREMITUS IN CASES OF SOLIDIFICATION OF LUNG, AND THE DELINEATION OF THE INTERLOBAR FISSURE ON THE CHEST BY PERCUSSION IN LOBAR PNEUMONITIS.

In the second number of the *Archives of Medicine*, edited by Dr. Beale, of London, Dr. George Johnson reports three cases of solidification of lung in which the vocal vibration, or fremitus, was less over the affected lung than on the opposite side. He reports these cases as exceptions to the rule that the vocal fremitus is increased by pulmonary solidification, and as proving that these exceptions occur oftener than is generally supposed. In two of the three cases, however, the solidification was on the left side. Now, the resonance in health over the right side so constantly exceeds that over the left side, and the normal disparity is often so marked, that these cases may only prove the continuance of the latter under the circumstances. The third case, moreover, was one of cancer of the lung, and the affected lung was found, after death, to contain twenty ounces of creamy liquid. It is hardly fair to affiliate a case of this kind with the solidification incident to pneumonitis and tuberculosis.

Dr. Johnson's cases, thus, do not seem to establish that exceptions to the rule of increased fremitus as a result of solidification, are frequent. But such exceptions do occur, and Dr. J. is not entitled to the credit of originality in indicating the fact. The correctness of this statement is shown by the following extract from '*Flint on the Respiratory Organs*,' published in 1856: "As a rule, the vocal fremitus is increased, in the second stage of this disease (pneumonitis), over the solidified lung. But the exceptions to this rule are not very infrequent. In some of the exceptional instances no disparity as respects this sign is appreciable on comparing the two sides of the chest. In other instances the fremitus is greater on the unaffected side. If the left lung be the seat of the disease, the explanation may be that the fremitus over the solidified lung in not increased, as naturally it is more marked on the right than on the left side. But I have observed the fremitus to be greater on the left side, when the pneumonitis was seated on the right lung. This shows that an effect of solidification, under certain circumstances, is a diminution of the natural fremitus. Instances, however, of this description are, probably, extremely rare."—p. 420.

Dr. Johnson, in the article referred to, thus speaks of another practical point relating to physical exploration: "There is one help toward a correct diagnosis (in pneumonitis) which has, I think, been too little regarded by those who have written on the subject. In percussing the chest, we should bear in mind the form and limits of the lobes of the lung, and ascertain whether the extent of dulness corresponds with the outline of one or the other of these lobes. If this be found to be the case, the probability is, that the dulness depends on solidified lung and not on liquid in the pleura." To bring this point before the profession he states was one of the objects of his communication.

Dr. Johnson is undoubtedly correct in saying that this has been too little regarded by "*those* who have written on the subject." It would have been more correct to have said that the point has not been regarded at all by writers, with the exception of the American work above mentioned. If Dr. J. has met with that work he has not only seen the point there indicated, repeatedly, but mentioned in the preface among the points supposed to be original.

[The above has been sent us by an esteemed correspondent, and we publish it with pleasure, as we think that honor should be done to whom honor is due.—Ed.]

SPECIAL SELECTIONS.

ON THE RADICAL CURE OF REDUCIBLE INGUINAL HERNIA.

Illustrated by nine successful cases. By Richard Jones (late of Guy's), Resident Surgeon in the Work house Hospital, Liverpool.

Among the modern innovations in surgery the new method for the radical cure of reducible oblique inguinal hernia claims a prominent position; and when we take into consideration the almost uniform success of this operation, it is surprising to find that it has not hitherto met a more general and encouraging recognition among British surgeons.

The relief of strangulated hernia is regarded as one of the brilliant triumphs of surgical skill over a certain approaching death, in which the surgeon can justly give himself the credit of having saved the life of his patient by his skill and professional knowledge. But an operation which can be performed without danger or much suffering, and which will prove an effectual security against the possibility of strangulation, also which relieves the patient of a great amount of inconvenience and deformity, is not inferior in its merits to the operation for strangulated hernia.

Notwithstanding nearly the universal success of the radical cure some object to its performance, and argue that it is better not to interfere with an old reducible inguinal hernia, except with palliative treatment; and indeed there would be some excuse for advocating this conservatism, if the operation was an experiment that might be attended with danger or place life in peril. The possibility ("not probability") of its causing serious symptoms is one of the speculative objections urged against it; but experience ("which is now sufficiently extensive to satisfy the most timid operator that there is no necessity for entertaining any anxiety or suspicion of danger if skilfully and judiciously performed") will furnish a satisfactory answer to this objection. If we compare the relative danger or safety attending a radical cure and the use of a truss, I think experience will lead us to the conclusion that the latter is much more liable to place life in danger than the former, which will serve as a prophylactic means against the possibility of eventual strangulation, to which old hernias are predisposed from thickening of the sac from the constant pressure of the truss. Mr. Spencer Wells, its principal advocate in England, uses the following eulogistic remarks in his excellent lecture upon radical cure of hernia: "But the surgeon who cures hernia radically with certainty and safety is a greater public benefactor than he who saves the life of his patient in strangulated hernia; as he not only relieves a large number of his fellow creatures from the suffering and inconvenience of wearing a truss, but he averts the danger of strangulation to which they are continually exposed in greater or less degree through every period of life."

The anxious solicitude which has been manifested with regard to the class of cases in which this operation may be judiciously done I think is quite unnecessary, and that most of the rules and restrictions laid down to guide us in our selection are of minor consequence; some of these contra-indications are decidedly disadvantageous, and might retard the progress of the case without frustrating the eventual success of the operation (*exempli gratia*), old age is an unfavorable condition, and will procrastinate the issue of the case for a few days; but *per se*, it is not a sufficient ground of objection, provided the constitution be tolerably healthy: and if we were asked, what is the most advanced age at which this operation may be safely and prudently done; we are hardly justified in giving a specific answer, inasmuch as it has been successfully performed at the comparative extreme age of sixty-four (and these cases are not few): nevertheless it has been suggested that unless some urgent or pressing symptoms are present it is more prudent not to interfere if the patient is over forty-five or fifty years old. This suggestion is practically untennable, as will be hereafter demonstrated; and age itself, without collateral contra-indications, should not influence our decision so much as the physical condition of the patient; because we often see persons of sixty affected with the decrepitude and infirmity generally present at seventy-five or eighty. The case, No. 9, aged sixty-four, is in a better condition, and posesses superior physical powers to No. 7, aged fifty. The five following successful cases, of the respective ages of sixty-four, fifty-nine, fifty-five, fifty, fifty, averaging fifty-five, will serve as an illustrative evidence in support of the above opinion. And this propitious fact in relation to radical cure is of considerable importance, because the beneficial effects of the operation is not limited to that period of life at which oblique inguinal hernia is proportionately uncommon.

Sometimes the size of the hernia is antagonistic to the success of the operation, for if the hernia is very small, the protrusion hardly perceptible, except on violent coughing, some difficulty will be met in effecting sufficient invagination on account of scarcity of integuments; if too much of scrotal integument be used for involution, the testicle on that side will be raised under the instrument, from the pressure of which it may become very painful and swollen, and this will sometimes cause considerable annoyance. In these cases the invagination should not be carried higher than is absolutely requisite for proper transfiction of the needle. In a case I saw with my friend, Dr. Banning, now of the Gateshead Dispensary (upon which we operated), the hernia was exceedingly small, and the scrotum contracted; it required careful and patient manipulation before the instrument could be adjusted without encroaching upon the testicle. On the contrary, hernia of extraordinary large size and of old standing, in which the normal relations of the rings and canal have been completely destroyed, make this operation impracticable; but fortunately these exceptionable cases are exceedingly rare. In cases of old standing hernias, in which the dimensions of the rings are increased and canals shortened from approximation of the inguinal openings, certain alterations will be requisite in the instrument to adapt it to the anatomical changes in the relations of these parts. Rothmond's instrument is specially constructed to meet these impediments, for its size may be changed according to the size and shape of the rings and canal: an occlusion of which must be performed with even pressure on every part. If the ring is relaxed and indistinct, and canal dilated and elastic, the double needle instrument (which consists of the central piece with two of the largest side pieces) is very well adapted to effect a complete obliteration of the canal. In case No. 5, of twenty-four years' standing, the double needle instrument was used, in which the herniary apertures were relaxed, and almost in juxtaposition; there is one objection to the use of the two needles, that if the punctures suppurate rather freely, the central portion may slough away. I had some apprehension that this would happen in this case once, and transform the punctures into one ulcer. In these somewhat unfavorable cases for radical cure, the instrument should be allowed to remain longer in the canal than in ordinary cases, to give sufficient time for strong adhesion to be formed; this precaution will be an additional security against the eventual retroversion of the sac. In case No. 5, already alluded to, I was going to remove the instrument on the eighth day, but found on slight traction that the adhesions were weak and imperfect; I readjusted the instrument, and left it for two days longer, when I could remove it with impunity. From these general remarks, we find that the mode of procedure must be modified according to the age and condition of the patient, also the anatomical changes at the seat of hernia, and that certain difficulties will be met in individual cases requiring a corresponding variation in the performance of the operation. Taking for granted that these minor obstructions already noticed are not insuperable (which has been proved by facts here recorded), the cases excluded as unfit for radical cure are exceedingly few indeed.

I will take the liberty of recapitulating the admirable description of performing this operation, given by Mr. Spencer Wells, in the 37th volume of the "Medico-Chirurgical Transactions." "The instrument is used in the following manner:—after the hair has been shaved off, the bladder and rectum emptied, the patient lies on his back with the thighs flexed and raised, and the operator stands or sits between them. The intestine is replaced if down. The Surgeon then places the point of his left forefinger upon the scrotum, about an inch below the abdominal ring on the affected side, and by carrying the finger with its palmar surface directed upwards and outwards through the ring into the canal, he pushes the yielding skin of the scrotum into the canal as deep as practicable; at all events, so far that the apex of the cone of the skin thus formed reaches the internal ring. The cylinder having been oiled, it is now introduced by the right hand, withdrawing the finger as the instrument enters. This is not always done without difficulty, and requires some practice. The invaginated skin may return as the finger is withdrawn, and require replacement; or, in cases where the ring is moderately narrow, the cylinder may not be easily introduced by the side of the finger. In this case the finger must be partially withdrawn to make room for the advance of the end of the instrument. Again, in old herniæ the cellular tissue about the ring is so lax, that the cylinder may be pushed up beneath the skin outside the canal. When this happens, the cylinder is found on examination to be much more moveable than when it is within the canal. When convinced that the cylinder properly fills the inguinal canal the needle is passed through the cylinder, the canal and the integuments; the wooden cover plate is placed over it, and pressed against the skin by the screw; the handle of the needle is then unscrewed, and the projecting point is covered by a small piece of cork."

Following their directions, the particulars of these cases are given in the tabular form suggested by Mr. Spencer Wells, in a note in the *Medical Times and Gazette,* in which he requested information as to the results of this operation.

RADICAL CURES OF HERNIA.

No. of Cases	Sex.	Age.	Variety.	Size of Ring and Canal.	Duration.	Date of Operation.	Immediate Effects.	Date of Removal of Instrument.	Result, etc.
1	Male.	22	Oblique inguinal hernia of the right side, about the size of two ordinary fists.	Ring small, and well defined; canal not much dilated.	3 months.	June 21..	The tenderness about the instruments necessitated a little relaxation of the pressure on the third day. The puncture suppurated rather freely.	June 29..	Six weeks from the date the operation was doing perfectly well; and three months from the date of the operation was passed into the army, perfectly free from any deformity or inconvenience.
2	Male.	22	Oblique hernia of the right side, of about the size of a goose-egg.	Ring large and relaxed; could introduce two fingers easily; canal shortened.	8 years...	June 23..	Free suppuratien from the puncture with a very small slough; no constitutional disturbance.	June 31..	Successful; he was also passed for the army 3 months from the date of the operation. A small quantity of discharge continued to ooze from the puncture for seven weeks.
3	Male.	59	Large oblique hernia of the right side.	Ring very large and loose; canal elastic and shortened.	12 months	June 30...	Very little ulceration of the puncture in this case (plated needle used) complained of tendernes for 2 days over the abdomen; "tongue and pulse healthy."	July 11 ..	Successful; does not suffer pain or any inconvenience now. He left the Hospital 3 months after the operation was done without any sign of relapse; but ordered to use a truss for six months.
4	Male.	55	Double oblique inguinal hernia of small size.	Ring and canal of normal size and shape.	9 months	July 9—right side; 19 left do.	Very little pain; the puncture healed up in five weeks.	July 19 & July 30	Perfectly well; can exert himself without a truss, or any other support. Nine weeks after the operation nothing perceptible but the cicatrices.
5	Male.	50	Large hernia on the right side.	Ring very much increased in size, and relaxed also; canal dilated and shortened.	24 years..	August 12	Testicle of the affected side very tender and painful; no other bad symptoms.	August 22	Complains of a slight pain in the cord & testicle of the affected side; otherwise perfectly well.
6	Male.	28	Oblique inguinal hernia of the left side, middling size.	Short canal & relaxed; ring large enough for the introduction of two fingers.	3 years..	August 13	No remarkable incidents besides slight abdominal pain occasionally; very small discharge from the puncture.	Aguust 20	Perfectly cured; is able to work without pain or annoyance; occasionally subject to neuralgic pain in the cord.
7	Male.	50	Inguinal hernia of the left side, of ordinary size.	Ring increase in dimensions, and boundaries loose.	9 months.	August 14	Small slough from this puncture; great deal of discharge issues from the inverted integuments.	August 24	Complained of some pain and soreness for seven weeks after the operation; now quite well.
8	Male.	18	Inguinal hernia of a very small size, of the left side.	Ring and canal very little modified.	14 years..	August 15	Had several epileptic fits on the following day; no other remarkable symptoms; this did not cause any serious consequence.	August 22	During a fit, on the second day after the instrument was removed, a little retroversion happened, but no herniary impulse perceptible on coughing.
9	Male.	64	Very small inguinal hernia of the right side, more like "bubonocele."	Ring normal: canal dilated.	6 months.	August 16	Great pain over abdomen; healthy tongue; much discharge from puncture, with small slough; more tumefaction about the instruments than in the other cases.	August 28	No sign of a relapse; feels quite well & comfortable, with the exception of neuralgic pain in the cord and testicle; but this improves daily.

Case 1, aged 22, was ruptured on the right side three months previous to his admission into this Hospital by a violent fall. At the time this accident happened he was serving in the militia, but was immediately discharged and ordered to use a truss, the application of which caused him great pain and annoyance, and entirely disabled him from following any occupation requiring physical exertion. Under these distressing circumstances he sought admission here, with the intention of getting his truss changed. A new truss was first tried, but he gained no advantage by the change, and he still remained in *statu quo.* The radical cure was proposed to him by Mr. Leather, as a *dernier ressort*, which he immediately sanctioned. This was a very promising case for radical cure, the ring being small and well defined; canal not much dilated, and strong; the size of the hernia equalled an ordinary fist.

He was ordered a dose of castor oil the previous night, and a glyster was administered in the morning, a few hours before the operation was done. Wützer's instrument was used; no difficulty was met in effecting the invagination or discriminating the boundaries of the ring. After the instrument was properly introduced, the tendon of the external oblique could be tangibly felt under the finger. The needle was next fixed, and the plate screwed over the duplicature of the scrotal integument. The after-treatment consisted in rest in the recumbent position, with fever diet. On the following day there was a little tenderness and redness about the instrument. He feels tolerably easy--slept many hours last night. On the third day a little discharge issues from the puncture and also from the plug. On the fifth day the puncture increased a little in dimension, with slight tumefaction about the instrument. No constitutional disturbance at all. On the eighth day the instrument was removed. The pressure was daily regulated. On the second day a little relaxation will be requisite from the swelling that generally follows; however, the feelings of the patient will serve as an index for modifying the pressure, the great object being to keep the opposed serous surface of the sac in mutual contact; and if too much pressure is exerted, the proximity of the puncture will be more likely to slough, because this is the spot upon which it is chiefly exerted; consequently the ultimate time required for granulation and cicatrization will be prolonged. After the instrument is withdrawn, the scrotum should be supported by a figure of T bandage for about fourteen or twenty days, and the puncture dressed with a little ointment, etc.

As a rule the patient should remain in bed for seventeen or eighteen days, to prevent the gravitation of the scrotum and testicle from breaking the adhesions before they are perfectly organized. It will also be advisable to recommend the patient to use a truss for two or three months, until the plug is perfectly secured in the canal.

Case 2 was also the subject of inguinal hernia of the right side, caused eight years previously by undergoing forcible bodily contortion; but he did not suffer much uneasiness from it up to twelve months ago, when it descended, considerably increased, with symptoms of strangulation. He was admitted to one of the Dublin hospitals, where it was reduced by taxis, and he was ultimately discharged with a truss; for four days after his dismissal the hernia descended in a similar manner, when he sought admission here. The hernia was reduced after long perseverance, and the radical cure was proposed, as the only alternative against the recurrence of the same misfortune.

This was not quite such a favorable case as the preceding, the rings were large and relaxed, and canal shortened and elastic.

The application of the instrument required great care, because the tube, without great caution, would slip above the margin of the ring, consequently render the operation abortive. Before the needle was transfixed, we satisfied ourselves that the tube was properly in the canal. I think this misapplication of the instrument has been the cause of failure in one case I saw in this Hospital.

The ultimate progress of the case was identical with the one already reported. Both those young men have been passed into the army since the operation was done.

Case 3, aged 59. This case presented more difficulties than those already noticed; he is an attenuated and delicate individual, always suffering under bronchitis. Twelve months ago, he was ruptured on the right side by a fall; he tried to wear a truss, but was finally compelled to leave it off on account of the pain it caused him: for the last three months he has been going about with his hernia hanging down, which incurred great deformity in his bodily appearance, also a great deal of dragging pain.

We thought this serious inconvenience which could not otherwise be relieved, justified trying the radical cure—his emaciated condition made the application of the instrument easy. The instrument used in the above cases would not suffice to fill the canal and ring, consequently we increased the diameter by covering it with gutta-percha. Contrary to our expectations the suppuration at the puncture was exceedingly small, but the discharge from the plug was more plentiful than in the other cases; the instrument was left in the canal for eleven days, for on the ninth we had some apprehension that the adhesions were not sufficiently secured; the upper plate was kept quite loose after the ninth day.

The after-treatment was similar to that adopted in the other cases. The discharge from the plug continued for eight weeks, and at first caused some excoriation over the scrotum, but this was soon cured by astringent lotion; he kept the bandage on for seven weeks, when it was replaced by a truss. There is not at present the slightest sign of a relapse; but he has not, however, been engaged in exerting himself much.

Case 4, a laboror, aged 55, was admitted into this Hospital four months ago, with double inguinal hernia, which was caused five months previously by a violent paroxysm of cough. For the first five months he was confined to his bed with acute bronchitis, consequently did not require any support for his hernia; but as soon as he was convalescent and able to walk about a little, his hernia descended and increased considerably in size: at first he applied a truss, and commenced to follow his ordinary occupation, but the pain consequent on the use of a truss compelled him to give up his work, and his truss was changed several times, but with no benefit. By seeing this serious inaptitude and disqualification for work, caused by using a truss, and no chance of improvement by another change, the radical cure was recommended to him as the only means of compensation, which was performed first on the right side, and ten days afterwards on the left: we thought the application of the instruments to both sides contemporaneously would cause a great deal of pain. This was upon the whole a favorable case, the relation of the ring and canal being not much disturbed, consequently rendered the operations easy; no incidents occurred in the ultimate progress of this case different from those already noticed: this man is now perfectly well, and can toil at his usual work without any pain or inconvenience; he is at present engaged in carrying heavy weights at one of the docks, and was exhibited at the Liverpool Medical Society, when the gentlemen present had an opportunity of making an examination, and satisfying themselves by ocular demonstration with the success of the operation.

Case 5, aged 50 years, porter, was admitted about three months ago with varicose ulcers on both legs. On admission his health was very much impaired from over-fatigue and want of proper nourishment: after his legs were improved, and his general health re-established, Mr. Leather proposed to have his hernia cured by Wützer's method. He was rather reluctant at first; but by seeing the success of the other cases, some presenting more unpromising *prima facie* evidence of success than his own, he was induced to give permission: he had been ruptured twenty-four years ago by lifting a heavy weight. In this case the rings were so loose and increased in size, and canal so widened, that the largest sized instrument would scarcely suffice to effect a complete occlusion. And although the double needle instrument was applied, only one needle was used, which was introduced through the central piece. The instrument was allowed to remain for ten days, the upper place being relaxed on the eighth day, also he was kept in bed for twenty-seven days with the T bandage—(in a case of this kind ample time should be allowed for perfect organization of the adhesions before weight is allowed to pend upon the plug). One of the immediate effects of the operation in this man was pain and swelling of the cord and testicle, which I think was entirely due to the pressure of the instrument. A slight neuralgic pain continued for about two months to annoy him occasionally, but at present he is able to work with ease, and is free from any pain; he was ordered to use a truss for four or five months; perhaps it is not absolutely necessary to wear the truss quite so long, but these precautionary measures should be adopted in proportion to the probability of retroversion. And in a case like this, in which the changes in the relations of the parts at the seat of hernia particularly predispose to a relapse, no weight should be allowed to gravitate on the plug until the interior is completely obliterated by granulations.

Case 6, aged 28 years, baker, who is a delicate and sickly looking individual, with a debilitated constitution, and generally troubled with some pulmonary affection, was ruptured three years ago by a heavy fall. Immediately after this accident he went to one of the Glasgow hospitals, where a truss was applied, but in this case, as in nearly all those already mentioned, the employment of the truss was accompanied with so much pain and uneasiness while engaged at any occupation necessitating little physical exertion, that he was often obliged to relax from his work (which was comparatively light) for weeks together. He lingered in this way for nearly three years, but had been able to use a truss with more comfort latterly; yet he felt exceedingly dissatisfied that this important inapitude for work could not be ameliorated by some means.

The anatomical condition of the ring and canal was not much changed. Canal little dilated and shortened, and rings large enough for the introduction of two fingers. The condition of his general health was not so propitious as in the other cases; but in an operation incurring so little constitutional disturbance as this, we did not think it requisite to be very anxious about the state of the constitution (providing no serious organic disease exists.)

Rothmond's instrument was used with two middle-size side pieces, and left in the ring for seven days; this abbreviation in the time from ten to seven days was made on account of some abdominal pains, extending up to the umbilicus, which proved to be nothing more than extension of the superficial pain about the instrument, and this was evinced by the superficial redness and tenderness, also by the absence of any positive sign of peritoneal inflammation, and immediate abatement of the pain on removing the instrument. The puncture suppurated somewhat freely; with these exceptions the case progressed as favorably as any of the others; and he is now engaged in very active exercise for about ten hours daily. And he does not feel the slightest pain in the side opperated on, even while engaged in heavy work.

Case 7, aged 50 years, railway porter, was ruptured nine years ago, while engaged in some active exercise at the station. He tried to use a truss, but with so much pain and inconvenience that he was compelled to give up his situation. He was admitted four months ago with hernia of the right side, being at that time about the size of a goose egg. The rings and canal were not much changed in size or shape; but the boundaries of the ring felt loose and relaxed. The operation was performed on August 14. Rothmond's instrument was used, of the middle size, and a steel needle was introduced instead of the plated one. Among immediate effects of the operation was the separation of a small slough from the puncture, and a more copious discharge from the plug than in the others. I think the steel needle is more apt to cause suppuration and sloughing than the plated one, which resists the action of the discharge. The scrotal integument had been covered with superficial eruption a few weeks previous to his admission, and, perhaps, this will account for the increase of discharge from the interior of the plug, which was obliterated sooner by granulations than in the other cases. He complained of pain and soreness about the plug and scrotum for seven weeks after the operation was done; this has now entirely disappeared, and he feels perfectly well and capable of performing his previous duties with ease.

Case 8, aged 18 years, suffering under congenital epilepsy, was ruptured fourteen years ago by a fall. His mental faculties are considerably impaired, and he is not able to give any account of himself previous to his admission. The rupture is small in size, the ring and canal very little modified. The operation was performed on the 15th August; on the following day had several epileptic seizures, and during a paroxysm he rolled about and so loosened the instrument that its re-application was necessary; also he had several fits on the subsequent days; however, the instrument was not much disturbed. On the seventh day the instrument was removed, and a little retroversion happened during a fit; but no herniary impulse is perceptible on cough.

There is some doubt about the ultimate success of this case, because the plug is somewhat loosened in the canal. At present about ten weeks has elapsed, and no more retroversion has taken place. If the patient had not been subject to epilepsy, the issue of this case would have been more satisfactory. And providing that no relapse will happen during an epileptic seizure, before another month is over, I think it exceedingly probable that the operation will prove effectual. Should this case turn out a complete failure in future, the fault cannot be attributed to the radical cure, but to other extrinsic causes, which bear no relation to the operation. I think it unjust to bring forward such case of relapse, as this might be, as an objection to Wützer's procedure. Yet I am disposed to think that some who have objected to the operation have, unintentionally, based their argument upon this unjust ground, and bring the total numbers of failures, irrespective of causes, as *bona fide* cases of retroversion.

Case 9, aged 64 years, potter. He is an exceedingly intemperate old man, nevertheless possesses very good physical powers, and generally enjoys good health, notwithstanding his profligate habits. His general aspect would lead us to think that he is eight or ten years junior to his real age. He was ruptured six months ago by fighting, when in a state of intoxication. The first remedy was a truss, but this gave him so much pain, that he was forced to give up his ordinary migrations about the country entirely. He came here almost *in forma pauperis*, unable to gain his living on account of his hernia. On admission he had no intention of undergoing any operation, but the advantage of the radical cure was represented to him, to which he willingly conceded. The hernia was small; ring normal size, and canal dilated. The operation was performed on August 16th; and among other trifling immediate effects, was pain over the lower part of the abdomen for the first two days, without constitutional disturbance, or any positive symptom of inflammation; tongue and pulse being healthy, it would be a great stretch of imagination to refer this anomalous pain to peritoneal inflammation, nor could such a supposition be substantiated in the absence of essential signs of peritonitis.

Also the puncture suppurated very freely in this case, from the circumference of which a small slough separated; but this vexatious effect was soon repaired by granulations. The instrument was removed on the 28th inst., the exudation from the interior of the invagination did not commence until the sixth day, and was not so copious as in the others; if the discharge is very scarce from want of sufficient inflammation, the introduction of a little "empl. lyttæ" inside the plug will assist the other causes in producing more inflammation. In this man, the epidermis covering the interior of the plug was morbidly thick, consequently more able to counteract the irritating action of the discharge issuing from the puncture and the presence of the cylinder; the man is at present quite well, with the exception of neuralgic pain occasionally in the cord and testicle.

I will now review in a cursory manner the principle reasons which have been alleged against the radical cure and will endeavor to show that most of them are based upon hypothetical notions unsupported by evidence even tantamounting to probability. Among the chief arguments brought forward against the operation is the *probability* of peritoneal inflammation supervening. Without absolutely denying the *possibility* of this taking place, I will unhesitatingly assert, after a careful investigation into the clinical history of sixteen cases, that it is exceedingly improbable. I have not seen the slightest indication of peritoneal inflammation in a single case. The little external redness and tenderness is merely an extension of the pain caused by the pressure exerted upon the duplicature of the integuments by the instruments, and this has been regarded with unnecessary apprehension in some cases, for I have myself seen a case in which it was proposed to prescribe active measures when all positive signs of peritoneal inflammation were absent. I think that there is not one well authenticated case of peritonitis on record out of sixty-four cases that have been reported. I believe, certainly there were some doubts on one, or even two instances; but even granting that these were genuine cases of peritonitis, I think we are justified in referring such a small proportion to mere chance or coincidence; consequently, if we appeal to statistics, it will furnish a most convincing testimony in contradiction to the apocryphal supposition, that peritonitis might follow.

And out of sixty-nine cases, neither death nor any dangerous symptom followed in a single instance; and upon careful reflection of the *modus operandi* of the radical cure, we should not be prepared to anticipate the extension of the peritoneal inflammation, because the exciting cause is limited to that spot when the serous membrane is kept in mutual contact by the instrument; consequently the immediate results of inflammatory exudation is the fibrillation of lymph, which will unite the opposing serous surfaces, and tend to prevent the diffusion of the inflammation. In conclusion, I will state that it is exceedingly problematical, whether a single case of peritonitis has hitherto happened, depending solely upon the operation; the one or two cases that have been mentioned as claiming some pretentions to depend upon the operation are enveloped in great obscurity, and we are not justified in regarding even these two cases as traumatic peritonitis, because two of the patients were in a debilitated and unhealthy condition at the time, and this circumstance is a strong presumptive evidence negatory to the supposition that the operation was the sole cause of the peritonitis; it might possibly assist and accelerate other predisposing causes.

The next cardinal point which must be taken into consideration is the probability of relapse. This has been urged with great stress as a serious adversity to the future generalization of the operation. I think this objection has been greatly magnified, as will be found by referring to statistical reports. And most probably in some of those unsuccessful cases recorded, the fault rested with the operator and not with the operation, for it requires a little tact and experience to perform this operation successfully. In one case of relapse that happened in this Hospital, fault was traced to the manner the instrument was introduced, which had slipped above the margin of the ring. I am disposed to think that some other cases of relapse depend upon similar causes, or the relapse might be occasioned by over officiousness on the part of the Surgeon in disturbing the instrument too often, consequently breaking the adhesions as soon as they are organized. It was proposed once to remove the instrument on the fourth day and cover with ointment; this is a very pernicious practice, and will very likely defeat the operation by rupturing the adhesions, which are at this period very weak. Nor is it productive of any benefit to remove the instrument as soon as the serous discharge issues from the interior of the plug, for in nearly all cases the serous effusion will happen before the fifth day, as the cases are exceedingly rare in which the instrument may be withdrawn at this early period with impunity. Nor should I feel disposed to risk this in any case whatever. Under certain conditions, vesication is excited in the interior of the plug by the third or fourth day. The duration of time that will intervene before serous effusion happens will be proportionate to the delicacy of the patient's skin, and this will range from three to seven days, and should in no way serve as an index for the time the instrument should be allowed undisturbed. Taking all these incidental causes of relapse into consideration we are forced to admit that the number which exclusively depends upon other irremediable causes are very few indeed, nor have I myself seen a single case which could not be traced to some accidental cause, which might be averted by foresight and care. I am disposed to believe that the majority of cases which have not succeeded can be referred either to misapplication of the instrument or removing it before the adhesion was sufficiently strong. Consequently, if we were able to subtract these from the total number of failures, the remainder would be proportionately a very small fraction; perhaps not one in fifty. This point, which has been so earnestly expatiated upon as an insuperable objection, will be satisfactorily answered by referring to statistics, which show that the unsuccessful cases, including those which depend upon accidental causes, are only three in sixteen, "and these cases were purely due to remediable causes." It was mentioned during the discussion on this subject at the Liverpool Medical Society, that it is rather premature to pronounce the cases successful three or four months after the operation, and that a relapse might occur after we have lost sight of the patient. In the first place, this argument is a mere hypothesis, destitute of any evidence, and comes in direct collision with facts already registered; for I do not know of a single case of relapse after the expiration of the third month, for the probability of relapse will be in an inverse ratio to the time that intervenes after the removal of the instrument; after three or four months the plug becomes so organized and identified with the adjacent tissue that a relapse is almost impossible at this advanced period.

The two chief objections have been fairly dispensed with, and in both instances statistical reports will serve as the best *onus probandi.* Also it behoves us, in an operation not demanded by urgent and pressing danger, to consider the amouut of suffering the operation causes to the patient; this is of such a trivial consequence in the radical cure that it is hardly worthy of consideration—there is scarcely an operation in Surgery accompanied with less suffering and danger. The radical cure has been regarded as officious Surgery by some who have not taken the trouble to test its merits; they argue that it is not intended to answer any other purpose than a mere substitute for a truss, or a surgical experiment to gratify curiosity; this is both *suppressio veri* and *suggestio falsi,* for it is intended to avert peril that may happen at any moment, also release the patient of pain and annoyance that will always be an important hindrance to his aptitude for physical exertion and social enjoyment; and the result of an extensive experience will serve as a proof that the operation is not a surgical experiment that might or might not answer the intended purpose; and it is no presumption to assert, that if the operation is skillfully performed under favorable conditions, and the case properly watched, that the issue will to a moral certainty be successful. It will seem surprising that an operation possessing so many superior advantages has not become more general in its application. I have even heard this and other similar remarks mentioned as reflecting suspicion upon the claims of the radical cure. I cannot help regarding such a subtle objection as an ungenerous wish of opposition, for it seems almost incredible that any one can conscientiously indulge in such an extreme scepticism, and no one would condescend to answer such scrupulous and sophistical course of reasoning if applied to other surgical operations legitimately acknowledged by the profession, nor would it create doubt or uncertainty respecting their advantages or impute a slur upon reputation founded upon the intrinsic merits of the operation.

Mr. Higginson proposed a simplification of the instrument at the Liverpoal Medical Society, which I think would be a great improvement. The central piece of the "Rothmond" instrument is composed of solid metal, which increases the weight considerably, and this causes unnecessary pain and inconvenience to the patient. This was the case with a patient at the West Derby Hospital, upon whom Mr. Edward Lister had performed the operation a few days previously. He was constantly complaining of a dragging pain caused by the instrument; also, it pressed upon the testicle, which was drawn high up. Besides this, there is another evil consequent upon using a heavy instrument, that it increases the tendency in the plug to descend in the canal, consequently incur more weight upon the needle. Wützer's instrument is decidedly superior in this respect, for the cylinder is a simple piece of round wood, with a hole in the interior for the needle. The only imperfection in this instrument is the impossibility of modifying its size according to the size of the canal and rings. Mr. Higginson's suggestion to have the middle piece composed of wood, would remedy the inconvenience caused by the greater weight.

The size of the needle with "Rothmond's" instrument is about an inch too short; for in the case already mentioned at the West Derby Hospital, the needles were too short to penetrate through the integument, except with a good deal of pressure; and we were obliged to get new needles made an inch and a half longer. I have seen the same thing in other cases in this Hospital, which caused a good deal of annoyance. And even if the needle has fairly penetrated the integuments, if the piece which is through is very short, it will in all probability slip from the opening in the upper plate when the parts begin to swell a little; and this will necessitate the re-adaptation of the instrument (a).

I think the adoption of these suggestions in the construction of the instrument will materially facilitate the operation.

I must plead indulgence for numerous omissions and defects, which I have not been able to correct from want of time; and I hope that the intense interest and novelty of the subject will compensate to a certain extent for these deficiencies and imperfections.

I hope the result of these cases will induce Surgeons to take the superior advantages of this operation into their serious consideration. It has hitherto remained in comparative seclusion; but will, I trust, soon be recognized as one of the most legitimate and successful of Surgical operations.

EDITORIAL AND MISCELLANEOUS.

The End of Volume V.—This number closes Volume Five of the New Orleans Medical News and Hospital Gazette. The writer of this was not in at the birth of the bantling, but he always felt the warmest interest in its welfare, and since circumstances have thrown it under his charge, either wholly or in part, he has used his best efforts to sustain it. Believing that the government of medicine should be republican in its form; that medical men and medical journals should be independent in thought and action (always thinking and acting for the welfare of our noble calling), our journal has never debased itself by pandering to public prejudice or sentiment; on the contrary, it has ever ennuciated its ideas of right and wrong in terms unmistakable, willing to incur the displeasure of the one-idea'd few who think it impolitic to say what we think. As the world wags, it is, doubtless, impolitic to speak the promptings of the heart on all occasions; but truth is to science what water is to the tender plant. But for the showers and dews of Heaven, the plant would wither under the influence of a burning sun; withhold truth, and science droops under the united power of ignorance and prejudice. Our conception of the mission of an editor of a medical journal may be wrong, but we are conscientious about the matter, and while we wield an editorial pen, we expect to do as we have already done. The medical press is, without doubt, the medium for interchange of professional opinions and experiences; but the medical profession is made up of something more than medical opinions and experiences; it is first composed of men, who are prone to err, morally and intellectually, and when a moral or an intellectual wrong is found floating by us, we believe it but our plainest duty to catch and hold it up to public gaze for the general good. We have no petty jealousies, no miserable malevolence to subserve; it is the matter, not the man, we point to, and we always point directly. How, but through the medical press, can the practising physician learn that books or pages are or are not worth the money asked for them, or that the opinions of men remote from him are good ar bad, or that lurking enemies to our best interests, in the shape of manufacturing pharmaceutists, etc., etc., are attacking our outposts? Who, but the editor, is thrown into such direct contact with the medical world, as to enable him to see all that is going on? The editor is the sentinel, and if he sleeps at his post, the great republic of medicine falls. The editorial chair is not an enviable one, but it has to be filled; the drudgeries of life, throughout all the grades of society, have to be accomplished by men, and inevitable circumstances throw the duties on us. We must either shrink from them entirely, or we must endeavor to do them well.

A retrospective glance shows us, that although we have done our duty to our patrons, there is room for improvement; but the same glance reveals the unpardonable negligence, on the part of many of those for whom we daily labor, of the plainest duty they owe us and their professional brethren, and this negligence fetters our ambition to issue the largest and best medical monthly in the land. We receive all our domestic journals in exchange, and we honestly believe that we furnish as good a number every month as can be found; but we desire to do more, and this we would do if our delinquent subscribers would pay their just, their honest dues, and thus defray the necessary expenses. We have many true patrons on our list—men who seem not to feel happy until they have squared their account with us every year; but we have a list again who hang about us like the mistletoe on the stalwart oak—they furnish nothing to our support, but hourly suck from us all that we derive from our honest friends. Go with us, reader, to the post-office this morning, and see what we find in our box. A letter from Dr. B. paying five dollars in advance for volume six (not yet commenced), and a letter from the post-master at M. saying that Dr. K. declines taking our journal from the office any longer. We return to our office and look at the ledger. Dr. B. is all right: we credit him with five dollars for volume six, and mark his letter and put it in pigeon hole No. 1, among "letters with money." We turn over a few leaves in the ledger, and there stands Dr. K.'s account, all on the debtor's side, owing us, for volume two, three dollars; volume three, five dollars; volume four, five dollars; volume five, five dollars: in all eighteen dollars, with interest, bringing the sum total (if men would thus honestly view things) to the snug little sum of twenty-one dollars and thirty-six cents. We are both anxious and indignant, however, and we write Dr. K. forthwith a very polite note, informing him that we are very willing to discontinue the journal, if he will only pay up the eighteen dollars—never once mentioning the interest.

Of course we get no answer to our note; on the contrary, no doubt he thinks us very impertinent; we strike his name from our list, and set over the eighteen dollars, with interest, to profit and loss--more especially the *loss*.

Reader, this is a weekly occurrence, and thus it is that there are this day several thousand dollars of debts justly due us, yet ignored by men whom we are obliged to call our professional *brethren*. But, says the philosopher, "you should strike every such name from your list and only furnish a journal to the honest." And so we do, *after* we find that there is no morality in him; but when we strike him off, away goes all the money he owes us—in other words, then he is *sure* not to pay us We are every day purging our list, but in the mean time, we are purged some too. Money that we hoped to get is gone forever. The fact is, we are going to make a fair proposition to these old delinquents. Gentlemen, send us now two-thirds of what you owe us, and we will give you the balance as a reward for your liberality! If we can only get two-thirds of our money, we will begin the work of enlargement and improvement forthwith. Surely, in these flourishing times, no man who has a spark of honesty or ambition about him will hesitate to buy up his own just debts at sixty-six and two-thirds of a cent in the dollar.

To all those who have honestly paid us their dues, and thus assisted us in our efforts to do good, we here offer our sincere acknowledgements, and we pledge our unceasing efforts to please them. For those who stand on our black list, we have no thanks, nor do we labor for them; on the contrary, we have only to say to them, pay your just debts and redeem yourselves, or repudiate like men and authorize us to say so for you in our pages.

On the first day of March next we shall begin a new volume, and we shall be pleased to know the basis on which we are to stand. All who desire to quit us, will please notify us before that time, but in withdrawing from the subscription list, please do not forget to square your accounts.

ANNUAL REPORT OE THE BOARD OF HEALTH OF THE STATE OF LOUISIANA.—Being unable, at this date, to procure a copy of this Report, we are obliged to clip from one of our daily papers the following interesting résumé of matters and things. When we get hold of a copy, we shall be able to make some remarks of our own:

"The annual report of the Board of Health to the Legislature is a voluminous document, filling, with accompanying tables, some eighty or ninety pages. Precluded by its length from reproducing it in our columns, we shall merely give a condensed summary of its more important statistics, taking occasion to recommend the perusal of the entire report to members of the medical profession and others feeling a direct interest in the sanitary condition of our city and State.

"The balance of funds of the Board remaining on hand, December 31st, 1857, was $3,774, which, added to the receipts from various sources, make the total resources of the Board for the past year $39,734. Of this sum $5,960 remain in the treasury. There has been paid for salaries of various officers $12,486, of which sum $7,767 was paid to the officers at the Mississippi station, and $4,720 to the officers stationed in this city. The Board have paid $5,380 for land, $3,646 to the employees at the Mississippi station, $2,304 for expenses of the Atchafalaya station, $1,712 for the expenses of the Riggolets station, $1,300 for rent, $1,000 for attorney's fees, $1,423 for groceries, $478 for printing, advertising and newspapers, $300 for insurance, $293 for hardware, $400 to Isaac E. Morse, $169 for stationery, and the remainder of the funds, except balance as above and $1,382 of uncollected drafts, for various smaller items of expenditure.

"The total number of deaths in the city of New Orleans for the year ending December 31, 1858, was 11,710, distributed thus: January, 448; February, 362; March, 518; April, 482; May, 576; June, 531; July, 813; August, 1,826; September, 3,069; October, 1,775; November, 834; December, 476.

"The ages of the deceased were as follows: Under one year, 1,949; from one to two years, 603; from two to five, 1,001; from five to ten, 365; from ten to fifteen, 293; from fifteen to twenty, 576; from twenty to twenty-five, 1,539; from twenty-five to thirty, 1,486; from thirty to forty, 2,001; from forty to fifty, 1,040; from fifty to sixty, 463; from sixty to seventy, 216; from seventy to eighty, 118; from eighty to ninety, 45; from ninety to one hundred, 11; over one hundred, 6.

"Of the deceased, 5,624 were natives of the United States, 1,891 of Ireland, 1,411 of Germany, 677 of France, 214 of England, 114 of Prussia, 66 of Spain, 76 of Switzerland, 50 of Scotland, and less than fifty of any other country. The nativity of 1,278 is not stated.

"Of the deceased 766 were negroes and 352 mulattoes. The slaves numbered 645, and the free negroes 475.

"In sex, 7,675 were males, and 4,035 females. The adults numbered 7,183, the children 4,557.

"Dr. H. D. Baldwin, Secretary of the Board, gives the following synopsis of the more important diseases causing death as compared with the report for the year 1857:

"*Accidents*—52 this year, against 32 last year; this includes falls, railroad accidents, etc., and comes under the head of casualties.

"*Apoplexy*—114 this year, against 99 last year; the smallest number of deaths occurred in April, viz.: 5; 13 each in July and August.

"*Bronchitis*—74 this year, against 55 last year; the greatest number of deaths being in January and February.

"*Cholera*—26 deaths from this disease this year, 24 last year.

"*Cholera Infantum*—108 this year, against 91 last year; 29 deaths in June, the most in any one month.

"*Congestion of Brain*—156 in 1858, against 92 in 1857; greatest number, 28, in September.

"*Consumption*—729, against 624 in 1857; great uniformity in the deaths each month—in 1856 there were 652 deaths of this disease.

"*Infantile Convulsions*—521, against 375 last year.

"*Delirium Tremens*—The deaths reported this year are 85; last year, 58.

"*Diarrhœa*—297 deaths in 1858, against 224 in 1857; 35 deaths in September, the greatest number.

"*Found Drowned*—86 in 1858, 67 in 1857, 13 in April, the greatest number.

"*Dysentery*—229, against 200 last year.

"*Enteritis*—117, against 106 last year.

"*Epilepsy*—20, against 21 last year.

"*Congestive Fever*—328 reported this year, 72 last year: under this head are included all deaths reported as pernicious fever.

"*Typhoid Fever*—189 this year, 118 last year.

"*Yellow Fever*—845 the number of deaths reported; ten interments are to be added, one of the Hebrew cemeteries having failed to report at the proper time—making 4,855; greatest number of deaths in September, being 2,204.

"*Gastro Enteritis*—87 deaths this year, 71 last year.

"*Heart Disease*—87 in 1858, 71 in 1857.

"*Inflammation of Liver*—65 this year, against 32 last year.

"*Inflammation of Lungs*—231, against 175; greatest number of deaths in December, viz.: 38.

"*Inflammation of Throat*—95 this year, 5 last year.

"*Intemperance*—44, against 26 in 1857; 11 deaths in July, greatest number.

"*Stillborn*—338 this year, in 1857, 358—20 less this year.

"*Teething*—189, in 1857, 151; greatest number of deaths in June, being 31.

"*Tris. Nascentium*—222, against 188 in 1857.

"There were 749 deaths from the three last named causes, during the year, being about 30 per cent. of all the deaths under one year.

"The deaths from yellow fever were 2 in June, 132 in July, 1,140 in August, 2,204 in September, 1,137 in October, and 224 in November.

"The deaths from consumption were distributed thus: January, 58; February, 43; March, 72; April, 78; May, 74; June, 46; July, 78; August, 54; September, 73; October, 54; November, 80; December, 69—Total 779.

"Infantile diseases show a formidable record of mortality. The number stillborn was 338; premature birth, 50; infantile marasmus destroyed, 188; hooping cough, 28; scarlet fever, 79; infantile debility, 75; infantile convulsions, 521; croup, 126; cholera infantum, 108.

"Dr. A. F. Axson, President of the Board, prefaces the tabular portion of the Report with a historical sketch of epidemic yellow fever in New Orleans, accompanied by suggestions relative to Quarantine, cleansing streets, drainage, and all that effects the health of the city. His remarks are characterized by his usual learning and research, but while too extended for our available space, any synopsis we might attempt would fail to do them justice."

"THOUGHTS ON "MANUFACTURING PHARMACY," IN ITS BEARING ON THE PRACTICE OF PHARMACY, AND THE CHARACTER AND QUALIFICATIONS OF PHARMACEUTISTS. *By William Procter, Jr.*—It has been frequently a subject of serious reflection with the writer, what influence the modern custom of manufacturing galenical preparations on a large scale has had, and will increasingly have, on the Pharmacy and Pharmaceutists of the United States. It is well known that vegetable extracts, fluid extracts, pills of all the officinal formulæ, plasters, cerates, compound powders, in fact nearly all the empirical organic preparations of any note, are now made by various large manufacturing establishments. The formulæ employed by these makers do not agree with each other, nor are they generally in accordance with the Pharmacopœia, either as regards medicinal strength or manipulation. With variable degrees of conscientiousness as regards the efficiency of preparations; and with varying knowledge and skill to effect the manipulations required, as must be expected; yet it is as rivals for a market, and in the competition which follows, that the most serious evils arise or may be anticipated. These gentlemen, like other manufacturers, take all the means considered usual or honorable by business men in creating a market. They do not wait until the slow and deliberate footsteps of the therapeutist and his apothecary have developed the value of remedies, and created a demand for them; but instantly on their announcement in the journals, and often before their permanence is demonstrated, quantities of preparations are disseminated over the country and vended by parties unacquainted with their merits, or kept in store by druggists as agents, until the process of deterioration renders them more or less worthless. There are many preparations which may be advantageously made on a large scale, or which, owing to the nature of the material or apparatus used, may be better made in one locality than another; but such are the exception, not the rule. It certainly should be the pride, as it is the duty of the Pharmaceutist, to make for himself all those important, though often not permanent, preparations which are termed galenical, as well as many chemical substances; yet there is less necessity for his preparation of the latter, because they are generally so uniform in composition, and are so easily tested, that he has the power of protecting himself from imposition. Not so with the former. How easily may a worthles extract be palmed off on the apothecary, and how utterly impossible, from known data, often is it, for a tincture or fluid extract to be detected when of deficient strength, or when prepared from drugs of inferior quality.

"Again, the influence of competition on quality may be illustrated by the two extracts, Krameria and Compound Colocynth. Apply to almost any druggist for the officinal Extract of Krameria, and you will be disappointed. Why? "Because," you are told, "the root yields so little extract to cold water that it won't pay to make it by the Pharmacopœia!"

Go into the market, and you will find compound extract of colocynth varying from seventy-five cents to one dollar and fifty cents a pound, when the scammony in a pound of the extract costs nearly two dollars and fifty cents, if of the best quality. Now how can a conscientious apothecary dispense a prescription of such materials? And yet it is often done by those who know better. But it is not necessary to enlarge further in this direction. Let us consider the effects of this policy of purchasing preparations on pharmacy and pharmaceutists.

"Pharmacy may be defined to be the art of preparing and dispensing medicines, and embodies the knowledge and skill requisite to carry them out in practice. But if the preparation of medicines is taken from the apothecary, and he becomes merely the dispenser of them, his business is shorn of half its dignity and importance, and he relapses into a simple shopkeeper. How can an apothecary properly educate his apprentices, unless he affords to them an opportunity to make the medicines they dispense? He cannot do it; and hence this policy has the gradual but certain effect to depreciate the standard of pharmacy and the status of its practitioners. Young men who enter such establishments scout the idea of its requiring four years to learn the business; and after a two years' experience in vending preparations and compounding prescriptions, are qualified clerks! at least in their own estimation. Of what avails is it to urge on such young men the advantages of chemical and pharmaceutical studies, or of graduating at a pharmaceutical institution, when they see that practical pharmacy means the retailing of drugs and preparations furnished ready for use by the druggist and "manufacturing pharmaceutist?" What is the object of our Association but to elevate the standard of practice, to give uniformity and value to the officinal preparations, to infuse so much of science and accuracy into our art that it shall cease to be a trade only, and, above all, in the language of the original preamble to our constitution, to "elevate the professional character of apothecaries and druggists throughout the United States," by making them what they ought to be, masters of their art, in the sense of understanding its theory as well as its practice? In Germany and Poland the apothecary is required by law to make most of the preparations he sells, and thus is accountable for their quality. If it were so here, our numbers would dwindle down to a fraction of that great array that constitutes the dealers in medicines of all grades.

"In making these remarks, it is from a sense of the importance of the subject to the lasting interest of our profession. Most of the gentlemen who are very largely engaged in manufacturing pharmacy are friends of the writer, who is moved by not the slightest degree of personal feeling in making these remarks. They are thrown out as a caution to us all, that we who claim to have the welfare of our professional body at heart may consider deeply the bearing which such innovations are having on the development and professional health of its members, and take such measures as will turn the current of this evil into a channel where its influence may be regulated as far as possible. The first remedial measure should be to insist on the clear and distinct annunciation by manufacturers of the proportional strength of preparations, and the processes employed, especially in reference to the menstrua used. Secondly, the Association should use its best endeavors to compel the adoption of uniform strength and formulæ for all standard preparations not in the Pharmacopœia, and an adherence to the processes of that authority for those that are found in it. And thirdly, to use efficient means to get the next edition of the Pharmacopœia to include all those preparations of worth which are now known and used as unofficinal preparations."

From the Nashville Monthly Record of Medical Science we make the foregoing extract, confident that the reader will find in it "food for reflection." It is strange indeed, but not less true than strange, that while the devotees of our profession are urging it forward in many respects, in others, and important ones, they are either directly or indirectly causing it to retrograde; and in the matter of purifying our Materia Medica, we all exhibit an especial degree of apathy. One would really suppose, at the present day, that it is only necessary to diagnosticate disease—that the administration of remedies is of minor importance. Chemistry has of late years come to our assistance and so isolated the active principles of many of our most valuable agents as to make them much more certain and far more agreeable, and it would seem that this alone would have taught us that in a close adherence to scientific research alone is to be found safety to the purity of our Materia Medica. But not so; instead of the name of Apothecary becoming more and more honorable to those bearing it, because of their admitted qualifications as well educated and scientific men, it has come to mean only that a man stands behind a counter and counts out pills or drops from the wholesale establishments of the manufacturing pharmacuetists, just as the dry-goods clerk measures tape by the yard. Instead of the Apothecary being the well qualified and faithful ally of the physician, jealous of the purity of his drugs, because conscientious in dispensing them, he is fast becoming but the tool of unscrupulous wholesale vendors of worse than worthless trash, sold with the exclusive view of accumulating wealth. Is our language too strong? Then go into the drug-stores and look at the thousand and one ready made pills, drops, etc. A house in Paris makes "Compound Cathartic Pills" by the formula of the United States Pharmacopia, (or they say they do) and coat them with sugar, and all the American druggist has to do is to count them out to his customers. Instead of his having the ingredients before him, being able to judge of the purity of the same, and then compounding and dispensing them, as the Physician has a right to expect him to do, he leaves that drudgery to the fashionable manufacturing pharmacuetist, and fills prescriptions mathematically! Such a man is not an apothecary, and he is not the natural ally of the Physician; so far from it, he is one of the worst enemies we have to contend against. We have a high respect for a real apothecary, a man of scientific attainments in his line, because we know his avocation is honorable and difficult; we always regard him as a part of our professional self; but for the mere counter of pills and drops, we ——— there is no use in saying what we do think of him.

But the strangest part of the business is, that medical men themselves are so far forgetful of their own interests, as well as the honor and usefulness of their profession, as to absolutely prescribe these manufactured medicines. Strictly speaking, we do not believe that they have a *right* to do this. We have recently noted the fact of Tilden & Co.'s Compound Cathartic Pills, "*without calomel*," being prescribed in Boston, and absolutely producing alarming purgation in the case of a lady. Now, suppose this lady had died from the purgation; in a medico-legal point of view, had the physician a right to prescribe Tilden's pills? We say not. Had the pills of the U. S. P. been prescribed, there was real scientific and reliable authority for so doing; but Tilden himself is the only, and a self-constituted authority for the use of his pills; the prescriber was totally ignorant of them, either through his own experience or that of his brethren, and the death might justly be laid at his door. It seems to us if there is a special privilege which we physicians should insist on enjoying, it is that of making our own prescriptions, of having medicines compounded according to our own directions; and then if there be another privilege, it is that of frowning on every man who shall dare to invade this sacred right. As things are going, we occupy the most ridiculous position imaginable—that of waging an eternal war of words on quackery, and yet being daily guilty of arrant quackery ourselves. Would that our feeble pen could exterminate every fashionable manufacturing pharmaceutist, we would do it, and send them to quack purgatory.

QUACKERY.—It is, in spite of philosophy, a humiliating circumstance to the true and honorable Physician, to witness the extent to which arrant quackery is patronized by our people; and New Orleans is every winter the especial scene of their depredations. Here, the real Physician toils on from one year's end to another, through rain and sunshine, through yellow fever epidemics, through every vicissitude incident to the most arduous of all avocations, with too often a remuneration scarcely sufficient to defray the actual expenses of his family; while during a few winter months there comes flocking in a horde of lung doctors, pile doctors, corn doctors, biologists, phrenologists, electricians, etc., etc., etc., who fill their pockets through the miserable credulity of mankind, and as soon as our spring sun begins to shine too warmly on their fair skins, off they go to the north or west to invest their ill-gotten gains, and await the next harvest time. Instead of these harpies being frowned on by all honest men, and forced to betake themselves to some honest avocation, they are applauded for their iniquity, and honest men are let to starve. But there is no use inveighing against them; rather let us amuse ourselves over

The Latest Quack Advertisement.

Homœopathy, hydropathy, *et id homne genus*, must now hide their diminished heads. Mr. Gayetty, of New York city, has found that the public mind is prepared for anything whatever in the shape of humbuggery, and he at once, with true yankee readiness, administers to their rapacious appetites in a manner to be admired—if humbuggery is ever admirable. Inasmuch as this idea of Mr. Gayetty is strictly a fundamental one, it must be valuable, and we cannot resist the temptation to "give him a lift" in our pages. We, therefore, insert his advertisement in full, as we clip it from the National Intelligencer (!!), with one suggestion, viz: that he not only places his autograph on each sheet of his invaluable paper, to prevent counterfeit, but that he furnishes his millions of patrons with his photograph in like manner. More, we suggest that the photograph be taken with a bland smile on the face. We are really anxious to see the face of the man who is going to eclipse even homœopathy in the inestimable benefits he thus rubs into mankind; and then, again, it would be such a capital idea to be thus cheering up the sufferer by smiling on the very seat of his troubles. The cheerful countenance of Mr. Gayetty would be worth the small price of the paper. *Oh, tempora! oh, mores!*

MISERY OBVIATED.—The greatest blessing of the age is J. C. Gayetty's Medicated Paper for the Water Closet. It is endorsed by the press, the clergy, the bar, the school teacher, the merchant, the family, and the public generally. To use the language of one of the New York dailies:

It is beautiful pure Manilla hemp paper, as delicate as a bank note, and as stout as foolscap, entirely divested of the poisonous chemicals of so-called pure white paper, and of printer's ink, and is medicated so as to cure and prevent piles. The medication is perfect, acquired by a process for which the discoverer has taken out a patent.

It is harmless to healthy persons, and a luxury into the bargain. It is warranted, let us repeat, to cure and prevent piles. It is a household comfort. It will become a popular necessity. It is cheaper than all other paper, retailing at the rate of $1 per thousand sheets.

It is sold by druggists throughout the world. Depot, wholesale and retail, at No. 41 Ann street, New York city. It will be sent anywhere, by express, upon the receipt of the price.

N. B. The name of J. C. Gayetty is water marked in every genuine sheet.

Obituary.—Died, on Wednesday, the 26th instant, of pleuro-pneumonia, Bryant E. Stutts, of Clarke county, Alabama, a member of the Medical Class of the New Orleans School of Medicine.

At a meeting of the students of the New Orleans School of Medicine, the following resolutions were unanimously adopted:

Whereas, we have heard with deep regret, of the death of our fellow student, Bryant E. Stutts, of Alabama.

Be it Resolved, That we most sincerely sympathize with the family and friends of the deceased, in the heavy affliction which has fallen upon them.

Resolved, That the students wear the usual badge of mourning for thirty days.

Resolved, That a committee of three be appointed to accompany his remains to his late home.

Resolved, That the students of the Medical Department of the University of Louisiana, be invited to join us in paying the last tribute of respect to the deceased.

Resolved, That the above resolutions be published in two of the New Orleans papers, and a copy forwarded to the family of the deceased.

J. L. ALSTON, Chairman.

W. S. Mitchell, Secretary.

With the deepest sorrow we make the above announcement. The death of this fine young man, is the only shadow that has fallen across the pathway of our class this season; but it is a deep one indeed. He was a noble youth—industrious, inquisitive, yet modest and kind, he won the esteem of his teachers and fellow-students. His loss must be terrible indeed to those who have so long placed their hopes in him; but it is not for them to dispose; their share is submission. They will sigh to think that he died so far away from home, but they will also have the satisfaction of knowing that he was well cared for even amongst strangers, and that all was done for him which frail man can do for his brother. To die is the common lot of man, yet to die when thus setting the foot on the very threshold of responsible life, seems hard indeed. Yet it is not for us to ask why? *Whatever is, is right.*

Our Leading Article.—Although the very able and interesting article which appears first in the original department of this number is not from the pen of a member of the profession, still we are confident the reader will not regard it as by any means inadmissable in a medical journal. On the contrary, we believe that its perusal will be productive of good to medical men. It will show them the high estimate placed on the true Physician by men who stand highest in our communities, and it will arouse a just ambition to merit that estimation. Not that we think it an easy matter for the Physician to arrive at the high pinacle pointed to by the eminent writer, but the very effort to reach it is sure to do him good. Indeed, we believe the path to be more difficult for the medical man to travel, than for men in any other vocation whatsoever—provided always, that in the estimation of his fellow men he is really traveling it. To do right in his own opinion, is as easy for the Medical man as for any other; but to do right for other men is hard indeed. Of all men on earth, the Physician has to be *most cheerful, most forbearing, most self-denying, most charitable.* His mission each day is to the sick bed of men of all religious creeds, of all temperaments, of all intellectual capacities, of all prejudices, of all superstitions, of all colors, of all grades of life. Who envies the Physician?

REPORT OF DEATHS IN THE CITY OF NEW ORLEANS FOR NINE WEEKS, ENDING JAN. 23d, 1859.—*From Nov.* 21*st to Nov.* 28*th*, 1858—Men, 62; Women, 18; Boys, 42; Girls, 25—Adults, 80; Children, 77; Males, 104; Females, 43; Total, 147.

Diseases; Consumption, 23; Infantile Convulsions, 9; Yellow Fever 5; Still-born, 9.

From Nov. 28*th, to Dec.* 5*th*, 1858—Men, 55; Women, 26; Boys, 30; Girls, 26; Adults, 81; Children, 56; Males, 85; Females, 52; Total, 137.

Diseases—Consumption, 19; Infantile Convulsions, 5; Inflammation of Lungs, 16; Yellow Fever, 1; Still-born, 2.

From 5*th to* 12*th Dec.* 1858—Men, 39; Women, 20; Boys, 35; Girls, 31; Adults, 59; Children, 66; Males, 74; Females, 51; Total, 125.

Diseases—Consumption, 12; Infantile Convulsions, 9; Inflammation of Lungs, 9; Yellow Fever, 2; Still-born, 10.

From 12*th to* 19*th Dec.* 1858—Men, 33; Women, 21; Boys, 21; Girls, 21; Adults, 54; Children, 42; Males, 54; Females, 42; Total, 96.

Diseases—Consumption, 21; Infantile Convulsions, 1; Inflammation of Lungs, 9; Still-born, 1.

From 19*th to* 26*th Dec.*, 1858—Men, 42; Women, 25; Boys, 25; Girls, 26; Adults, 67; Children, 51; Males, 67; Females, 51; Total, 118.

Diseases—Consumption, 17; Infantile Convulsions, 3; Inflammation of Lungs, 4; Yellow Fever, 2; Still-born 8.

From 26*th Dec. to* 2*d Jan.*, 1859—Men, 37; Women, 19; Boys, 24; Girls, 18; Adults, 56; Children, 42; Males, 61; Females, 37; Total, 98.

Diseases—Consumption, 12; Infantile Convulsions, 2; Inflammation of Lungs, 4; Still-born, 5.

From Jan. 2*d, to Jan.* 9*th*, 1859—Men, 25; Women, 16; Boys, 22; Girls, 24; Adults, 41; Children, 46; Males, 47; Females, 40; Total, 87.

Diseases—Consumption, 12; Infantile Convulsions, 2; Inflammation of Lungs, 2; Still-born, 12.

From Jan. 9*th to Jan.* 16*th*, 1859—Men, 35; Women, 24; Boys, 30; Girls, 18; Adults, 59; Children, 48; Males, 65; Females, 42; Total, 107.

Diseases—Consumption, 29; Infantile Convulsions, 2; Inflammation of Lungs, 5; Still-born, 6.

From 16th to 23d Jan., 1859—Men, 30; Women, 16; Boys, 27; Girls, 29; Adults, 46; Children, 56; Males, 57; Females, 45; Total, 102.

Diseases—Consumption, 14; Infantile Convulsions, 7; Inflamation of Lungs, 12; Still-born, 5.

During the nine weeks, forty-four children died of Trismus Nascentium, or an average of nearly five weekly—the greatest number for one week being nine.

In the nine weeks there were fifty-eight still-born children, or an aver- of more than six a week.

The latest deaths by Yellow Fever (2), occurred in the week ending Dec. 26th, 1858.

APOLOGY.—We must crave the indulgence of our patrons for the late appearance of this number. Tardy contributors, bad weather, worse colds, patients very sick, streets almost impassable; all these and more constitute our reasons for the apparent negligence. We hope to do better in future.

LIST OF PAYMENTS TO JAN. 28th, 1859.—Drs. P. F. Brown, $10, vols. 5 & 6—J. T. Scott, $10, vols. 4 & 5—John Lewis, $10, vols. 4 & 5—T. A. Sheppard, $10, vols. 4 & 5—J. R. McDow, $10, vols. 5 & 6—R. B. S. Hargis, $15, vols. 3 4 & 5—Mr. T. L. White, $35, for advertis. C. F. Hamer, $5, vol. 6—Valsin Dupuy, $18, vols. 2, 3, 4, 5, and $2 on vol. 6—T. Clapp, $10, vols. 4 & 5—J. W. Tottenham, $5, vol. ending Jan. 1860—M. M. Davis, $5, vol. ending Jan. 1860—H. Woods, $5, vol. ending Jan. 1860—W. B. Booth, $10, vols. 4 & 5—T. H. L. Smith, $10, vols. 5 & 6—W. P. McMillan, $10, vols. 4 & 5—J. W. Folwell, $10, vols. 4 & 5—R. Fowler, $5, vol. 6—R. H. Day, $5, vol. ending Sept. 1859—J. G. Freeny, $5, vol. 5—A. P. Jones, $10, vols. 4 & 5—W. R. Sims, $18, vols. 2, 3, 4, & 5—A. C. Holt, $5, vol. 6—W. H. Ruffin, $5, vol. 6—R. J. Turnbull, $10, vols. 5 & 6—J. Wilson, Havana, $5, vol. 6—R. La Roche, $5, vol. 5—F. J. B. Romer, $10, vols. 5 & 6—C. H. Stowe, $5, vol. 5—M. C. Boyce, $5, vol. 5—A. L. Grayson, $15, vols. 4, 5 & 6—J. S. Brockinton, $5, vol. 5—T. K. Powell, $5, vol. 6—

Williams & Bemiss, $7,50 for bal. on vol. 5, and vol. 6—L. H. Jones, $5, vol. 5.

BOOKS AND PAMPHLETS RECEIVED.—"*The Modern Practice of Midwifery. A Course of Lectures on Obstetrics: Delivered at St. Mary's Hospital, London, by Wm. Tyler Smith, M. D., etc.*"; *edited by Augustus R. Gardner, A. M., M. D., of New York city. Robert M. De Witt, Publisher*, 1858." With the compliments of the Editor and Publisher.

When these lectures of Tyler Smith were being issued in the pages of the London Lancet, we praised them. Now, we cannot do more, nor less, than to say that we think them the best of the day. About ten years ago the same author put forth a small volume with the modest title: "Smith on Parturition," a treatise on the Principles and Practice of Obstetrics, and to the present day, in all that it discusses, it stands unrivalled. Put that little volume and the present one together, and we think they would make the best volume on obstetrics of this or any other day. We have no desire to appear censorious, but we think De Witt and Blanchard & Lea would have thus made both books much more acceptable to the profession, and the distinguished author would have fared better in the bargain.

The subject matter of the volume before us (the lectures of Smith) is unobjectionable, but the "getting up" of the book is not to be praised. At the present day, inferior paper and rough wood cuts will go far to prevent the sale of a medical work. We hope the new publishers will do better next time.

"*A Treatise on Fractures: By J. F. Malgaigne, Chirurgien de l'Hopital St. Louis, etc., etc., etc., with One Hundred and Six Illustrations; translated from the French, with notes and additions by John H. Packard. M. D. Philadelphia: J. B. Lippincot & Co.*, 1859."

The profession at large are certainly indebted to Dr. Packard, for this valuable contribution in the way of translation. Many of the most valuable medical works are hidden from the mass of the profession, because issued only in foreign languages, and the man who devotes himself to the almost herculean task of translating them is a public benefactor. Malgaigne's Treatise on Fractures requires no recommendation at our hands: it is one of the pillars of the surgical library.

The mechanical execution of the volume is good—though we think the paper might have been a little better, to do real justice to the wood cuts. The "notes and additions" of Dr. Packard are modest, and, so far as we can judge from a hasty perusal, not inappropriate—really tending to enhance the value of the volume, instead of, like some we have seen, acting as a mechanical obstacle. We are under obligations to the publishers for this book, which has been handed us by our enterprising friends, H. D. McGinnis & Co., booksellers, etc., Camp street.

"*A System of Human Anatomy, General and Special: By Erasmus Wilson, F. R. S., etc., etc. A new and improved American, from an enlarged London Edition. Edited by Wm. H. Gobrecht, M. D., Professor of Anatomy, etc., with Three Hundred and Ninety-seven Illustrations on wood. Philadelphia: Blanchard & Lea,* 1858."

This is a new edition of the *very best text-book on Anatomy ever issued.* Until some one can issue a book which is a real improvement, we think all medical students should be urged to adhere to Wilson. The profession is under obligations to Dr. Gobrecht, for the best edition ever issued in this country.

"*Diseases of the Urinary Organs: A Compendium of their Diagnosis, Pathology and Treatment: By Wm. Wallace Morland, M. D., etc., etc., with Illustrations. Philadelphia: Blanchard & Lea,* 1858."

We are not aware that there was any necessity for this work, but, so far as we are able to judge, from a necessarily cursory glance, it appears to be a good one. The respected author would hardly issue so extensive a work, on so important a subject, without being careful to have it "up to the times." The volume is of 562 pages, and the mechanical execution is of the peculiarly beautiful style of the well-known publishers.

"*Dupont's Artesian Well, Louisville, Kentucky. Report, Analysis, and Medical Properties of its Waters, with Remarks upon the nature of Artesian Wells: By Professor J. Lawrence Smith, of the Medical Department of the University of Louisville.*"

A very interesting and instructive pamphlet, for which we are under obligations to the esteemed author.

"*Annual Report to the Legislature of South Carolina, relating to* *the Registration of Births, Deaths and Marriages, for the Year ending December* 31, 1857. *Columbia,* 1858."

We are under great obligations to Dr. R. W. Gibbes, Jr., *Registrar,* for this valuable Report. A man who can give himself up to such a labor as is evinced in this pamphlet deserves the praises and patronage of his fellow men. When will we ever see such a report on so important a subject in the State of Louisiana?

NEW MEDICAL JOURNALS.—"*The New York Medical Press: A Weekly Journal of Medicine, Surgery and the Collateral Sciences. Edited by J. L. Kiernan, A. B., M. D., and W. O'Meagher, M. D.*"

This is a new weekly journal, the pages of which seem to be principally intended for the setting forth of the clinical lectures of the Professors in the different New York medical schools. So far, the manner is very good; but, with a few exceptions, we think the matter very exceptionable. What the student of medicine is to learn from the twaddle between the Professor and the patient, as it is set forth in these pages, we are at a loss to conceive. But this is no affair of ours: if the students of the South will travel all the way to New York after such *clinical* (*!*) instruction, we say—give it to them *usquè ad nauseam.* We wish the new Editors all the success they can reasonably desire, for they are evidently working hard. One thing, however: cannot one of them pay New Orleans a short visit, and learn truly that *Gotham is not the metropolis?*

"*The Saint Joseph Journal of Medicine and Surgery, a Monthly Journal devoted to the Advancement of Practical Medicine and Surgery, under the auspices and supervision of the members of the Saint Joseph Medical Society. Editorial Committee—J. H. Crane, M. D., O. B. Knode, M. D., and G. C. Catlett, M. D. St. Joseph, Mo.,* 1859."

No. 3 of this very nice little journal is before us, and we exchange with pleasure. We trust the Editors and Medical Society may succeed in their efforts to make it "grow."

"*Semi-Monthly Medical News: Edited by S. M. Bemiss, M. D., Professor of Clinical Medicine, University of Louisville, and J. W. Benson, M. D., Professor of Anatomy, University of Louisville.*"

This is another new journal just laid on our table. It is in every respect creditable to its projectors, and we trust it will meet with a liberal support. We exchange willingly.

EXCERPTA.

Cases of Exhaustion from Heat treated in the New York Hospital by Stimulants and Ice to the entire Surface of the Body. *Reported by B. Darrach, M. D., Resident Physician*—*Case* I. Daniel Murphy, æt. 29, native of Ireland, laborer, was admitted August 14, 1857; brought by the police from a drug-store, without any history of his attack. Comatose; skin burning hot, but moist; respiration 40 per minute, stertorous, labored, and sighing; pulse 160, and feeble; pupils contracted, and insensible to light; eyes congested; has great difficulty in deglutition; vomited before admission. Ordered an enema of carbonate of ammonia and brandy, ice to the head, and sinapisms to the chest, abdomen, and thighs. While under this treatment, the intense heat of skin seemed to indicate some attempt to reduce it. Ice was accordingly applied to the body, at first cautiously. In half an hour he vomited some green fluid, and had a tetanic convulsion of the muscles on the back of the neck. Pupils dilating, and show signs of sensibility. Ice applied more freely, being rubbed over the whole body and down the arms and legs. Sinapisms removed. Three hours after admission he had a violent convulsion; pupils dilated, and sensible to light; the heat of skin still kept down by ice.

Aug. 15*th.* The ice was applied at short intervals through the night. Patient, was conscious this morning; but now, 11, A.M., is semi-comatose. Pupils contracted; skin comfortable; tongue dry and brown, with sordes on the teeth; pulse 108 to 120, feeble. Treatment suspended early this morning.

16*th.* Yesterday afternoon he was able to give his name and age, but spoke so indistinctly that nothing else could be understood. Has an idiotic expression of countenance.

20*th.* Notices himself a difficulty in his articulation, though it is very much improved. Says that on the day he was taken sick he had felt bad all day; had drank four or five glasses of brandy, and freely of cold water.

28*th.* Still speaks indistinctly, but is otherwise perfectly well. Discharged.

Case II. Marie Hass, æt. 19, German emigrant just arrived, admitted

June 26, 1858. Had been running about all day. Was said to have had a slight attack of sunstroke in the morning. Ate a hearty dinner, and drank freely of beer and ice-water. Was taken suddenly sick on the Erie Railroad Pier about 3, P.M., and brought to the hospital about three hours afterwards. On admission she was insensible, with violent convulsions; skin burning hot, and pungent; pulse 135, small; pupils contracted; respiration hurried, but easy; vomiting. Ordered ice and iced brandy freely; carbonate of ammonia, grs. v, every quarter of an hour; to be rubbed from head to foot with ice constantly; ice kept to the head. 7, P.M. Pulse 116; respiration 36; skin cool while the ice is applied, but becomes hot as soon as it is stopped; convulsions less. Continue treatment. Fell asleep at 7½, P. M. Respiration easy; pulse much less frequent. Treatment suspended. Woke in a convulsion half an hour afterwards, but slept immediately after it, and was not disturbed through the night.

June 27*th.* Pulse natural. Appears and feels well. Wants to go. Discharged.

Case III. Unknown man, apparently a German baker, æt. about 22, admitted June 28, 1858. Was taken suddenly in a store in Seventeenth street, nearly two miles from the hospital. Admitted three hours and a quarter after. Insensible; pupils contracted; pulse 150, small and feeble; respiration 34, labored and sighing; skin hot and pungent; has sinapisms of his feet, wrists, and abdomen; tongue hot; unable to swallow. Gave two ounces of brandy by stomach-pump; rubbed the surface with ice from head to foot; ice to the head. Forty minutes after his face became livid, with irregular respiration, dilated pupils, then a short struggle and death.

Case IV. Mary Duffy, æt. 40, native of Ireland, married, admitted June 29. Distressed looking, ragged creature. As ascertained afterwards, she had been complaining for a week, but kept at her work as washerwoman; and to-day, though suffering a pain in the stomach, took a long walk in the sun. She drank freely of water, and once of gin. Was taken with blindness about the middle of the afternoon while in the street. When brought in by the police she was so far insensible as to be unable to answer, but repeated the question until asked another. Pulse 160; respiration 40, sighing, but not very labored; skin hot. Took brandy ℥vj in the course of two hours, and was rubbed with ice as the others, keeping the skin cool. An hour after admission she could tell her name and express her gratitude. Pulse 112; respiration 18; surface cool. Complained of pain in the head; relieved by ice. Had some spasmodic respiration for a few minutes, but afterwards became natural.

June 30th. Slept well; feels sore this morning.

July 2d. Feels well; gaining strength. Has a constant tremor of the eyes.

10*th.* Eyes well. Discharged.

Remarks.—Of all the cases—four in number—treated in the hospital during the last two summers, by stimulants, sinapisms to the extremities and body, and ice to the head, none were more grave on admission than those recorded above, and none recovered. Dr. H. S. Swift, in a paper published in the *New York Journal of Medicine*, vol. ii., 1854, gives an account of sixty cases which occurred in the hospital under his care, of which thirty-three died. He has collected the cases in the hospital for about thirty years back, and found 150, of which 78 died. None of the cases admitted under my observation were of a mild form, the first stage of Dr. Swift. Two of those which recovered had contracted pupils a uniformly fatal symptom with the doctor. In the other recovery the pupils were not noted. In *Braithwaite's Retrospect*, vol. xxxvii., page 363, is a paper from the pen of Dr. Beatson, army surgeon in India, whose treatment, as given there was affusion of cold water to the head, chest, and epigastrium, with moderate stimulation; all ligatures about the neck being removed, and the patient, if possible, carried to the shade. He was uniformly successful in eight or ten instances, in all of which the treatment appears to have been applied immediately. In my own cases the ice was rubbed over the surface until it became cool and the pulse diminished in frequency. Of those that recovered, it was continued on one of them, with little or no interruption, until she slept, a period of one hour and a half; in another an hour, when she was able to give a correct account of herself, and the pulse was reduced from 160 to 120 beats per minute; in the other it was continued at intervals for twelve hours. Any increase of the heat of skin or permanent frequency of pulse being considered an indication for the ice. All the cases of the past season were admitted after 3, P.M., and after the daily visit of the Attending Physician, Dr Joseph M. Smith, though the treatment met his subsequent approval. Case I. was treated by Dr. Thos. B. Dash, then Resident Physician, at the suggestion of the writer. New York Hospital, Sept. 17, 1858. [*American Journal of the Medical Sciences.*]

EXTRACT FROM MINUTES OF THE SOCIETY FOR THE ADVANCEMENT OF NATURAL SCIENCES OF LOUISVILLE. *Furnished by* W. N. HAILMAN, *Secretary.*—Mr. Dembitz read a paper on "Acoustics in its connection with the human voice."

He first defined sound as "certain vibrations of matter, of sufficient strength and velocity to be perceptible to the sense of hearing," stated that any substance or body, in so far as it is elastic, may produce sound by its vibrations, and may communicate it (nothing but a perfect vacuum interrupting it); that we must distinguish in sound three things; its *pitch*, its *strength*, and its *quality*.

Pitch was defined as the result of the frequency of the vibrations, increasing in height with the frequency of those vibrations or oscillations. He then described the various manners in which the numbers of oscillations producing a sound have been ascertained by actual experiment, stating that by one of these methods, a steel spring was forced to make twenty-four thousand oscillations in a second; producing an audible sound two octaves higher than the highest note of a seven octave piano; and that by a slight modification of this method, the steel spring produced a clear and intense sound of only eight oscillations a second. The method here alluded to consists in revolving an indented wheel, the teeth of which lift a flat spring affixed to a strong wooden frame. This spring being raised by each tooth of the wheel, and rebounding by its own elasticity, before the succeeding tooth can work upon it, will make an oscillation for each tooth. By varying the rapidity of the wheel's motion, Savart arrived at the results mentioned above.

The strength of the sound, which can be best tested by the distance at which it is audible, depends on the amplitude and force of the oscillations. Mr. Dembitz here compared the oscillations of the air (the condensing and rarifying of the air in alternate strata) which finally bring the sound to the ear, to the oscillations of a pendulum, proving this analogy true in several points of view. Mr. D. considers the quality of sound, practically, of much greater importance than even its pitch or its intensity, because on it alone the human language depends.

He dismissed this subject after some striking illustrations, as not belonging properly to his discourse; then enumerated the various directions of the vibrations of sounding bodies; and proceeded to give an account of the nature of the human voice.

For a long time, he said, it was believed the windpipe produced sounds on the principles of a wind instrument—the length of the vibrating column of air modifying the sound. But even a superficial reflection would have shown this belief to be unfounded; for the voluntary contraction of the windpipe is too inconsiderable, when compared with the compass of the human voice. It was then thought sound was produced in the mouth by the air which is blown into it through the glottis.

These theories were both proved faulty by Dr. Mueller, of Berlin (who died a few months ago), who by experiments made on the human larynx, directly proved that the voice had its seat in the vocal chords, whose tension modified the pitch of the sound produced; the greater the tension, the higher the pitch. J. Mueller would cut a larynx from a human subject, separating from it the muscles, which in the living individual, serve for its tension and relaxation, and by substituting strings with appended weights, would illustrate the influence of this tension upon the pitch of the voice." By increasing the tension from one-quarter of an ounce to eighteen ounces, Dr. Mueller succeeded in raising the sound produced two and a half octaves, or in other words increasing the frequency of vibrations almost six-fold.

The lower vocal chords are then the seat of the voice, and the upper vocal chords serve as a kind of sounding board, strengthening by their vibrations the sound given by the lower ligaments, whilst in the mouth the articulation and vowels are modified.

The latter part of Mr. D's. discourse was illustrated by some experiments on the larynx of an inferior animal.

Dr. Goldsmith then inquired about the construction and cost of an ozonometer.

T. E. Jenkins answered that Schoenbein's ozonometer was made by saturating filtering paper with starch, in which some iodine of potassium had been dissolved; that it indicated presence of ozone in the open air or any other location in the following manner:

Ozone, by its oxydizing influence, combines with the potassium of the iodine, liberating the iodine, the latter coming in contact with the starch, imparts to it a blue color, by the intensity of which the amount of ozone may be approximately estimated.

The *Chemist* (November, 1858, p. 119) has the following article:

Dr. Lankester exhibited an instrument for measuring the constant intensity of ozone. The instrument consisted of two small rollers, included in a box, which (rollers) were moved by means of an ordinary clock-work. Over the rollers a strip of paper prepared with iodine of potassium and starch is allowed to revolve, the paper becoming exposed to the air for an inch of its surface in the lid of the box. Twenty-four inches of paper pass over the rollers in the course of twenty-four hours, and thus the paper registers by its color the intensity of the action of ozone in the atmosphere. Dr. Lankester pointed out the importance of ascertaining the amount of ozone in the air, on account of its undoubted relation to health.

The situation where this experiment is made must be free from currents of acid gases or vapors, because acids would, with the iodine and moisture in the instrument, form hydriodic acid, which, by light, is decomposed into its elements—hydrogen and iodine. The iodine would thus, also, be at liberty to act on the starch and impart to it the characteristic blue color.

Mr. Scheffer remarked that another inconvenience might make the instrument unreliable, especially in warm weather, when the starch will easily ferment. The acids produced by this fermentation would then act in the same manner as the currents of acid gases or vapors mentioned by Mr. Jenkins.

A member inquired what was the nature of ozone?

Mr. Jenkins answered, that it was oxygen in a nascent state, produced by electricity, evaporation of volatile oils and the slow combustion of phosphorus in air or oxygen. Ozone is a powerful oxydizing agent, producing decomposition in many organic and inorganic compounds.

Messrs. Brandeis and Scheffer remarked that ozone had been used in Munich to disinfect hospitals from cholera; but with very unsatisfactory results, perhaps on account of the malignancy of the miasm. The ozone was created by the evaporation of turpentine.

Dr. Goldsmith stated that in Guy's Hospital, London, it had been used with decided success in erysipeloid diseases; that erysipelas was now assuming an epidemic character in Kentucky; he thought it, therefore, advisable to institute experiments in regard to the amount of ozone in the atmosphere—a diminution of that substance being, perhaps, the cause of the prevalence of erysipelas.

Note by Professor Hailman.—I found subsequently, in the *Journal de Chimie et de Pharmacie,* a few remarks on ozone, which I add here:

"Mr. de Babo, Professor of Physics at the University of Freilburg, in Breisgan, exhibited before the meeting (the scientific congress, at Carlsruhe), an apparatus to prepare ozone. This apparatus in which ozone is obtained by the combustion of phosphorus allows us to rid the gas from the phosphorous acid by which it is generally rendered impure. We arrive at this result by washing the gas with cromic acid. This acid not only oxydizes the phosphorous acid, but it increases the ozone, for after the washing we have more ozone than before, evidently because the oxydation of phosphorous acid is itself a cause of ozonization."

Babo succeeded in obtaining ozone in an anhydrous state, whence it follows that this gas, or at least this particular kind of ozone, cannot be confounded with the hydrogenated ozone ($H\ O^3$) discovered by Mr. Beaumert.

Bunsen and Magnus, who spoke on this occasion, think that we must admit two kinds of ozone, the one the allo-tropical oxygen, the other a hydrogenated compound. This obscure question of the nature of ozone has made a considerable step in one of the subsequent sessions, as may be seen from the following article:

Mr. Schoenbein commenced by announcing the existence of three kinds of oxygen; the one, ordinary oxygen, that which we breathe in the air; the two other kinds of oxygen are two kinds of ozone, which behave towards each other like the two kinds of electricity. Indeed, ordinary oxygen results as soon as these two kinds of ozone are brought together; and, on the other hand, we destroy the ordinary oxygen, as soon as by a certain chemical action, we deprive it of the two allo-tropical modifications composing it.

This tendency on the part of the two modifications to produce ordinary oxygen, explains certain so-called *catalytical* effects; thus the peroxide of barium and oxygenated water made acid with nitric acid, are reciprocally decomposed, producing water, oxide of barium, and ordinary oxygen. Under the same circumstances the permanganate of potassa is reduced to manganic oxide, the chromic acid becomes oxide of chromium; that is, these compounds are deprived of oxygen in the presence of an abundant source of oxygen, and in close contact with ozone, whose combusting power is able to oxydize even the least oxydizable bodies, such as nitrogen, which is transformed directly into nitric acid under the influence of ozone.

These so contradictory effects are explained by what we said above: a strongly oxygenated compound may be decomposed in the presence of a compound rich in oxygen, whenever one of these compounds contains oxygen, which may be called *positive,* and the other contains *negative* oxygen. The result of this decomposition is ordinary *neutral* oxygen. This also happens when ozone obtained from phosphorus is treated with oxygenated water; the product is only pure water and ordinary oxygen. In order, then, that ozone or nascent oxygen should act as an energetical oxydizing agent, it must not be in the presence of nascent oxygen from oxygenated water. As an acid loses its acid properties in contact with an alkali, and *vice versa,* thus also *positive* ozone loses its oxydizing properties in contact with *negative* ozone.—*Louisville Med. News.*

TREATMENT OF THE APPARENTLY DROWNED.—The following instruction for the resuscitation of the apparently drowned have been extensively circulated by the National Lifeboat Institution throughout the kingdom and our colonies. They contain the latest discoveries on this important subject, the institution having taken great pains to obtain the opinion of the medical profession generally, both at home and on the continent, on the comparative merits of this and the antiquated plan hitherto adopted in this country. Of the 300 medical men, some of them representing medical bodies, who gave the society their opinion on the subject, 254 were entirely in favor of the new method, 10 were on the side of the old plan, and 36 were undecided. It should be stated that the leading principles of the instructions are those of the eminent physiologist, the late Dr. Marshall Hall:—"1. Treat the patient instantly on the spot, in the open air, exposing the face and chest to the breeze (except in severe weather.) 2. To clear the throat, place the patient gently, face downward, with one wrist under the forehead, in which position all fluids will escape by the mouth, and the tongue itself will fall forward, leaving the entrance into the windpipe free. Assist this operation by wiping and cleansing the mouth. If there be breathing—wait and watch; if there be no breathing, or if it fail, then—to excite respiration—3. Turn the patient well and instantly, on the side; and 4. Excite the nostrils with snuff, hartshorn, volatile salts, or the throat with a feather, etc., and dash cold water on the face, previously rubbed warm. If there be no success, lose not a moment, but instantly to imitate respiration—5. Replace the patient on the face, raising and supporting the chest well on a folded coat or other article of dress. 6. Turn the body very gently on the side and a little beyond, and then briskly on the face, alternately; repeating these measures deliberately, efficiently, and perseveringly about fifteen times in the minute, or every four seconds, occasionally varying the side. (By placing the patient on the chest it is compressed by the weight of the body, and expiration takes place; when turned on the side this pressure is removed, and inspiration occurs.) 7. On each occasion that the body is replaced on the face make uniform but efficient pressure with brisk movement, on the back between and below the shoulder-blades or bones on each side, removing the pressure immediately before turning the body on the side. (The first measure increases the expiration, the second commences inspiration.) The result is respiration or natural breathing; and, if not too late, life. 8. After respiration has been restored, promote the warmth of the body by the application of hot flannels, bottles, or bladders of hot water, heated bricks, etc., to the pit of the stomach, the armpits, between the thighs, and to the soles of the feet. To induce circulation and warmth— 9. During the whole time do not cease to rub the limbs upward, with firm grasping pressure, and with energy, using kandkerchiefs, flannels, etc. (By this measure the blood is propelled along the veins toward the heart.) 10. Let the limbs be thus warmed and dried, and then clothed, the bystanders supplying the requisite garments.

"Cautions.—1. Send quickly for medical assistance and for dry clothing. 2. Avoid all rough usage and turning the body on the back. 3. Under no circumstances hold up the body by the feet; 4. Nor roll the body on casks; 5. Nor rub the body with salt or spirits; 6. Nor inject tobacco smoke or infusion of tobacco. 7. Avoid the continuous warm bath. 8. Be particularly careful to prevent persons crowding around the body.

"General Observations.—On the restoration to life a teaspoonful of warm water should be given, and then, if the power of swallowing has returned, small quantities of wine, or brandy and water warm, or coffee. The patient should be kept in bed, and a disposition to sleep encouraged. The treatment recommended should be persevered in for a considerable time, as it is an erroneous opinion that persons are irrecoverable because life does not soon make its appearance, cases having been successfully treated after persevering several hours."—*London Times.*

NARCOTIC INJECTIONS IN NEURALGIA. *Chas. Hunter, Esq.,* House Surgeon St. George's Hospital, records (Med. Times and Gaz., Oct. 16th) the following cases of Neuralgia treated by narcotic injection into the part, as proposed by Dr. A. Wood, of Edinburgh.

Case I.—J. G., aged 55 was admitted into St. George's Hospital, July 21, under Dr. Pitman, with tic douloureux. He had been constantly subject to it for four years, with but little intermission; at one time he obtained for a few weeks from seven to eight hours' sleep at night, but with that exception he used always to be in pain day and night, and seldom slept an hour without a violent paroxysm.

On admission he was suffering these repeated violent attacks of pain all over the left side of the face, which caused him day and night to keep up a cry of anguish. Various remedies to palliate the pain were attempted, but unsuccessfully till the 7th of August, when the local injection of morphia was commenced. About one grain and one-third of the acetate of morphia was injected at 8, P. M.; the man fell asleep very soon after, and continued to do so for seven hours. During the next few nights the same dose was regularly injected, and he slept either all night or for several hours.

On the 11th, he was asleep when visited, so no more morphia was injected; he, however, slept two hours; the next few nights the injection was not given; he slept either not at all, or most indifferently.

16th. A larger dose was injected into the cheek from within the mouth, he went off to sleep at once, and did not awake all night; he was also easy the whole of the next day; after this the original dose was continued, both night and morning.

20th. He sleeps a good deal; has good nights, and two or three hours' sleep in the day. The paroxysms are now so slight, that often no one except the patient can tell when they are on; no continued pain is felt, and the paroxysms are "sometimes off for half a day, often for several hours."

30th. Until to-day the morphia has been injected night and morning; but, for the present, the administration is left off on account of a considerable sized abscess which has been gradually forming the last few days, and which was opened to-day.

The part injected was the gum over a back upper tooth, as that was the most painful part, and the spot which, if touched, always brought on a paroxysm; latterly, the adjacent tissue of the cheek was injected close to the gum.

Thus, not only was sleep procured, but the patient obtained considerable ease during the day while the injection was gone on with. The constant recurrence of the attack of pain was put an end to, and, the paroxysms when they did occur were far milder; but a large abscess formed in the cheek.

Case II.—E. P., aged 18, was admitted into St. George's Hospital, July 25, under Dr. Tatum, suffering from excessive neuralgia in the right eye, which was also extensively diseased. As there were no hopes of saving the eye, and the pain was constant, the globe was removed for fear the other eye should also suffer; unfortunately it did, and ran a most rapid course—the lids becoming swollen, hard, thick and everted; the neuralgia in this eye became even worse than it had been in the other.

All kinds of remedies were tried—aconite, morphia, hyoscyamus, opium quinia, etc., all failed to give relief; chloroform was then used and frequently, but it only gave her ease and sleep for a few minutes, or at the most an hour or so.

Sept. 9th. $\frac{3}{4}$ gr. of morphia (the acetate) was injected under chloroform into the eyelid, but produced no sleep, as sickness (which had commenced in the afternoon after a dose of morphia by the stomach) continued during the night.

10th. No morphia given by the stomach, $1\frac{1}{3}$ gr. injected under chloroform into the eyelid; she went off to sleep for seven hours continuously, which she had not done for some months. She slept also once or twice the next day without chloroform.

11th. Injection repeated 10, P. M.; a part escaped; she slept four hours; had acute paroxysms between the periods of sleep.

12th. Sleep produced by the injection, and the severity of the paroxysms much diminished.

In the next few days the morphia was injected, and gave ease and sleep in proportion to the amount injected; from this time no chloroform was employed while inserting the point of the syringe in the skin.

16th. Slept four hours last night. The pain now is nothing to be compared to what it previously was, the swelling is going from the eye. In the evening nearly three grains of morphia were injected; sleep was immediately produced, and continued eight hours. The next day she was far quieter and easier, and appeared so comfortable at night that no morphia was injected.

18th. No morphia having been injected, no sleep was obtained last night, although a six-hour dose (gr. i.) was continued to be administered by the stomach.

19th. 1½ gr. injected into the eyebrow, gave sleep for several hours at night, and a little in the day; at night two grains were given by the stomach; it gave no sleep, but after an hour or so caused considerable sickness.

Oct. 4. The morphia injection is still continued, and with considerable relief to the patient.

Remarks.—In this patient, then, it appears:

1. That a very great change has been made for the better, the progress of the affection appears arrested; or, at all events, for the present kept at bay; the health of the patient is improved.

2. That the local affection appears so far improved that all the hardness, thickness, and eversion of the conjunctiva have subsided; the pain in the head is very much less, the pain in the eye is far less acute, and the attacks much less frequent, so that sleep is every now and then obtained during the day without medicine.

3. But it must be observed that this girl, like the man, has had abscess as a result of the local injection; the eyelid, the eyebrow, and the side of the eye, have all been opened for the liberation of matter.

4. It is very interesting to observe, that in this girl the injection of morphia into the cellular tissue was most effectual; but that morphia given by the stomach was of no benefit at all, but always did harm; that general irritation to the nervous system was produced; that sleep hardly ever followed, and was then probably accidental, because so seldom, but that sickness, nausea, giddiness, etc., almost always accompanied its administration by the stomach, whatever the strength of the dose happened to be.

In considering the results of the trial of the local treatment in the two cases, the advantages obtained appear to me to be—

1. That much less constitutional (nervous) irritation attends the local introduction of the narcotic than when it is given by the stomach.

2. The effect of the narcotic is more immediately produced.

3. The action of the narcotic appears more sure when injected. The exact amount taken into the circulation can be more readily seen, and the risk of contamination or alteration which it is exposed to, given by the stomach, is avoided.

4. It appears to exert more benefit on the local affection when it has to be absorbed from the part affected itself, probably from being brought more directly into contact with the nerves involved in the disease.

On the other hand, there are the disadvantages; these are chiefly—

1. The pain occasioned by the introduction of the fine canula.
2. The chance of the fluid escaping from the wound or puncture.
3. The production of local inflammation, effusion of blood, abscess.

To conclude: Are the disadvantages of such import that they ought to preclude the local employment of narcotics by injection? do the advantages preponderate over them? I think they do; and that the disadvantages are only those which, with care and experience, may either be avoided or much diminished; for instance: 1. By employing such a syringe as that used for the perchloride of iron (to inject aneurisms, etc.), with a very fine point to the nozzle, the pain is not more than that occasioned by the prick of a needle. 2. By having the injecting tube no larger than that of such fine syringes, the puncture in the integument is so small that the fluid does not escape. 3. With regard to the formation of abcess; it is only, for the most part, after repeated injections have been made in one place that such happens. One great thing then to avoid it is, to vary as much as possible the exact site to be injected, still injecting in the painful part, or to cease injecting for a time. The necessarily acid state of the solution of the morphia (for it must be strong), is certainly another disadvantage; but as irritation to the integument appears produced, as little acid as possible ought to be employed, and any excess in the solution neutralized by potash. These inconveniences being obviated as much as possible by the means pointed out, I think such advantages as the more rapid introduction of the remedy into the system, the avoidance of constitutional (especially nervous) irritation, the greater certainty of the effect, and the more concentrated effect of the remedy on the painful part ought not to hinder the local treatment of neuralgia from having a fair trial.

On the Prevention of Laceration of the Perinæum. By *Mattie.* (*Vierteljahrsschrift, f. p. Heilk.*, 1858.)—Dr. Mattei gives the following views on the means of preventing laceration of the perinæum. It is especially necessary that the head pass the vulva in a favorable direction. This can only happen when it passes with the necessary degree of flexion. While the occiput passes under the pubic arch, the face has not yet quitted the pelvic outlet; first when the upper part of the neck comes under the pubic arch, can the extension of the head (or the separation of the chin from the breast) begin. If the distension of the perinæum begins too early, the head must pass the vulva with unfavorable diameters—namely, with the great oblique, or great or straight diagonal diameters. Such a passage easily causes laceration. Hence it is the task of the physician to prevent a premature distension by the head. This he effects by placing two fingers between the labia, or in some cases between the pubic arch and occiput, so as to bring the head downward and outward, at the same time laying the other hand on the hinder part of the perinæum, upon which the face is lying and pushes this upward. This manœuvre is to be executed during the pains, which will thus protrude the head forward in the requisite arc. A very simple means of expediting the birth of the head consists in compressing firmly the distended perinæum with the whole hand. This resembles the squeezing out the kernel from a cherry. On the passage of the shoulders care must also be taken lest the two shoulders pass together.—*British and Foreign Medico-Chirurgical Review*, October, 1858.

EXAMINATION OF BLACK VOMIT.—Dr. Packard read the following:—During the past summer I have had two opportunities of making microscopical examinations of specimens of black vomit.

One of these was given to me by Dr. La Roche, immediately after its ejection. Examined under a magnifying power of 360 diameters, were seen—

1. Epithelial cells of the squamous variety.
2. Bodies resembling epithelial cells in various states of distension, some apparently filled with coloring matter of different shades, from light yellow to black. A few of these were perfectly opake; many of the rest were highly refractive.
3. Some brownish-yellow, nucleated cells, about twice the size of lymph-corpuscles.
4. Some very small reddish cells which may, perhaps, have been altered blood-corpuscles.
5. Many fine granules.
6. A few quite minute oil globules.

In one of the portions examined there were several masses of what seemed to be partly digested food.

The other specimen was taken from the stomach of a sailor who died, with all the signs of yellow fever in the Pennsylvania Hospital. The power employed was 472 diameters.

1. Epithelial cells in vast numbers, arranged in masses. Very few of them were squamous ; the immense majority were columnar, and ranged side by side.
2. A great many fat globules, of all sizes.
3. Free nuclei.

The liver of this patient was very decidedly fatty, both in its grosser characters and under the microscope.

The same degeneration was very marked in the liver of another patient in whose case a similar diagnosis had been made.

Dr. S. Henry Dickson enquired whether any of the members had met with the ruptured capillaries which are described by Blair as present in black vomit. In examination of black vomit made at Charleston none were detected.

Dr. La Roche, and several other members of the Society, stated that they had never met with them.

THE ACTUAL CAUTERY IN WHITE SWELLING.—The *Medical and Surgical Reporter* (Weekly), of October 8th, contains many interesting reports of Hospital Practice in the city of Philadelphia. In the *service* of Dr. Pancoast, at the Pennsylvania, we find recorded thirty-five operations for his term. At the conclusion of his service the Professor addressed the clinical class at much length on the very satisfactory results obtained from the use of the actual cautery in scrofulous swellings of the joints. A number of cases were exhibited on whom the operation had been performed, that were in progress of rapid recovery. He remarked that Dr. Physic (and he might have included hundeds of others of equal celebrity) formerly regarded anchylosis as a necessary result of "white swelling," even if the patient's life was not lost from the progress of the disease. Nothing could prove more satisfactory than the practice of applying the actual cautery in such cases, as had been done during the last three months in that institution. Its object is to cure the disease, and prevent anchylosis, which indication it fulfills in a most pleasing manner.

The *let alone* system, in vogue for so long a period, has at last given way, and the *surgeon* now finds that something can be done to ameliorate at least, the condition of the patient laboring under this formidable malady, even if he do not effect a radical cure; and that man must be somewhat backward in his knowledge of the surgical resources of the present day who permits the life of his patient to slowly fade out, from the progress of the malady, or makes no attempt for his relief, save, perhaps, the removal of the offending member, and with it, when that *dernier* is had recourse to, also the last hope of life.

The Professor, in his clinical remarks, makes no allusion to the late revival of this method of treatment, which is probably an inadvertancy, as we should judge from his concluding remarks. To M. Notta we are indebted for the earliest valuable suggestions of this practice, "which has been marked with such perfect satisfaction for the past three months" at the Pennsylvania Hospital in the service of Dr. Pancoast. We publishd this treatment of M. Notta in August last, (see No. VIII, page 327 *Pacific Medical and Surgical Journal,*) and it gives us pleasure at this early day to thus be able to add to the testimony of M. Notta, that of one of our own countrymen. Dr. Pancoast (a name too well known to require comment from us), that the judicious use of the actual cautery in these diseases, is an agent on which reliance for success may rest. The Professor has but added another star to his already well merited reputation, he being the first to apply *the* method in our own country.

NÆVUS.—At the Jefferson College Hospital, in the service of Dr. Gross, is the report of a case of *Nævus Materni*, on which an operation by the ligature had been performed, but which from some unaccountable cause had become untied. He again operated by passing another ligature and tying on both sides. The Dr. spoke of the different methods pursued to remove this trouble, all of which belong to the days of yore.

It is probable that Monsel's Salt has not yet reached that far-off land, or the Dr. would not have allowed himself to have been annoyed by so unforeseen an accident as that which occurred. In this city we have none of the perplexities that ordinarily arise in the treatment of the nævi by the antiquated methods of ligature, vaccination, nitric acid, tying carotids, etc.; we simply elevate the diseased part with a pair of smooth forceps, or perhaps the finger and thumb, then with a pair of scissors remove it, and apply our hœmostatic on a dossil of lint. The blood ceases to flow at once, no other dressing is applied, the parts are left to themselves, and they heal kindly, with a radical cure in *all* cases, and usually with a very trifling scar. Our operation and its dressing requires about the same time we take to describe it. We would recommend it to the attention of the surgeons of Jefferson College Hospital. It is very serviceable in many other ways.

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